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IS CONFLICT ADAPTATION AN ILLUSION?

Topic Editors
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Look closely.
The owner of this image is James Schmidt.

Conflict adaptation theory is one of the most popular theories in cognitive psychology. The theory argues that participants strategically modulate attention away from distracting stimulus features in response to conflict. Although results with proportion congruent, sequential congruency, and similar paradigms seem consistent with the conflict adaptation view, some researchers have expressed scepticism. The paradigms used in the study of conflict adaptation require the manipulation of stimulus frequencies, sequential dependencies, time-on-task regularities, and various other task regularities that introduce the potential for learning of conflict-unrelated information. This results in the unintentional confounding of measures of conflict adaptation with simpler learning and memory biases. There are also alternative accounts which propose that attentional adaptation does occur, but via different mechanisms, such as valence, expectancy, or effort. A significant (and often heated) debate remains surrounding the question of whether conflict adaptation exists independent of these alternative mechanisms of action. The aim of this Research Topic is to provide a forum for current directions in this area, considering perspectives from all sides of the debate.

Table of Contents

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Is conflict adaptation an illusion?

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Keywords: conflict adaptation, contingency learning, cognitive control, attention, timing, expectancies, proportion congruent, congruency sequence effect

Conflict adaptation theory is one of the most popular theories in cognitive psychology. The theory argues that participants strategically modulate attention away from distracting stimulus features in response to conflict. This idea was particularly popularized with the publication of the conflict monitoring model of Botvinick et al. (2001). Although the conflict adaptation view is able to explain a wide range of results with a seemingly intuitive set of mechanisms, some researchers have expressed skepticism. The paradigms used in the study of conflict adaptation typically require the manipulation of stimulus frequencies, sequential dependencies, time-on-task regularities, and various other task regularities that introduce the potential for learning of conflict-unrelated information (for a review, see Schmidt, 2013a). This raises the possibility that although the *data patterns* (e.g., reduced congruency effects following incongruent trials) might be very real, the conflict adaptation *mechanism* typically used to explain them might be an illusion.

This research topic produced 17 articles from 39 authors. The contributions span a range of tasks, broadly divided into work on the congruency sequence effect (CSE) and various versions of the proportion congruency (PC) task. Duthoo et al. (2014) provide an updated review of the CSE literature, including considerations regarding difficulties with learning confounds that will need to be overcome in future research. Braem et al. (2014) provide a review and synthesis of work on cross-task CSEs, and they highlight a potentially important role of similarity in task context. Egner (2014) provides another review wherein it is argued that "learning biases" and conflict adaptation may be two expressions of a similar learning mechanism, the latter merely more abstract than the former.

The role that feature bindings play in confounding the CSE has been a central issue since seminal papers by Mayr et al. (2003) and Hommel et al. (2004). Spapé and Hommel (2014) further this work with a paradigm in which target location boxes rotate to new positions on the screen between trials, with results seeming to indicate a dependency of CSEs on bindings between stimuli. Van Lierde et al. (2014) present masked-priming experiments that produced an irregular CSE pattern when feature repetitions were included, but a regular CSE in the error rates with feature repetitions excluded. Wendt et al. (2014) present data to suggest that controls for feature bindings may be insufficient in cross-task CSEs when there is a semantic overlap between features in the two sub-tasks.

As early as the very first observation of a CSE, the role of expectancies about a repetition vs. alternation of congruency type (i.e., congruent vs. incongruent) has been discussed (Gratton et al., 1992). Jiménez and Méndez (2014) present evidence to suggest that conscious expectancies only influence behavior when participants are explicitly probed for their expectancies. In a less traditional paradigm using alphabet verification and serial reaction tasks, Gaschler et al. (2014) present evidence for the transfer of control demands from one learning task to another.

Some key articles have illustrated the major issues with contingent regularities in PC and CSE tasks (e.g., Schmidt and Besner, 2008; Schmidt and De Houwer, 2011; Mordkoff, 2012). Hazeltine and Mordkoff (2014) observe that robust effects of contingencies fully account for item-specific PC (ISPC) effects (see also, Schmidt, 2013b). They further observe sequential modulations of both contingencies and congruency on the CSE. In contrast, Blais et al. (2014) suggest that contingency biases and "congruency switch" biases are unlikely to contribute to the CSE, though Schmidt (2014b) contests the interpretation of the data in a response paper.

A particularly interesting, howbeit controversial, development in the PC literature came with the suggestion that adaptation to conflict might occur in an item-specific (Jacoby et al., 2003) or context-specific fashion (Corballis and Gratton, 2003; Crump et al., 2006). Schmidt et al. (2014) present a non-conflict analog to the context-specific PC effect and argue that the "context-specific proportion easy" effect they observe is consistent with the notion that context-specific rhythms might explain context-specific PC effects. Atalay and Misirlisoy (2014) investigate the ISPC effect with different asynchronies (SOA) between targets and distracters. Generally consistent with a contingency learning perspective, they observe robust ISPC effects across lags, except when the distracting word came too late after the color.

Entel et al. (2014) investigate the influence of explicitly instructed contingencies on PC effects. They suggest that instructions alone might trigger proactive control, while also arguing an important role for contingencies. Hasegawa and Takahashi (2014) investigate block-wide PC effects and CSEs in a masked priming paradigm. They observed block-wide PC effects even with minimal stimulus awareness, but evidence for CSEs was limited to errors.

The topic closes with two opinion articles. Schmidt (2014a) discusses yet another potential caveat with contingency biases in cognitive control paradigms: if some stimuli are highly predictive of a response, whereas others are not, then differences in stimulus informativeness can lead to attentional capture biases. Finally, Levin and Tzelgov (2014) discuss an interesting distinction between task and informational conflict, and how this distinction might have important implications for theorizing in the cognitive control literature.

The range of perspectives presented in this research topic are as diverse as the questions assessed. Regarding the main question of interest (i.e., "Is Conflict Adaptation an Illusion?"), some authors argue that the answer is a resounding "yes," others argue that evidence for conflict adaptation is clear, and yet others fall somewhere in between. Whether or not conflict adaptation is merely an illusion is still an open question, but the contributions of the current research topic add interesting new layers to the debate. We hope that this research topic will open new avenues for research in the area that may lead to more definitive answers.

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The heterogeneous world of congruency sequence effects: an update

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Congruency sequence effects (CSEs) refer to the observation that congruency effects in conflict tasks are typically smaller following incongruent compared to following congruent trials. This measure has long been thought to provide a unique window into top-down attentional adjustments and their underlying brain mechanisms. According to the renowned conflict monitoring theory, CSEs reflect enhanced selective attention following conflict detection. Still, alternative accounts suggested that bottom-up associative learning suffices to explain the pattern of reaction times and error rates. A couple of years ago, a review by Egner (2007) pitted these two rivalry accounts against each other, concluding that both *conflict adaptation* and *feature integration* contribute to the CSE. Since then, a wealth of studies has further debated this issue, and two additional accounts have been proposed, offering intriguing alternative explanations. *Contingency learning* accounts put forward that predictive relationships between stimuli and responses drive the CSE, whereas the *repetition expectancy* hypothesis suggests that top-down, expectancy-driven control adjustments affect the CSE. In the present paper, we build further on the previous review (Egner, 2007) by summarizing and integrating recent behavioral and neurophysiological studies on the CSE. In doing so, we evaluate the relative contribution and theoretical value of the different attentional and memory-based accounts. Moreover, we review how all of these influences can be experimentally isolated, and discuss designs and procedures that can critically judge between them.

Keywords: cognitive control, congruency sequence effect, contingency learning, feature integration, conflict adaptation, repetition expectancy

INTRODUCTION

Over the last decades, the study of cognitive control – the flexible and adaptive regulation of our behavior – has increasingly drawn the attention of psychologists and neuroscientists alike. One critical aspect of this ability concerns the continuous monitoring of our behavior for situations in which missteps become more likely, allowing us to adjust behavior and prevent (further) deviation from goal-directed performance (i.e., conflict adaptation). The seminal *congruency sequence effect* (CSE) is considered a hallmark phenomenon of such cognitive control (Botvinick et al., 2001; see also Verguts and Notebaert, 2008, 2009). However, despite its central position in this research domain, the interpretation of the CSE is far from unequivocal, and alternative accounts highlighted the role of episodic memory (Hommel et al., 2004; Schmidt, 2013) or subjects' explicit expectations (Gratton et al., 1992). Given the wealth of behavioral and neuroscientific studies relying on the CSE to further our insight in cognitive control, both in basic research and in more applied and clinical contexts, it seems of cardinal importance to recognize and dissociate these alternative views. Here, we give an overview of the studies that tested these accounts before we provide guidelines for further research.

The studies reviewed in the present paper investigated the CSE in typical conflict tasks such as the Stroop (Stroop, 1935), Eriksen flanker (Eriksen and Eriksen, 1974), and Simon (Simon, 1969) task. In these tasks, participants are asked to respond to a relevant stimulus feature (e.g., color), and the congruency between an irrelevant stimulus feature and either this relevant stimulus feature or the response is varied. The extent to which the irrelevant dimension is able to capture attention and influence performance is reflected in the size of the *congruency effect* – the difference between incongruent (I) and congruent (C) trials. This difference is typically strongly reduced when the previous trial was incongruent compared to when it was congruent – the CSE. In this review, we first elaborate on the standard interpretation of this CSE in terms of conflict adaptation and its underlying neural signature. Building further on a previous review by Egner (2007), we then set out to evaluate three alternative hypotheses for the conflict-monitoring theory: feature integration, contingency learning, and repetition expectancy. For each of these accounts, we highlight behavioral and neurophysiological evidence and discuss experimental procedures that can critically isolate their influence on the CSE. In the final section, we summarize the relative contribution of conflict adaptation, feature integration, contingency learning, and expectancy, and put forward some outstanding questions for further research.

CONFLICT ADAPTATION

The CSE has been a major inspiration to the conflict-monitoring theory of Botvinick et al. (2001), which boosted and dominated research in the field of cognitive control over the last decade. Within this framework, it is assumed that fluctuations in the size of the congruency effect provide a direct window into online adjustments in cognitive control. The theory posits that the information processing stream is continuously monitored for the occurrence of conflict. Contingent upon the detection of conflict by the monitoring system, control is up-regulated. Following low conflict on congruent trials, control is temporarily down-regulated, and stronger interference effects on subsequent trials are predicted.

The CSE has proven to be a very robust and generalizable effect. Following its initial report in the context of an Eriksen flanker task (Gratton et al., 1992), it was replicated in a wide variety of tasks, including the color–word (e.g., Kerns et al., 2004), numerical (e.g., Cohen Kadosh et al., 2011), and gender face-word Stroop (e.g., Egner et al., 2008), the social (e.g., Kunde et al., 2012) and spatial Simon (e.g., Stürmer et al., 2002), the parity judgment (e.g., the spatial–numerical association of response codes or SNARC effect; Pfister et al., 2013), the picture–word interference (e.g., Duthoo et al., in revision), the perceptual fluency (e.g., Dreisbach and Fischer, 2011), the prime-target (e.g., Kunde and Wühr, 2006), and affective priming task (e.g., Frings and Wentura, 2008). Also in studies on arithmetics, difficulty arising from inappropriate strategy execution is susceptible to sequential, trial-to-trial performance adjustments (e.g., Uittenhove and Lemaire, 2012; Lemaire and Hinault, 2013). Notwithstanding the diversity of these experimental paradigms, the sequential effects are typically interpreted in terms of increased cognitive control following the detection of conflict.

The conflict-monitoring theory's broad appeal can partly be attributed to the clear predictions it makes concerning the underlying brain mechanism involved in different cognitive control operations. According to Botvinick et al. (2001), the anterior cingulate cortex (ACC) is specifically involved in the detection of conflict (Jones et al., 2002), whereas subsequent control adjustments are implemented by the dorsolateral prefrontal cortex (DLPFC; Egner and Hirsch, 2005a). The CSE lends itself well to tease apart these brain regions, by comparing the neural response to incongruent trials dependent on whether the preceding trial was congruent or incongruent: the former trial transitions are supposed to evoke a strong conflict detection signal, whereas the latter trial transitions are associated with strong conflict resolution. fMRI investigations of the CSE in both the Stroop (Kerns et al., 2004) and Simon task (Kerns, 2006) convincingly showed that conflict-evoked ACC activity predicted subsequent behavioral adaptations, which were, in turn, accompanied by stronger DLPFC activity. In a follow-up study, Egner and Hirsch (2005b) elegantly showed that these behavioral adjustments are presumably brought about through cortical amplification of task-relevant information.

Other neurophysiological studies generally confirm the predictions of the conflict-monitoring theory. A series of EEG studies has uncovered deflections in event-related potentials that readily map onto the behavioral pattern of the CSE (for a recent review, see Larson et al., 2014). In a flanker task, sequential modulations of the ACC-mediated N2 component have been shown to covary with conflict adaptation effects in reaction times and error rates (Clayson and Larson, 2011, 2012; Forster et al., 2011; Larson et al., 2012). Similarly, the conflict slow potential elicited by incongruent Stroop trials is strongly reduced if the previous trial was incongruent compared to when it was congruent (Larson et al., 2009; Donohue et al., 2012). In the Simon task, Stürmer et al. (2002) showed smaller lateralized readiness potentials (LRPs) over the motor cortex following incongruent trials, indicating a reduced impact of the irrelevant dimension on response execution. In a follow-up repetitive transcranial magnetic stimulation (rTMS) study, Stürmer et al. (2007) demonstrated that the CSE was effectively abolished following TMS stimulation over the left DLPFC. Finally, Sheth et al. (2012) combined fMRI and human single neuron recording to show that modulation of the dorsal ACC by the previous trial predicts behavioral adaptation (i.e., a CSE). Moreover, these conflict adjustments were completely abolished following surgically targeted ablation of the dACC.

FEATURE INTEGRATION

Despite being the dominant interpretation of the CSE, the conflict monitoring hypothesis has been challenged by alternative accounts in terms of episodic memory effects deriving from stimulus-response events, excluding a role for higher-level cognitive control processes. In essence, the feature integration account argues that the pattern of sequential modulation is problematically confounded with low-level repetition effects. Mayr et al. (2003), for example, pointed out that in a standard two-value arrow flanker task, exact stimulus repetitions will evoke priming effects that mimic the CSE. When they excluded these stimulus repetitions from the analyses, the CSE vanished. Hommel et al. (2004) took this idea one step further, by showing that not only complete, but also partial stimulus feature repetitions influence performance, again mimicking a CSE. Briefly, the feature integration account assumes that stimulus and response features of a current trial will be temporarily bound together into a common episodic memory representation. Activation of any of these features on the next trial will automatically co-activate the remaining features. Therefore, complete stimulus repetitions and complete stimulus alternations evoke faster responses, since no previous feature binding has to be undone. Critically, in a typical Simon or flanker task, comprising of four unique stimuli, sequential congruency, and feature integration are perfectly confounded: CC and II trial transitions entail complete stimulus repetitions or alternations, whereas CI and IC trial transitions always consist of partial stimulus repetitions.

In the wake of the feature integration account, extensive research efforts were dedicated to unraveling the relative contribution of higher-level attentional control and lower-level episodic memory effects to the CSE. A widely applied approach was to simply expand the stimulus set of a given congruency task and restrict the analysis to a subset of trials in which feature overlap was absent or kept equal. Still, studies that followed this logic drew some remarkably inconsistent conclusions. Even though the CSE was found to be completely abolished following *post hoc* exclusion of feature repetitions in some studies (Chen and Melara, 2009; Experiment 1 of Mayr et al., 2003; Nieuwenhuis et al., 2006; Fernandez-Duque and Knight, 2008), other studies reported a remaining CSE for transitions with equal feature overlap (Wühr, 2005) or devoid of any feature overlap (Kerns et al., 2004; Ullsperger et al., 2005; Kunde and Wühr, 2006). Notebaert and Verguts (2006), Akçay and Hazeltine (2007, 2011) and Bugg (2008) further removed negative priming trial transitions from the analysis (e.g., sequences where the irrelevant, to-be-ignored stimulus information of the previous trial becomes the relevant stimulus information on the next), and confirmed a contribution of attentional control to the CSE. Still, this experimental strategy is somewhat problematic: by excluding more and more trial transitions, the decision on the presence or absence of a CSE is made on an increasingly small and thus special subset of the data. In an attempt to circumvent this problem, Notebaert and Verguts (2007) proposed a multiple regression approach to statistically separate the influence of bottom-up feature repetitions and top-down control (see also Braem et al., 2012; Kunde et al., 2012). Another solution is to preclude trial transitions that are contaminated with feature integration *a priori*. Duthoo and Notebaert (2012) devised such eight-color vocal Stroop task devoid of any feature overlap and still found evidence for the CSE. Puccioni and Vallesi (2012; Experiment 1) ran a similar manual four-choice Stroop task. Again, a remaining CSE was found, yet only in the accuracy data.

However, both accounts are not mutually exclusive: The fact that a CSE is still found in the absence of feature repetitions does not imply that the feature integration account should be discarded (cf., Egner, 2007). Notebaert et al. (2006) elegantly demonstrated the additive contribution of both sources in a three-color manual Stroop task. By varying the response-to-stimulus interval (RSI), these authors were also able to show that bottom-up priming effects are evident at very short RSIs (i.e., 50 ms), whereas top-down, conflict-induced processes required more time to influence behavior (i.e., 200 ms). Given these insights, one can, however, wonder whether the neurophysiological evidence reviewed above is able to separate both contributions. Even though none of these studies were set up to test the predictions of the feature integration account, they did control for such effects in the analyses. Yet, as discussed by Egner (2007), predictions of the feature integration and conflict adaptation account crucially differ with respect to II sequences: whereas feature integration would predict these transitions to be associated with facilitation, being complete repetitions or alternations, conflict adaptation links these transitions with enhanced conflict resolution and controlled processing. The strong DLPFC activation in response to such transitions clearly favors the conflict adaptation hypothesis. Moreover, feature integration has no straightforward explanation as to why the CSE completely vanishes following TMS over the DLPFC (Stürmer et al., 2007) or surgical removal of the dACC (Sheth et al., 2012). As such, the feature integration account does not easily accommodate the interactions between ACC and DLPFC that lie at the core of the conflict-monitoring theory.

CONTINGENCY LEARNING

Even though controlling for feature integration effects (be it *post hoc* or *a priori*) has become common practice in experiments on the CSE, this design choice actually comes at a price. Since most researchers decide to expand the stimulus set of their conflict tasks while at the same time maintaining a 50% congruent/incongruent ratio, they artificially increase the amount of congruent trials that would result from a random feature selection. Congruent trials would indeed occur less often, if stimulus features are selected randomly (e.g., 25% in a four-choice congruency task). As Mordkoff (2012) has argued, increasing the proportion of congruent trials forces irrelevant stimulus dimensions to become informative. In a Stroop task, for example, each (irrelevant) color word would then be more often paired with its congruent color than with any of the other colors. This association between a stimulus dimension and response is termed a contingency. Over time, such contingencies will render the stimulus dimension increasingly predictive of the correct response. Increasing the amount of congruent trials in a Stroop task would, for example, strengthen the association between the word "RED" and the corresponding response "red." It has already been shown that participants are able to pick up and exploit such contingencies (see e.g., Dishon-Berkovits and Algom, 2000; Melara and Algom, 2003). This idea was elaborated upon in the work of Schmidt and Besner (2008) and Schmidt (2013), who claimed that contingency biases can artificially elevate the size of the CSE. More specifically, Schmidt et al. (2007) showed that high-contingency trials (i.e., predictive of the correct response) are responded to more rapidly and accurately than low-contingency trials, and that the difference between the two (i.e., the contingency effect) is larger following high-contingency trials compared to following low-contingency trials. In contingency-biased congruency tasks, congruent trials are high-contingency, so that the congruency sequence effect is perfectly confounded with the contingency sequence effect.

To illustrate the impact of these confounding contingency biases, Mordkoff (2012) compared performance on a contingency-unbiased (i.e., 25% congruent trials) and a contingency-biased (i.e., 50% congruent trials) four-choice Simon task. After removing all trial transitions involving feature repetitions, only the contingency-biased Simon task revealed a clear pattern of sequential modulation. Strikingly, there was no sign of a CSE in the contingency-unbiased task. In similar vein, Schmidt and De Houwer (2011) observed no remaining CSE in a Stroop task where all contingencies were kept equal. These observations led Schmidt (2013) to claim that conflict adaptation may simply be an illusion, and that the brain-behavior correlations that have been interpreted in support of the conflict-monitoring theory actually reflect the memory biases that alternative theories have put forward. ACC activity, for example, might then reflect contingency learning rather than conflict detection. Alternatively, Grinband et al. (2011) argued that the ACC is sensitive to time-on-task, irrespective of conflict. As such, every effect present in RTs (including the CSE) will correlate with ACC activity (cf., Schmidt, 2013). Even though there is no simple way of judging between these competing views on the basis of existing neurophysiological evidence, the lack of a behavioral effect in contingency-unbiased tasks poses a considerable challenge for a conflict adaptation account of the CSE.

As a critical test for conflict adaptation, Duthoo et al. (in revision) constructed versions of three common conflict tasks that controlled for both feature integration and contingency confounds *a priori*. To this end, a vocal six-color Stroop task was designed in which color and word never repeated across two consecutive trials and each word was equally often paired with its congruent color as with one of the five remaining incongruent colors. In this way, color–word contingencies were equated between congruent and incongruent trials, while the ratio of congruent/incongruent trials was kept at 50%. In similar vein, a six-letter manual flanker task was constructed. Finally, as to further minimize the contribution of memory biases, a picture–word interference task with 120 unique congruent and incongruent picture–word combinations was administered. Interestingly, a robust CSE was found in all three paradigms, notwithstanding the differences in response modality and conflict type. Moreover, this result was recently replicated in a similarly optimized four-choice flanker task (Hengstler et al., 2014). In order to account for the discrepancy with the findings of Schmidt and De Houwer (2011) and Mordkoff (2012), Duthoo et al. (in revision) point out that the introduction of a proportion congruent manipulation in contingency-unbiased designs (i.e., 75% incongruent trials) might have induced a sustained control state that potentially obscured the more transient control adjustments reflected in the CSE. Second, they argued that precluding memory biases by design might be crucial to observe "pure" cognitive control effects. This relates to the idea that conflict adaptation might be seen as a "last resort" which participants fall back on when simply relying on the environment (e.g., stimulus-response associations) is insufficient (Bugg, 2014).

Corroborating evidence comes from other recent studies showing significant sequential modulation in the absence of both stimulus/response repetitions and contingency learning. Kim and Cho (2014) let participants alternate between two color-flanker tasks. In one task, participants responded to vertically aligned red or yellow circles with two fingers, whereas in the other task they responded to horizontally aligned blue or green circles with two different fingers. As such, each trial transition never involved a response or stimulus repetition, and random selection of stimulus features produced a 50% congruent/incongruent ratio. When both response sets were assigned to one single hand (leading participants to process the two tasks as a single response mode), a significant CSE was obtained. In a similar vein, Schmidt and Weissman (2014) created a prime-target paradigm in which horizontally aligned stimuli ("<" or ">") alternated with vertically aligned stimuli ("^" or "v"). To rule out contingency confounds, four unique incongruent stimulus-distractor pairings were selected (i.e., "< >," "> <," "^ v," and "v ^"). Even though the task comprised of different stimulus sets (horizontal vs. vertical) and response sets (left vs. right hand), Schmidt and Weissman (2014) found sequential modulation. Moreover, they replicated these findings in an identical task in which arrows were replaced with words (e.g., "up" or "left"). Weissman et al. (2014) found very similar results in an online replication study, as well as in an analogous contingency-unbiased Simon and temporal flanker task.

Finally, Freitas and Clark (2014) similarly restricted their analysis to transitions involving a shift in the vertical/horizontal dimension of their newly designed "Stroop-trajectory" paradigm, thereby excluding stimulus and response repetitions without introducing contingency confounds. On each trial, a series of identical, slightly overlapping pointing black triangles were presented one at the time in fast succession. Lastly, a smaller gray triangle pointing in the same direction was presented at either the top or bottom of the vertically aligned arrays, or at the left or right of the horizontally aligned arrays. Participants were asked to indicate the location of the smaller gray triangle, which either matched (congruent trials) or mismatched (incongruent trials) the direction in which the triangles were pointing. According to the authors, the gradual trial build-up in the task discouraged both negative priming and feature integration effects. Again, the authors reported strong sequential modulation. Taken together, the designs of Duthoo et al. (in revision), Freitas and Clark (2014), Kim and Cho (2014), Schmidt and Weissman (2014) and Weissman et al. (2014) suggest that a robust CSE can still be found, even when all known memory and learning confounds have been controlled for.

REPETITION EXPECTANCY

In his review, Egner (2007) pointed out that the role of participants' expectations has remained a strikingly underexplored factor potentially contributing to the CSE. Interestingly, in the original description of the CSE, Gratton et al. (1992) explained their findings in terms of strategic attentional adjustments driven by participants' subjective expectations regarding the nature of the upcoming trial. Their *repetition expectancy* account assumed that participants were biased to expect repeating stimulus conditions over successive trials, regardless of the objective probability of these conditions to occur (Remington, 1969). Such repetition bias leads participants to expect that the trial following a congruent trial will be congruent, and the trial following an incongruent trial will be incongruent. Gratton et al. (1992) further theorized that such (passive) expectancies fed into (pro)active preparations that are not different from a situation in which congruency is explicitly cued: in anticipation of an incongruent trial, participants would focus their attention to the relevant dimension, whereas they would loosen their control settings in expectancy of a congruent trial. Such attentional filtering leads to fast responses to CC and II trial sequences, as expectancies are confirmed, but slow responses to CI and IC trials, since preparation misfires. The repetition expectancy account therefore predicts improved performance on congruency level repetitions, and impaired performance on congruency level alternations (i.e., a CSE). In contrast to the *reactive*, conflict-driven, more or less automatically induced control adjustments proposed by the conflict-monitoring theory, the repetition expectancy stresses the role of *proactive*, anticipatory, voluntary control processes.

Even though this theory quickly faded to the background of the theoretical discussion following the publication of the influential conflict-monitoring theory (Botvinick et al., 2001), a couple of studies has recently attempted to experimentally isolate the relative contribution of expectancy-induced controlled processes in a Stroop task, which yielded seemingly inconsistent findings. Duthoo and Notebaert (2012), for example, created experimental conditions that either favored or discouraged repetition expectancies (by raising the amount of congruency level repetitions or alternations, respectively) and looked for a transfer of these induced expectancies to a test phase in which congruency level repetitions and alternations were equally likely. The lack of a transfer effect suggested that participants failed to exploit the global transitional probabilities and prepare accordingly. Even in a context where congruency level alternations were highly probable, performance benefitted from scarce repetitions of congruent and incongruent trials. Jiménez and Méndez (2013) manipulated transitional probabilities in a similar way and also found that performance was not strongly affected by expectancies. As they also measured participants' expectancies in separate blocks, the authors were able to show that even though expectancies aligned with the transitional manipulation, Stroop performance revealed a reaction time pattern in the opposite direction. Duthoo et al. (2013a), however, set out to test the prediction of the repetition expectancy account in a more direct fashion, by explicitly asking participants whether they expected an easy (congruent) or difficult (incongruent) trial before they responded to the Stroop stimulus. Over four experiments, results confirmed that participants displayed a repetition bias, expecting congruency level repetitions above chance level. Moreover, only when they predicted a congruency level repetition, a robust CSE was found. They concluded that expectancy can exert an influence on control above and beyond conflict-induced adjustments, yet only when these expectancies are explicitly manipulated or registered.

Another way to tease apart the relative contributions of reactive and proactive influences to the CSE is to examine their time course. Reactive, conflict-induced influences are assumed to be short-lived, transient and thus subject to decay over time, whereas proactive, anticipatory effects need some time for expectancies to build up and are therefore theorized to grow stronger or at least persist over time. By systematically varying the size of the RSI (between 500 and 5000 ms) and inter-stimulus-interval (ISI; between 500 and 7000 ms), Egner et al. (2010) demonstrated that CSEs are observed with small intervals (from 500 ms up to 2000 ms for RSI, and up to 3000 ms for ISI), yet completely disappear at the longer intervals. According to the authors, an interpretation in terms of conflict adaptation processes with a fairly steep decay function best fitted the data. Based on these data, van den Wildenberg et al. (2012) emphasized that adaptive cognitive control is inherently transient in nature. In a recent study, Duthoo et al. (2013b) replicated the reduced CSE with increasing intervals. However, they reasoned that expectancy-induced, proactive control is more likely to affect the CSE in situations that promote such control mode more strongly. To this end, they applied an RSI proportion manipulation that increased the probability of the stimulus appearing at the longer RSI. Under these conditions, they observed a reliable CSE for both short and long intervals, suggesting that proactive control can prevent the CSE from decaying rapidly.

Finally, a series of studies have manipulated expectancies more directly, by investigating the impact of explicit cues on the CSE. In their original paper reporting on the CSE, Gratton et al. (1992) already showed that a CSE-like pattern also emerged when applying probabilistic cues, and suggested that the previous trial's congruency triggers a similar expectancy-driven attentional filtering mechanism as an explicit cue. This was later picked up by Aarts and Roelofs (2011) in an fMRI setting. They applied a similar probabilistic cueing procedure to a Stroop-like task to point out that anticipating upcoming conflict (or lack of conflict) can trigger similar sequential adjustments as experienced conflict (or lack thereof) on the previous trial. Interestingly, they not only replicated the CSE behaviorally, but also showed a similar sequential modulation of ACC activity that has been reported in previous fMRI studies on conflict-induced adjustments (Kerns et al., 2004; Kerns, 2006). The authors concluded that the ACC was involved in strategic allocation of cognitive control. An EEG study by Correa et al. (2009) also found that the ACC-mediated N2 deflection was reduced following cues that signaled high conflict. They theorized that anticipating conflict can speed up conflict detection and conflict resolution. Taken together, the neurophysiological data thus seem suggestive of a certain degree of neural overlap between the control networks triggered by reactive and proactive signals. However, such proactive control adjustments will be limited to these situations where expectancies are induced sufficiently strong or explicitly cued, suggesting that repetition expectancy cannot be the default interpretation of the CSE.

CONCLUSION, GUIDELINES, AND OUTLOOK

Since its first report by Gratton et al. (1992), the CSE has boosted an extensive body of research that aimed to uncover the underlying mechanisms of sequential modulation, in order to better understand how people flexibly adapt their behavior. Based on the present literature review, some general conclusions can be drawn. First and foremost, consensus can be reached that both top-down, attentional adjustments and bottom-up, associative learning contribute to the (size of) the CSE. Moreover, their contributions seem to be largely dependent on the paradigm used to assess the CSE. In two-value congruency tasks, the relative share of feature integration will be substantial, if not complete (Mayr et al., 2003; Hommel et al., 2004; Bugg, 2008). When administering congruency tasks with more than two stimulus values, maintaining a 50% congruent/incongruent ratio introduces contingencies that will exert a strong influence on sequential effects (Schmidt and De Houwer, 2011; Mordkoff, 2012; Schmidt, 2013). Still, a series of recent studies (Duthoo et al., in revision; Freitas and Clark, 2014; Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014) has convincingly shown that in the absence of feature repetition and contingency learning confounds, a CSE can still emerge.

These studies allow distilling a set of guidelines on how to assess such relatively "pure" CSEs. First, the standard two-value congruency task has to be expanded to a four- (or more) value congruency task. Second, all transitions involving feature repetitions should preferably be excluded by design, rather than excluded *post hoc* or controlled for in the statistical analyses. Third, a 50% congruent/incongruent ratio should be installed while keeping all contingencies equal. One way of accomplishing this is to (a) create a unique set of incongruent stimuli (e.g., "RED" in green in a Stroop task, or "HHSHH" in a flanker task), so that irrelevant stimulus information is equally predictive of a congruent and incongruent response, and (b) constrain random selection of stimuli to avoid feature overlap (Duthoo et al., in revision; Hengstler et al., 2014). The major advantage here is that the classical conflict task remains intrinsically the same. Alternatively, four-value congruency tasks can be split up into a pair of two-value congruency tasks with separate stimulus-response mappings that alternate on a trial-by-trial basis (Schmidt and Weissman, 2014; Weissman et al., 2014). As such, stimulus features never repeat over successive intervals, and contingencies are kept equal across all trials. In a four-color Stroop task, for example, stimuli can be divided into two color pairs (and thus two sets of congruent and incongruent trials) that are presented in alternating fashion (e.g., Jiménez and Méndez, 2013; Weissman et al., 2014). Kim and Cho (2014) applied a similar strategy to a color-flanker task, but besides color and response they also varied the stimulus dimension (e.g., vertical vs. horizontal) on alternating trials. As a caveat, this manipulation only produced reliable CSEs when participants responded to all trials with one hand, and not when separate hands were used for horizontal and vertical trials. According to the authors, the latter led participants to no longer perceive the two tasks as involving a common "response mode." It has indeed been well documented that increasing the difference between two tasks might hamper a transfer of control settings (i.e., a CSE) across tasks (for a review, see Braem et al., under revision).

Second, given that the evidence to date is indicative of a contribution of both attentional adjustments and episodic memory effects, the key theoretical question no longer pertains to which mechanism accounts for the CSE, but rather how these mechanisms interact and work together in producing adaptive behavior. Even if a CSE is still found in the absence of memory confounds, this does not at all imply that feature integration and/or contingency learning have no share in bringing about the CSE in designs that do not (fully) control for these. One promising avenue for further research therefore lies in parametrically manipulating these influences within the same experiment to systematically explore their contribution to the CSE. It could, for example, well be that inserting contingencies in the design precludes the need to engage in attentional control adjustments, as picking up and adapting to these regularities would be advantageous and less metabolically costly. In this light, Bugg (2014) has suggested that attentional control adjustments would constitute a "last resort" that participants will cling on when simpler learning mechanisms fail to produce satisfactory outcomes. To shed more light on this issue, one could also investigate whether participants will still pick up and rely on associative learning after they performed a congruency task in which such influences were controlled for, or vice versa (see Abrahamse et al., 2013, for an example of order effects on cognitive control strategies). The paradigms that were described above might serve as an excellent tool for such research endeavor.

Third, in almost all of the studies reviewed above, conflict-induced cognitive control has been the default explanation in situations where the CSE was found and confounding memory effects were controlled for. Rather than confirming the predictions of the conflict-monitoring theory, such findings mainly show that memory confounds cannot be the whole story. Therefore, the field is in need of observations that positively confirm specific predictions of the conflict-monitoring theory, and may at the same time benefit from a systematic exploration of other cognitive control processes that could (additionally) drive the CSE. In the present paper, we have evaluated one such additional source: repetition expectancy (Gratton et al., 1992). The studies reviewed above suggest that strategic, expectancy-based control adjustments only contribute to the CSE when they are induced sufficiently strong, and therefore cannot be the default explanation for the effect. Manipulating the proportion of congruency level transitions appeared too subtle (Duthoo and Notebaert, 2012; Jiménez and Méndez, 2013), whereas applying an RSI proportion manipulation (Duthoo et al., 2013a) and inserting probabilistic cues (Gratton et al., 1992; Aarts and Roelofs, 2011) or self-generated congruency predictions (Duthoo et al., 2013b) proved successful in eliciting strategic control adjustments. Such experimental manipulations provide an excellent research tool to investigate how proactive, expectancy-based control processes interact with reactive, conflict-induced control processes. A second additional source to the CSE that has recently gathered researchers' attention is motivational in nature. More specifically, Botvinick (2007) hypothesized that the experience of cognitive conflict, or cognitive effort more generally, is inherently aversive, and that this negative value could modulate or even drive adaptations to conflict. A number of studies has indeed confirmed the first assumption, namely that that conflict is perceived as a negative event (Dreisbach and Fischer, 2012a; Lynn et al., 2012; Schouppe et al., 2012; Fritz and Dreisbach, 2013). However, whether it is this aversive nature (van Steenbergen et al., 2009), or rather the (conflict) resolution thereof (Braem et al., 2012; Schouppe et al., in press) that motivates adaptations to conflict, remains an open question (Dreisbach and Fischer, 2012b). There is substantial room for further research to investigate which of these components contribute to the CSE, and how they interact.

Fourth, once conflict adaptation has been clearly identified and demarcated, this would (re)activate some crucial challenges for further research. For example, it should be explored which precise mechanism(s) underlie such adaptation. These strategic adjustments could entail the altering of perceptual attention to target and/or distractor information (e.g., Egner and Hirsch, 2005b; Polk et al., 2008), the facilitation or inhibition of responses to target and/or distractor information (Ridderinkhof, 2002), or the general strengthening of active associations (i.e., Hebbian learning; Verguts and Notebaert, 2008, 2009). Furthermore, such strategic adjustments might also differ across (and depend on) specific tasks used to assess the CSE (Egner, 2008), and should ideally be thoroughly explored in unbiased designs (e.g., Weissman et al., 2014). Another challenge would be to investigate how domain-specific the mechanism(s) underlying conflict adaptation is/are. Indeed, earlier studies already explored this potential for transfer across different tasks, contexts and/or conflict types, spawning an interesting, yet seemingly inconsistent set of results (for reviews, see Braem et al., under revision; Egner, 2008). Although most studies appear to demonstrate domain-specific CSEs, others claim that adaptations to conflict can be domain-general. Therefore, it remains to be investigated which general principle(s) this transfer adheres to (Braem et al., under revision). To further test the generalizability of the conflict adaptation mechanisms, it should also be explored if adaptation by *recent* conflict (i.e., the CSE) relies on a similar mechanism as adaptation to *frequent* conflict, or not.

The latter is often studied in proportion congruency tasks, where specific proportions of (in)congruent trials modulate the congruency effect – and thus presumably the amount of cognitive control. Interestingly, the literature on this proportion congruency effect sparked similar debates as for the CSE reviewed here. For example, it has been discussed if the proportion congruency effect reflects conflict adaptation or merely S-R contingency learning processes (Abrahamse et al., 2013; Schmidt, 2013; see Bugg and Crump, 2012 for a review). Similar to the CSE, the empirical evidence seems to suggest that contingency learning cannot account for all observations – and thus that conflict adaptation seems to be involved. However, it is still strongly debated if the CSE and the proportion congruency effect involve the same underlying mechanism (Verguts and Notebaert, 2008, 2009) or not (Funes et al., 2010a,b; Torres-Quesada et al., 2013; Wühr et al., in press). These types of questions should be (re)considered using the appropriate designs for assessing CSEs.

Finally, the neurophysiological data on the CSE to date do seem to nicely confirm predictions of the conflict monitoring account: Both imaging results (Kerns et al., 2004; Egner and Hirsch, 2005a,b; Kerns, 2006; but see Grinband et al., 2011), EEG data (Stürmer et al., 2002; Larson et al., 2009, 2012; Clayson and Larson, 2011, 2012; Forster et al., 2011; Donohue et al., 2012; but see Wendt et al., 2007), as well as an rTMS (Stürmer et al., 2007) and human single neuron recordings and lesion study (Sheth et al., 2012) are consistent with the proposed ACC-driven conflict detection and DLPFC-implemented conflict resolution control processes. However, all of these studies have employed congruency tasks that were critically contaminated with feature integration and/or contingency learning confounds. Even though no study has yet set out to directly test the neural correlates of these learning accounts, it may well be the case that medial and dorsolateral prefrontal cortex are also critically involved in such associative learning (Grandjean et al., 2013; Schmidt, 2013). The unbiased designs discussed above might therefore be an excellent starting point for further neurophysiological studies that want to elucidate the respective roles of the ACC and DLPFC in producing a CSE. A final challenge for future research then lies in integrating conflict-control and associative learning mechanisms, as well as their interactions and neural substrates into overarching models of cognitive control (see, e.g., Verguts and Notebaert, 2008, 2009).

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What determines the specificity of conflict adaptation? A review, critical analysis, and proposed synthesis

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Over the past decade, many cognitive control researchers have studied to what extent adaptations to conflict are domain-general or rather specific, mostly by testing whether or not the congruency sequence effect (CSE) transfers across different conditions (e.g., conflict type, task sets, contexts, et cetera). The CSE refers to the observation that congruency effects in conflict tasks tend to be reduced following incongruent relative to following congruent trials, and is considered a prime measure of cognitive control. By investigating the transfer of this CSE across different conflict types, tasks, or contexts, researchers made several inferences about the scope of cognitive control. This method gained popularity during the last few years, spawning an interesting, yet seemingly inconsistent set of results. Consequently, these observations gave rise to a number of equally divergent theories about the determinants and scope of conflict adaptation. In this review, we offer a systematic overview of these past studies, as well as an evaluation of the theories that have been put forward to account for the results. Finally, we propose an integration of these various theoretical views in a unifying framework that centers on the role of context (dis)similarity. This framework allows us to generate new predictions about the relation between task or context similarity and the scope of cognitive control. Specifically, while most theories imply that increasing contextual differences will result in reduced transfer of the CSE, we propose that context similarity and across-context control follow a U-shaped function instead.

Keywords: cognitive control, congruency sequence effect, task structure, associative learning

The study of cognitive control is generally concerned with how we adapt our information processing and action selection to constantly changing task environments and goals. Central to this research has been the study of cognitive conflict, where it is investigated how irrelevant information interferes with action selection by evoking conflicting responses. Previous work has convincingly demonstrated that humans (and other animals) have the ability to flexibly and rapidly adapt to such conflicting response activations, in order to carry out the rest of the task (or other related tasks that follow) more efficiently. In the current review we focus on the precise nature of such conflict adaptation processes by zooming in on empirical and theoretical work on the *congruency sequence effect* (CSE).

The CSE is a hypothesized marker of conflict adaptation and has served as an important research tool for investigating the scope of cognitive control. In the lab, the CSE is typically studied by means of a conflict task, such as the flanker, Simon, or Stroop task (Stroop, 1935; Simon and Rudell, 1967; Eriksen and Eriksen, 1974). In the Stroop task, for example, the participants' task is to respond to the ink color of the word, while ignoring the word's meaning. This way, congruent trials, where ink color and word meaning evoke similar responses (e.g., the word GREEN printed in green), and incongruent trials, where ink color and word meaning evoke different responses (e.g., the word RED printed in green), can be created. On incongruent trials, word meaning is believed to interfere with the processing of the ink color, thereby slowing down and occasionally preventing accurate responses. The difference between reaction times or response accuracies is then referred to as the congruency effect (or in this task, the Stroop effect). Whereas the congruency effect is assumed to reflect conflict in information processing, the CSE is typically taken as a proxy for how people adapt their behavior in response to this conflict. Specifically, the CSE concerns the observation that congruency effects tend to be reduced after an incongruent as compared to after a congruent trial. This effect was first observed by Gratton et al. (1992), and is also known as the Gratton or conflict adaptation effect.

A central issue in discussing the nature of conflict adaptation concerns its specificity. On the one hand, it is possible that conflict adaptation is characterized by domain-general boosts in attention that allows us to enhance overall performance. However, as we shall see below, empirical work has resulted in theorizing on more specific adaptation processes. The major tool in exploring such specificity then concerns the transfer of the CSE across various conditions. For example, if one experiences a conflict in a particular task A, to which extent will this influence the processing of cognitive conflict in a subsequent task B? The current paper reviews this type of transfer studies and will consist of three main sections. In a first part of this review, we will outline some of the most prominent theories on cognitive control in general, and the CSE in particular. Importantly, besides conflict adaptation theories, this will also include theories that ascribe the CSE to non-conflict based adaptation but still have something to say about specificity of the CSE as well. Next, we will offer a brief but comprehensive overview of the empirical work thus far that investigated the transfer of the CSE across conditions. These two sections serve as a state-of-the-art reference guide for future work on the specificity of cognitive control. From there, however, we will also re-evaluate and integrate the ongoing theories and ideas in light of these empirical studies. We will close the review by identifying some outstanding research questions and outline how we can validate or falsify these new hypotheses.

THEORIES ON THE CONGRUENCY SEQUENCE EFFECT

In this section, we summarize what we believe to be the most important current theories on the specificity of cognitive control, and the CSE in particular. Although these theories are obviously not mutually exclusive, we will extract from each its core notion (see **Table 1**). This overview is meant to be comprehensive, but not exhaustive. Therefore, our description of theories will focus on, and hence often be restricted to, the hypotheses concerning the specificity of cognitive control, without offering the computational details. We deliberately opted to first provide a general overview of the different theories, as this will allow the reader to better frame and evaluate the empirical findings reviewed in the section to follow.

The most prominent theory of the CSE is the conflict monitoring theory by Botvinick et al. (2001). In this theory, cognitive conflict is proposed to be registered by the anterior cingulate cortex (ACC), which monitors the environment for conflicting response tendencies. This quantifiable measure of conflict then acts as a warning signal that motivates people to increase task focus. Although computationally specific about how conflict detection can be modeled (i.e., by measuring the activity at the response level), the conflict monitoring theory is underspecified in terms of how subsequent control can be autonomously implemented. In the example of the Stroop task, it predicts that ink color detection would be facilitated following Stroop conflict. In a flanker task, however, conflict adaptation would lead to an enhanced processing of target (location) information, relative to flanker (location) information. In this sense, the conflict monitoring theory thus proposes that conflict leads to an enhanced focus on the task-relevant stimulus dimension. This implies that the CSE would be restricted to the enhancement of task-specific processes and therefore would not transfer to alternative tasks: The CSE will only be observed when the previous and current *task-relevant information* remain the same.

Table 1 | Brief description of theories and their view on the scope of conflict adaptation.

Key publication	Conflict adaptation theory	Determinants of the scope of adaptation
Botvinick et al., 2001	Conflict monitoring	Task-relevant information
Egner, 2008	Multiple conflict-control loops	Conflict type
Hazeltine et al., 2011	Set-level control	Task structure or task set
Verguts and Notebaert, 2009	Adaptation-by-binding	Active representations during conflict
Hommel et al., 2004	Feature integration	Active features or event files
Schmidt, 2013	Contingency learning	Contingencies

Egner (2008) offered a more detailed theory on the specificity of conflict adaptation by stressing that the conflict type is what limits the impact of conflict processing on the previous trial. Egner's proposal is inspired by the taxonomy of Kornblum et al. (1990), which allows to differentiate conflict types on the basis of their overlap between, for example, the relevant and irrelevant stimulus dimension (e.g., Stroop task), or between the response dimension and irrelevant stimulus dimension (e.g., Simon task). Using this taxonomy, Egner (2008) proposes the concept of multiple conflict-control loops whereby the detection of one conflict type (e.g., Stroop conflict) will and can only lead to the enhanced recruitment of resources in dealing with that specific type of conflict. Therefore, this theory suggests that (dis)similarity in *conflict type* is the crucial factor determining whether the CSE will transfer from one task to the other.

Hazeltine et al. (2011) stress the role of *task structure* or *task set* in determining the specificity of conflict adaptation (see also Akçay and Hazeltine, 2008). Specifically, Hazeltine and colleagues argue that not the relevant stimulus features *per se*, but rather the entire task set will influence how participants perceive the task, and subsequently determine the scope of conflict adaptation. According to Hazeltine and colleagues, CSEs reflect adjustments in task representations and are highly sensitive to salient or relevant task boundaries. Therefore, the degree to which participants will perceive the tasks as (dis)similar (i.e., the subjective *task set boundaries*) will determine whether or not the CSE can be observed across tasks.

In their adaptation by binding theory, Verguts and Notebaert (2008, 2009) offer a new computational model to explain how adaptations to conflict occur. Although adopting the conflict monitor for conflict detection as proposed by Botvinick et al. (2001), Verguts and Notebaert (2008) take a different approach when it comes to how conflict adaptation is ultimately implemented. Specifically, they argue that upon conflict detection, a Hebbian learning signal is sent throughout the brain that strengthens all ongoing and active representations. As the CSE is typically studied following correct trials only, active representations are usually task-relevant associations and representations—and these are thus predominantly strengthened, leading to increased control. This theory is consistent with the views of Hazeltine et al. (2011) in that it similarly proposes that task representations and their associated boundaries are strengthened following conflict. However, this model goes one step further as this process is indifferent to the precise nature or task-relevance of these features, focusing rather on all active representations.

According to the adaptation by binding theory, then, *every feature that is active during conflict* and coincides with the activation of one or the other task set can codetermine to which extent a CSE will occur on the subsequent trial.

The major reason why the adaptation by binding theory of Verguts and Notebaert is characterized by high specificity relates to the notion that conflict adaptation derives from associative learning processes, rendering it intrinsically bound to the overall set of representations that are active during a particular event, including both task-relevant and -irrelevant information (i.e., the overall context).

Interestingly, a number of theories have been proposed that understand the CSE not so much as a cognitive control phenomenon (i.e., it does not entail conflict-based adaptation), but rather as a direct consequence of specific episodic memory processes. First, Hommel and colleagues propose that CSEs reflect feature integration processes (Mayr et al., 2003; Hommel, 2004; Hommel et al., 2004; Nieuwenhuis et al., 2006). That is, on each trial all the available stimulus and response features are bound together into a so-called event file. However, when some of the features from the previous trial are reused in a new combination (i.e., partial feature repetition), this will require a breakdown of the event file that was formed on the previous trial, and this takes time. Partial feature repetitions taking more time than full repetitions or full alternations of feature sets can produce a similar behavioral pattern as underlies the CSE. Second, Schmidt and colleagues have argued that learning the contingencies between specific stimulus and response features can also lead to this behavioral pattern as especially congruency repetitions are benefiting from such contingency learning processes (Schmidt and De Houwer, 2011). It requires no detailed elaboration that both these episodic memory accounts predict very context-specific effects of the previous trial on the next: *every active feature* or *relevant contingency* can co-determine whether a CSE would be observed across conditions. Importantly, these two accounts, in contrast to the four before-mentioned accounts, do not see congruency identity of the previous trial as an important determinant for CSEs to occur.

TRANSFER OF THE CONGRUENCY SEQUENCE EFFECT

In the previous section, we briefly sketched the stance that theories take on the specificity of the CSE. Here, we will offer an overview of the published empirical work that is relevant to this issue. Complementing our approach to the section above, where we discussed the theories without the data, we will now try to provide a theory-neutral description of the available data. As the scope of the CSE has been a popular topic in recent years, a substantial number of studies has contributed to the discussion of what determines its specificity. We will structure the discussion of these studies based on which research question they tried to tackle (see also **Table 2**). Specifically, we will first discuss all studies that tested the specificity of the CSE by investigating the potential for transfer across different types of conflict. Second, we will discuss a small set of studies that investigated transfer across conflict dimensions (e.g., vertical vs. horizontal Simon task). Last, we will discuss studies that looked at the impact of specific task parameters (i.e., response or stimuli sets) or more contextual task-irrelevant factors, respectively.

THE CONGRUENCY SEQUENCE EFFECT ACROSS CONFLICT TYPES

When investigating the scope of conflict adaptation, one of the first research questions that comes to mind is whether or not one conflict type will influence the processing of another—and indeed most relevant studies have investigated just that. We divided these studies into two broad categories, depending on whether or not the different conflict types were combined in a factorial manner (see Egner, 2008). In factorial designs, the two tasks share the same relevant dimension, and the task-irrelevant features are crossed (i.e., both conflict types are combined within each trial). As such, stimuli can be (in)congruent to one of the two irrelevant dimensions, or to both. The second category involves switching designs (Egner, 2008) in which each trial is (in)congruent with respect to only one of the two irrelevant dimensions, and either share the relevant dimension across all trials (i.e., stimulus-switching designs) or not (i.e., task-switching designs). In all of the abovementioned designs, it can be investigated whether conflict adaptation is specific to one conflict type, or transfers across conflict types. Below, we start out with studies that employed a factorial task-crossing design, and then review studies that used task- or stimulus-switching designs.

In a first study, Kunde and Stöcker (2002) factorially combined spatial and temporal Simon conflict. They asked participants to respond by pressing either long or briefly on a left or right key to colored stimuli that were presented left or right from a fixation cross for either a long or short duration. As such, both the correspondence between stimulus and response location (i.e., spatial Simon conflict) and the correspondence between stimulus and response duration (i.e., temporal Simon conflict) were manipulated. The authors did not observe across-conflict CSEs. However, a within-conflict CSE for the temporal Simon task was also not observed. Four years later, Kunde and Wühr (2006) and Wendt et al. (2006) also used factorial designs to study across-conflict CSEs. Kunde and Wühr used a factorial combination of a horizontal Simon task and a spatial prime-target task. Specifically, a prime arrow was presented before the onset of a target arrow and participants had to respond to the direction of this target arrow with a left or right hand button. The direction of prime and target arrows could either correspond or not, and both stimuli were presented at either the left or right hand side of the screen. This way, two types of congruencies were created: a (non)correspondence between the prime and target arrow direction, and a (non)correspondence between the arrow and response location. As expected, Kunde and Wühr (2006) observed a CSE within conflict type: the Simon effect was smaller following an incongruent Simon trial, and the priming effect was smaller following trials with an incongruent prime-target pair. More interestingly, the authors also observed a CSE across conflict types, albeit smaller than for within conflict type. Wendt et al. (2006) used a factorial combination of a Simon task and a flanker task (Experiment 2A), or a Simon and a Stroop task (Experiment 2B). Thus, each trial could be defined by both Simon and flanker conflict by using a task where flanker stimuli were laterally presented, or Simon and Stroop conflict by laterally presenting Stroop stimuli. In both tasks—and in contrast to the study by Kunde and Wühr (2006)—they observed CSEs within conflict type, but not across conflict type.

Table 2 | Studies investigating the scope of cognitive control, using the congruency sequence effect (CSE).

Authors	Conflict tasks	Method	Findings: specific or global?
CONFLICT TYPE			
Akçay and Hazeltine, 2011	Simon and flanker	A factorial combination of a Simon and a flanker task	*Specific.* CSE was observed within, but not across conflict type
Boy et al., 2010	Flanker and prime-target	A factorial combination of a Simon and a spatial prime-target task	*Specific.* CSE was observed within, but not across conflict task
Egner et al., 2007	Color Stroop and Simon	A factorial combination of a Simon and a color Stroop task	*Specific.* CSE was observed within, but not across conflict task
Fernandez-Duque and Knight, 2008	Number Stroop and Flanker or color Stroop	Performance on a number Stroop task was investigated as a function of previously (cued) flanker or word Stroop congruency	*Global.* CSE was observed across conflict tasks. Notably, the congruency identity of the previous trial was always cued
Forster and Cho, 2014	Simon and Stroop	A Simon and Stroop task with shared response sets were presented in fixed or mixed blocks	*Specific.* CSE was observed within, but not across conflict task
Freitas et al., 2007, Experiments 2 and 3	Flanker and color Stroop or spatial Stroop	An arrow flanker task was intermixed with either a color word Stroop task (Experiment 2), or a spatial Stroop task (Experiment 3)	*Global.* CSE was observed across conflict tasks
Freitas and Clark, 2014, Experiments 2 and 3	Stroop-trajectory, Spatial Stroop, flanker, and Simon	Two different Spatial Stroop tasks were intermixed with a flanker task (Experiment 2) and a newly developed Stroop-trajectory task was intermixed with a flanker and Simon task (Experiment 3)	*Global and specific.* CSE was observed across conflict tasks, except across the Simon and Stroop-trajectory task
Funes et al., 2010a	Spatial Stroop and Simon	A Spatial Stroop task was intermixed with a Simon task	*Specific.* CSE was observed within, but not across conflict tasks
Funes et al., 2010b, Experiments 1 and 2	Spatial Stroop and Flanker or Simon	A Spatial Stroop task was intermixed with a Flanker (Experiment 1) or a Simon (Experiment 2) task	*Specific.* CSE was observed within, but not across conflict tasks
Kan et al., 2013	Color Stroop and sentence processing or perceptual ambiguity	Stroop trials were intermixed with a sentence processing task in a first experiment, and with a perceptual ambiguity task in a second experiment	*Global.* CSE was observed from the sentence processing task, as well as the perceptual ambiguity task, to the Stroop task
Kleiman et al., 2014	Flanker task and a gender flanker task or race priming task	The influence of flanker congruency on stereotypical biases was investigated combining a letter flanker task with a gender flanker task (Experiment 1) or race sequential priming task (Experiment 2)	*Global.* Stereotypical biases were observed following flanker congruent trials, indicated by a CSE from the flanker task to both the gender flanker task, and the race priming task
Kim et al., 2012	Color Stroop and arrow Stroop	A factorial combination of a color and arrow Stroop task	*Specific.* CSE was observed within, but not across conflict type
Kunde and Stöcker, 2002	Spatial and temporal Simon	A factorial combination of a temporal and spatial Simon task	*Specific.* CSE was not observed across conflict type, but, importantly, also not within-conflict type for the temporal Simon task
Kunde and Wühr, 2006, Experiment 2	Simon and prime-target	A factorial combination of a Simon and a spatial prime-target task	*Global and specific.* CSE was observed across conflict tasks, but was smaller across than within conflict type

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Table 2 | Continued

Authors	Conflict tasks	Method	Findings: specific or global?
CONFLICT TYPE			
Kunde et al., 2012	Simon and affective interference	A factorial combination of an affective interference and a Simon task was used where the interference was either of a different type (Experiment 1) or the same type (Experiment 2)	*Specific.* CSE was observed within the conflict types but not across the conflict types in both experiments
Rünger et al., 2010	Flanker and number Stroop	Performance on a number Stroop task was investigated as a function of previously (cued) flanker congruency	*Specific.* CSE was not observed across tasks. This study was set up as a replication study and reported as a replication failure of Fernandez-Duque and Knight (2008)'s Experiment 4
Schlaghecken et al., 2011	Simon and prime-target	A factorial combination of a Simon and a spatial prime-target task	*Specific.* CSE was observed within, but not across conflict type
Verbruggen et al., 2005	Spatial Stroop and Simon	A Simon task was intermixed with a Spatial Stroop task	*Specific.* CSE was observed within, but not across conflict type
Wendt et al., 2006	Simon and flanker or Stroop	A factorial combination of a Simon and a flanker task (Experiment 2A) or a Simon and a Stroop task (Experiment 2B) was used	*Specific.* CSE was observed within, but not across conflict type
Wühr et al., 2014	Simon and Stroop	A manual Simon and verbal Stroop task (Experiment 3) were intermixed	*Specific* CSE was observed within, but not across conflict type
CONFLICT DIMENSIONS			
Cho et al., 2009	Stimulus response compatibility task	A stimulus-response compatibility task was used where each trial was preceded by a cue denoting an either compatible or incompatible response mapping along a horizontal or vertical dimension	*Global.* In four experiments, a CSE was observed across dimensions
Freitas et al., 2007, Experiment 1	Flanker	An arrow flanker task was administered that was oriented on either a horizontal or vertical dimension	*Global.* CSE was observed across dimensions
Freitas and Clark, 2014, Experiment 1	Stroop-trajectory	A newly developed Stroop trajectory task was oriented on either a vertical or horizontal dimension	*Global.* CSE was observed across dimensions
Funes et al., 2010b, Experiments 3 and 4	Spatial Stroop	A Spatial stroop task was varied on horizontal or vertical dimensions with the same (Experiment 3) or a different stimulus set (Experiment 4)	*Global.* CSE was observed across dimensions in both experiments
Kunde and Wühr, 2006, Experiment 1	Prime-target	An arrow prime-target task was presented on either a horizontal or vertical dimension	*Global.* CSE was observed across dimensions
Lee and Cho, 2013, Experiments 1A, 1B, and 4	Simon and Spatial Stroop	The relevant information and conflict type was the same, but the dimension (vertical vs. horizontal) varied in a Simon (Experiment 1A) and Spatial Stroop task (Experiment 1B)	*Specific.* CSE only when the previous and current dimension was the same, even when both dimensions of the Simon task where mapped to the same response (Experiment 4)
Mayr et al., 2003	Flanker	An arrow flanker task was used with either horizontal or vertical arrows	*Specific.* CSE only when the previous and current dimension was the same

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Table 2 | Continued

Authors	Conflict tasks	Method	Findings: specific or global?
CONFLICT DIMENSIONS			
Schmidt and Weissman, 2014	Prime-target	The relevant information and conflict type was the same, but the dimension (vertical vs. horizontal) varied in an arrow prime-target (Experiment 1) and word prime-target (Experiment 2) task	*Global.* CSE was observed across dimensions in both experiments
Wühr et al., 2014	Simon	A vertical and horizontal Simon task with shared relevant dimension (color; Experiment 1) or different relevant dimension (shape and color; Experiment 2) were intermixed (Experiment 1)	*Global and Specific* CSE was always observed within dimensions, yet only across dimensions when both tasks shared the relevant dimension
TASK STRUCTURE, RESPONSE SETS, AND CONTEXT			
Akçay and Hazeltine, 2008	Simon	Two separate response sets were assigned to either shared or segregated stimuli sets in Experiment 1, 2, and 4, and two segregated stimuli sets were assigned to one response set in Experiment 3	*Global and Specific.* CSE was observed within, but not across segregated stimuli sets assigned to two separate response sets. CES was observed across task sets when either stimuli sets or response sets overlapped
Braem et al., 2011	Simon	Stimulus color determined distinctive response sets (hands and feet) vs. similar response sets (combination of hand responses)	*Specific.* CSE across response sets when similar, but not when distinctive
Braem et al., 2014	Flanker	A flanker task was presented in the context of a visual search experiment where task-irrelevant color could interfere with visual search	*Specific.* CSE was only observed when previous and current task-irrelevant color surrounding the flanker stimulus was the same
Fischer et al., 2010	Simon	Single and double-task contexts were mixed	*Specific.* CSE did not depend on task load, but was only observed within and not across task contexts
Hazeltine et al., 2011, Experiment 2 vs. 3	Prime-target	Two stimuli sets were assigned to one vs. two hands	*Specific.* CSE when assigned to the same, but not when assigned to different response set
Hazeltine et al., 2011, Experiments 1 and 4	Prime-target	One (letters) vs. two (letters and animals) sets of stimuli were used in experiment 1 vs. 4, and stimuli were presented in either visual or auditory modality	*Global and Specific.* CSE only when the preceding and the current stimulus were of the same modality. However, CSE was observed across modalities when two stimuli sets were used
Kiesel et al., 2006	Parity/Magnitude Task	A parity task (press left when odd, right when even) was intermixed with a magnitude task (press left when smaller, right when bigger than five) and conflict originated from incompatible mappings	*Specific.* CSE only when tasks repeated, not when tasks alternated
Kim and Cho, 2014	Flanker	One stimulus set was assigned to four fingers of one hand vs. two times two fingers of both hands	*Specific.* CSE across fingers when assigned to one hand, but not when assigned to two hands
Lee and Cho, 2013, Experiment 2 vs. 3	Simon and Spatial Stroop	The two conflict tasks were assigned to the same, or different hands	*Specific.* CSE across conflict types was observed when the same, but not when a different, response set was used for both tasks

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Table 2 | Continued

Authors	Conflict tasks	Method	Findings: specific or global?
TASK STRUCTURE, RESPONSE SETS, AND CONTEXT			
Notebaert and Verguts, 2008	Simon and SNARC	Stimulus color was the relevant dimension in both tasks, or only in one task (and orientation in the other)	*Global and Specific.* CSE was observed across conflict type, but only when task relevant information was the same
Spapé and Hommel, 2008	Color Stroop	Voice gender, irrelevant to the task, was manipulated in an auditory Stroop task	*Specific.* CSE only when previous and current voice gender were the same

Several studies followed in the wake of these first seminal observations. Many used similar factorial combinations of two conflict types where each trial could be subject to two types of compatibility effects. For example, Schlaghecken et al. (2011) used a similar design as Kunde and Wühr (2006), but with a centrally, rather than laterally, presented prime. In contrast to Kunde and Wühr, Schlaghecken and colleagues observed no CSE across conflict types. Additionally, instead of using a Simon task as a secondary task as in Wendt et al. (2006), Boy et al. (2010) used a factorial combination of a prime-target task and a flanker task. These authors, too, observed a CSE within, but not across conflict types. Akçay and Hazeltine (2011) followed up more directly on Wendt et al. (2006) by using a similar design where they factorially crossed Simon and flanker conflict whilst controlling for feature repetition effects. Like in the study by Wendt et al. (2006), a CSE was observed within, but not across conflict type (see also Egner et al., 2007). Kim et al. (2012) also found conflict type specific CSEs when using a factorial combination of an arrow and color Stroop task. Last, Kunde et al. (2012) extended this research into the affective domain, by factorially combining a Simon task and an affective interference task. The affective interference task either consisted of a conflict between the relevant and irrelevant stimulus dimension (i.e., affective pictures and affective words, Experiment 1), or a conflict between the relevant response and irrelevant stimulus dimension (i.e., affective verbal responses and smiley faces, Experiment 2). In both experiments Kunde et al. (2012) only observed a CSE within, but not across, conflict types.

Another set of studies explored the transfer of the CSE across conflict types in paradigms where congruency conditions were not factorially crossed within trials, but rather varied across trials (i.e., stimulus- and task-switching designs). First efforts along this line involve a study by Verbruggen et al. (2005), where spatial Stroop trials were intermixed with Simon trials and only CSEs within, but not across, conflict types were observed. Funes et al. replicated this pattern twice with a similar combination between a Simon and spatial Stroop task (Funes et al., 2010a,b), as well as with a spatial Stroop and flanker task (Funes et al., 2010b). Recently, Wühr et al.(2014; Experiment 3) similarly found no evidence for a transfer of the CSE from a manual Simon task to a verbal Stroop task. Last, intermixing Simon and color Stroop trials, Forster and Cho (2014) again demonstrated how CSEs could only be observed within, but not across conflict types.

However, some of these studies did report a CSE across conflict type. Specifically, it has been observed between a Simon and SNARC task (Spatial-Numerical Association of Response Codes; Notebaert and Verguts, 2008), between a number Stroop task and a flanker task or color Stroop task (Fernandez-Duque and Knight, 2008), between vocal flanker and color Stroop task (Freitas et al., 2007; Experiment 2), between joy-stick-based Flanker and spatial Stroop task (Freitas et al., 2007; Experiment 3), and between two different spatial Stroop tasks and an arrow flanker task (Freitas and Clark, 2014). With respect to the study of Fernandez-Duque and Knight (2008), the congruency identity of the previous trial was always cued which renders the design susceptible to more proactive control processes. Also, it must be noted that Rünger et al. (2010) attempted and failed to replicate the results of Fernandez-Duque and Knight (2008). Therefore, the generalizability of this experiment remains to be tested. Moreover, Freitas and Clark (2014) also used a design intermixing a newly developed Stroop trajectory task (to circumvent feature integration and contingency learning confounds) with a factorial combination of a flanker and Simon task (Experiment 3). Notably, while a CSE was observed between the Stroop trajectory task and flanker task, no transfer of the CSE was observed between the Stroop trajectory and Simon task (CSEs between flanker and Simon congruencies were not analyzed).

Finally, two recent studies by Kan et al. (2013) and Kleiman et al. (2014) used a slightly different approach by combining distinctively different tasks with a Stroop or flanker task. Specifically, a recent study by Kan et al. (2013) showed CSEs across tasks by demonstrating how difficult sentence processing experienced in a sentence reading task, or perceptual ambiguity experienced in a perceptual detection task, can decrease the Stroop effect on a subsequent trial. Similarly, Kleiman et al. (2014) demonstrated how flanker congruency on a previous trial can modulate stereotypical biases measured on the current trial. Specifically, stereotypical biases, measured using a gender flanker task (Experiment 1) or race prime-target task (Experiment 2), were only observed following congruent flanker trials, but abolished following incongruent flanker trials.

Taken together, the studies employing factorial combinations of conflict type generally demonstrate that CSEs are conflict type specific, except for the study by Kunde and Wühr (2006), where a CSE between prime-target and Simon effect was still observed (albeit reliably smaller than was the case for within conflict type). On the other hand, studies that investigated the transfer of CSEs across conflict types by using designs where separate trials belong to either one or the other conflict type, have resulted in a more equivocal set of findings.

THE CONGRUENCY SEQUENCE EFFECT ACROSS SPATIAL DIMENSIONS

A small but substantial number of experiments has been devoted to the detection of CSEs across dimensions but within conflict types. These studies were restricted to spatial congruency effects, where conflict on each trial is induced on either a horizontal or vertical dimension. A first study to investigate this was the study by Mayr et al. (2003), in which an arrow flanker CSE was observed within but not across spatial dimensions. Freitas et al. (2007), however, reported on a reliable CSE across dimensions in a similar setting (Experiment 1), which, they argued, could be due to their shorter stimulus presentation time. Similarly, Kunde and Wühr (2006, Experiment 1) later on demonstrated how the CSE can also be observed across spatial dimensions when administering an arrow prime-target task. Comparable studies followed and CSEs across vertical and horizontal dimensions have been observed in spatial Stroop tasks (Funes et al., 2010b, Experiments 3 and 4), stimulus-response compatibility tasks (Cho et al., 2009), arrow and word prime-target tasks (Schmidt and Weissman, 2014), a Stroop trajectory task (Freitas and Clark, 2014), and the Simon task (Braem et al., 2011; Wühr et al., 2014, Experiment 1). Finally, in addition to Mayr et al. (2003), one more study has failed to observe such CSEs. Specifically, Lee and Cho (2013, Experiments 1A and 1B) did not observe a congruence sequence effect across dimensions when varying dimensions in a Simon (Experiments 1A) or spatial Stroop task (Experiments 2B). Overall, then, with two exceptions (Mayr et al., 2003; Lee and Cho, 2013) it has been consistently demonstrated that CSEs can be observed across vertical and horizontal dimensions within conflict type.

THE IMPACT OF TASK SETS ON THE CONGRUENCY SEQUENCE EFFECT

A third group of studies manipulated specific task parameters to investigate the determinants of CSEs across task sets. According to the definition offered by Schneider and Logan (2014), task sets can be understood as "a set of representations and processes capable of performing a task, including the parameterization of those processes and the identification of their neural substrates" (Schneider and Logan, 2014, p. 29). Importantly, and in strong contrast to some of the work described above, these studies mostly kept conflict type constant, but were interested in whether or not stimulus sets or response sets might co-determine the scope of conflict adaptation. For example, Kiesel et al. (2006) used a parity/magnitude task-switching study in which congruency conditions were created by partially (in)compatible response mappings between both tasks. Using this task, Kiesel et al. (2006) were the first to demonstrate how CSEs are task-specific and thus observed on task repetitions only. Notably, the two tasks used competing response mappings. For example, in the parity task participants had to press left when the number was odd and right when even, whereas in the magnitude task participants had to press left when the number was smaller than five and right when bigger than five. This way, congruent numbers (i.e., "1") required the same response on both tasks, whereas incongruent numbers (e.g., "2") did not.

Other studies used either the same, or not necessarily competing, response mappings to investigate the impact of task sets on the specificity of conflict adaptation. In two closely matched conditions, Notebaert and Verguts (2008) demonstrated how their above-mentioned transfer of CSE between Simon and SNARC tasks crucially depended on whether or not both tasks used the same task-relevant information, as the transfer disappeared when both tasks where assigned to different task-relevant information. In a similar vein, Wühr et al. (2014) only showed transfer of the CSE between vertical and horizontal Simon tasks when the relevant dimension was identical between the two tasks (i.e., color), but not when the relevant dimension varied across tasks (i.e., color and shape). Other studies focused on whether or not two tasks use the same response set or not. Braem et al. (2011), for example, demonstrated how a CSE could be observed between a vertical and horizontal Simon task, but only when two highly similar response sets were used (a complex combination requiring both hands for both tasks), and not when both response sets distinctively differed (hand vs. feet). Kim and Cho (2014) observed a similar dependence on response sets in a four-color flanker task where two out of four colors were only presented on odd trials, and the other two on even trials. Specifically, when assigning the four horizontally aligned color buttons to four fingers from one hand, a CSE was observed across colors. However, when the two leftmost buttons were assigned to the left hand (odd trials), and the rightmost buttons to the right hand (even trials), no CSE was observed. In a similar vein, Lee and Cho (2013) showed how a CSE between a spatial Stroop and Simon task could be obtained, but only when the tasks were assigned to the same response hand (Experiments 2 vs. 3). Again using a Simon task, Akçay and Hazeltine (2008) likewise demonstrated how the CSE could not be observed when two segregated stimuli sets were assigned to two separate response sets. However, whenever either the stimuli sets or the response sets overlapped, a CSE across conditions was found. In a similar vein, using a prime-target paradigm, Hazeltine et al. (2011) demonstrated how two stimuli sets assigned to either one or two hands only showed an across-set CSE when both sets were assigned to one hand (Experiments 2 vs. 3). Overall, these studies indicate that the partitioning of particular stimuli and response sets within a certain task can be a sufficient condition to observe set-specific CSEs.

CONTEXTUAL TASK-IRRELEVANT FACTORS AND THE CONGRUENCY SEQUENCE EFFECT

A final set of studies investigated the CSE as a function of task-irrelevant contextual factors. Most notably, Spapé and Hommel (2008) demonstrated how the Stroop CSE is sensitive to voice gender in an auditory Stroop task. In their Stroop task, color detection could be facilitated or hampered by the auditory presentation of congruent or incongruent words spoken by male or female voices. Interestingly, CSEs were only observed when voice gender repeated, but not when voice gender alternated. In a similar vein, Hazeltine et al. (2011) demonstrated how the CSE depended on whether or not the previous and current stimuli were of the same modality (visual or auditory, Experiment 1). However, when increasing stimulus set heterogeneity by introducing a second different category of stimuli, this modality-specificity of the CSE disappeared (Experiment 4). Investigating the impact of task load on the CSE, Fischer et al. (2010) observed how the CSE was dependent on whether both the current and previous trial were presented in both dual task or single task conditions, but not when alternating between them. The authors concluded that task context, rather than task load, is a crucial determinant in bringing about the CSE. Last, Braem et al. (2014) recently observed how a flanker CSE depends on whether or not a surrounding shape is presented in the same, or rather in an alternative color as the previous trial. By pairing a visual search task with a flanker task (i.e., participants had to search a unique shape out of six shapes and respond to the flanker task presented within that shape), these authors demonstrated that only under conditions where task-irrelevant shape color repeated, a congruence sequence effect occurred. Together, these experiments all seem to suggest that the repetition or alternation of task-irrelevant contextual salient features can codetermine whether or not a CSE will occur.

CRITICAL ANALYSIS

On the basis of our review we can identify two important factors determining the specificity of conflict adaptation: conflict type and context-similarity. When it comes to conflict type, we agree with Egner's (2008) review that in full-factorial designs there is abundant evidence that the CSE appears to be conflict type-specific (Egner, 2008). Only Kunde and Wühr (2006) observed a reliable, albeit smaller, CSE across congruency conditions, which, according to Egner (2008), was most likely due to the high similarity between the two conflict types. Consistently, when Schlaghecken et al. (2011) attempted to replicate Kunde and Wühr (2006) with a centrally (rather than laterally) presented prime, they did not observe a CSE across conflict types.

A second line of research used non-factorial designs. Note that Egner (2008) argued against using such designs because they are often confounded with switch costs and do not allow the researcher to investigate if both conflict types are independent (additive) or not. Importantly, Egner (2008) was only interested in the conflict-specificity of the CSE, whereas we, in our review, are interested in the scope of conflict adaptation more generally (across various conditions). Therefore, we consider switch costs a second, informative symptom of the same phenomenon that might explain the absence of a transfer of the CSE across conditions: participants represented both conflict types as deriving from two different task sets (see below). For this reason, we will also discuss these studies, but treat them separately as studies indexing the impact of task sets or context, rather than conflict type. Although some of these studies' main intention was to investigate the conflict-specificity, the impact of conflict type cannot be disentangled from task set or task context. Therefore, these studies do not allow to make more specific inferences about conflict-specificity (Egner, 2008).

Interestingly, despite the fact that most studies using non-factorial designs found task-specific CSEs, some found across task CSEs. Most of these across-task—and thus relatively domain-general—CSE observations can be ascribed to using either similar conflict types (e.g., Fernandez-Duque and Knight, 2008; Notebaert and Verguts, 2008; Freitas and Clark, 2014) and/or very similar response mappings (e.g., Freitas et al., 2007; Notebaert and Verguts, 2008). Indeed, when using different conflict types (Freitas and Clark, 2014), or different response mappings (Notebaert and Verguts, 2008), some of these studies reported task-specific CSEs as well. Additionally, Rünger et al. (2010) attempted and failed to replicate the results of Fernandez-Duque and Knight (2008) so there might be need for further replication studies (see suggestions for replication endeavors below). However, it is not our aim to refute these domain-general CSE observations as methodologically flawed. In fact, such observations might sometimes be obscured by the lack of statistical power to observe more subtle across-domain CSEs. Moreover, as we will argue below, these observations might be expected in cases where both task sets are not interfering with each other.

In contrast to the full-factorial designs described above, we cannot conclude on whether or not these studies evidence the conflict specificity of the CSE. However, at the very least these studies do suggest that task structure can determine the specificity of CSE. This idea that task sets might play an important role in determining the scope of the CSE is not new. Indeed, as reviewed above, Hazeltine and colleagues (Akçay and Hazeltine, 2008; Hazeltine et al., 2011) clearly stressed the role of task sets and task boundaries in bringing about CSEs. Recent evidence seems to support this hypothesis. An increasing number of papers have demonstrated this importance of stimulus and response sets by demonstrating how CSEs across task sets do not occur when both sets are clearly distinguishable and thus perceived as different task sets, especially when both sets have conflicting response mappings. Interestingly, this does not necessarily depend on the complexity of the task, which naturally increases with using multiple stimuli sets. In fact, Hazeltine and colleagues demonstrated how increasing stimulus set size and modality to the extent that it is not longer beneficial to dissociate the different sets, allows for across-modality and across-set CSEs to occur (Hazeltine et al., 2011, Experiment 4). We believe this role of task sets remains an under-investigated aspect of conflict adaptation, and we will offer some suggestions below as to how the implementation and impact of such instructed task sets can be further investigated.

The adaptation-by-binding theory offers a computational implementation of conflict adaptation. It states that upon conflict detection, a general Hebbian learning signal ("now print") is sent throughout the brain to strengthen all active ongoing associations. Task set representations are activated by task demand units which can, just like simple stimulus or response units, be strengthened following conflict detection. Therefore, this theory can account for the susceptibility of CSEs to task set boundaries. However, it actually goes one step further and predicts that also task-irrelevant features can influence the scope of conflict adaptation. In fact, everything that is salient and/or systematically co-activated can be picked up by this Hebbian learning mechanism and incorporated in the strengthening of associations. This way, we can expect that if a salient event occurs in the temporal vicinity of conflict detection, task associations might become temporarily associated with this stimulus feature or event and CSEs can therefore be specific to the repetition of this event. Indeed, as reviewed above, a number of studies have observed such context-specific CSEs. In some of these studies, this effect was not anticipated (e.g., Fischer et al., 2010), so there is clear room for further systematic investigations on the impact of such contextual features.

PROPOSED SYNTHESIS

Taken together, these stimulus-, task-, or context-switching designs might be integrated by the general principle that conflict adaptation is highly specific to the context—where we should understand context in its broadest sense to cover the impact of both (instructed) task-relevant features (including, for example, S-R mappings) as well as task-irrelevant (but salient) features. However, how then should we understand the few studies that seem to refute context-specificity, such as those of Kan et al. (2013) and Kleiman et al. (2014) which demonstrated that CSEs can be observed across very different task sets? Therefore, we propose the working hypothesis that transfer of CSEs can be observed across contextual features (both task-relevant and -irrelevant) as long as these features are simultaneously and actively maintained (in working memory). Studies in which two contexts are used that would substantially interfere with each other when they are both actively maintained, will result in strategies where only one context is active at any time such that transfer is prevented. This fits nicely with interference based models of working memory as developed by Oberauer and colleagues (Oberauer and Kliegl, 2006; Oberauer et al., 2012) where working memory capacity restrictions arise from interference, rather than, for example, limited resources. Similarly, Oberauer et al. (2012) model suggests that interference in working memory will be greater when (task) features overlap or belong to similar categories. Therefore, two contexts that are sufficiently different such that simultaneous maintenance is possible without much interference (cf. Kan et al., 2013; Kleiman et al., 2014), may result in transfer across (very different) contexts. Hence, whereas one would predict a linear relation between context similarity and the chance of observing transfer of the CSE from the idea that task sets determine the scope of conflict adaptation (Hazeltine et al., 2011), we predict a U-shaped relationship (see **Figure 1**): transfer is observed whenever two contexts (task-relevant and -irrelevant) are either very similar or sufficiently dissimilar to prevent interference.

Importantly, this idea is not entirely new. Hazeltine and colleagues made a similar observation when discussing the findings of Freitas et al. (2007) who did observe a CSE across task sets. Specifically, they argued that because Freitas and colleagues increased the heterogeneity within their tasks, and switched the tasks randomly, the salience of the task boundaries was reduced, allowing CSEs to occur across task sets (Hazeltine et al., 2011). This fits with our proposal that *interfering contexts* or *interfering tasks* rather than *context or task similarity* are the key factor in determining the scope of conflict adaptation. Interestingly, this might also explain why factorial combination studies mostly observe conflict-specific CSEs while similar investigations using stimulus-switching designs have sometimes observed across-conflict CSEs. Whereas the former closely intermix different congruency conditions, the latter dissociate both congruency conditions to the extent that they can again either be perceived as one task set, especially when they share task-relevant information (Notebaert and Verguts, 2008), or as two non-interfering task sets, that can easily be maintained in parallel in working memory.

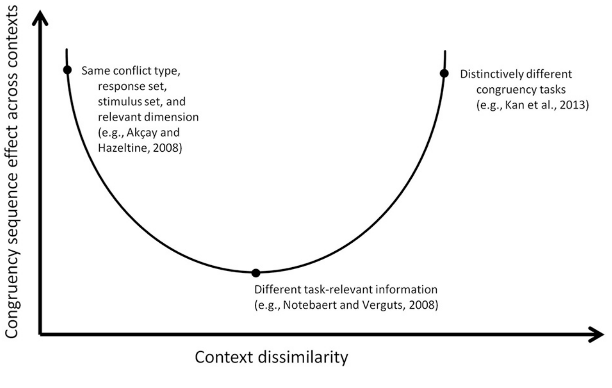

FIGURE 1 | Abstract depiction of the hypothesized u-shaped relation between context (dis)similarity and congruency sequence effects across contexts. As per example, three different empirical studies are displayed along the function.

This idea is also compatible with the computational model of Verguts and Notebaert (2008, 2009). In their adaptation by binding theory, Verguts and Notebaert propose that following conflict detection, a general Hebbian learning signal is sent throughout the brain that reinforces all active and ongoing associations. Importantly, although the model is blind to what is task-relevant or not, task-relevant associations are mostly the ones that are strengthened since these are the most active during conflict-resolution. Whenever tasks are defined on the basis of features (e.g., S-R mappings) that are mutually interfering, the result may be that at each moment in time (i.e., each trial) only one of the two tasks can be actively maintained—and thus that binding processes underlying adaptation are specific to one but not the other task (no "transfer" between tasks). Now, in such designs where interfering associations (e.g., from interfering task sets) have to be suppressed to ensure successful conflict resolution, we can predict that these task sets will not benefit from this Hebbian learning signal, and domain-specific CSEs will be observed (halfway the U-shaped function on **Figure 1**). However, when both task sets are very compatible either because they are highly similar (left hand side of the U-shaped distribution on **Figure 1**), or because they are highly distinctive and can be simultaneously kept available in working memory (right hand side of the U-shaped distribution on **Figure 1**), domain-general CSEs might occur.

Lastly, we would like to add that memory-based theories (Hommel et al., 2004; Schmidt, 2013) should not—as is currently the case—be seen as theoretical alternatives to conflict adaptation theories (for similar arguments, see Spapé and Hommel, 2008; Verguts and Notebaert, 2008; Braem et al., 2011; Hazeltine et al., 2011; Jimenez and Méndez, 2014). In fact, these theories and their predictions concerning the specificity of CSEs are largely compatible to the above-made predictions and observations. For example, using their theory of event codes, Hommel et al. (2004) proposed that the CSE can be understood in terms of slower partial repetitions vs. faster complete repetitions or alternations of event files (see above). This view can easily be extended to task sets or contexts. CSEs without feature repetitions have now been demonstrated (e.g., Duthoo and Notebaert, 2012), but it is still possible that (latent) partial repetitions of certain task sets can prevent CSEs from occurring across task sets. In fact, this idea is compatible with our proposed U-shaped function between task-similarity and CSEs across tasks: complete repetitions (of largely overlapping task sets) or complete alternations (of distinctively different task sets) can allow for across-task CSEs to occur, while partial repetitions (task sets partially sharing features or resources) are more demanding for working memory and therefore induce task-specific CSEs. Interestingly, these learning theories motivate us to further pursue the research question how exactly task structures are implemented, remembered, and dealt with, while simultaneously investigating the impact of contextual features and associative learning. However, although learning theories as such, we believe, offer important theoretical insights, and need to be taken into account when considering modulations of cognitive control, they do need to be controlled for when using the CSE as a metric of conflict adaptation. For example, while the theory of event coding (Hommel, 2004; Hommel et al., 2004) is consistent with our proposed U-shaped function between context-similarity and CSEs across contexts, it does not account for observations showing CSEs devoid of feature integration, as it does not acknowledge a role for congruency identity of the previous trial as an important determinant for the CSE. Importantly, such CSEs devoid of feature integration and contingency learning have been demonstrated (for a comprehensive review in this same issue, see Duthoo et al., 2014), but only recently. As a result, only a small number of the above-mentioned studies controlled for both feature integration and contingency learning (Freitas and Clark, 2014; Kim and Cho, 2014; Schmidt and Weissman, 2014). These paradigms should be taken as guides for further research on the specificity (and other modulations) of conflict adaptation. In fact, such experiments are much needed as it is currently unclear to which extent earlier studies on the CSE (and their modulations) could have been attributed to their confound with feature integration or contingency learning.

CONCLUSIONS, CHALLENGES, AND GUIDELINES

In sum, we reviewed the most prominent theories, and, to our knowledge, all studies that investigated the specificity of conflict adaptation. We identified a number of elements that can determine the scope of conflict adaptation. In fact, in line with the reviewed theories, conflict type (Egner, 2008), task set (Hazeltine et al., 2011), and context (Verguts and Notebaert, 2009), all seem to play a central role in determining whether or not across-condition CSEs will occur. In contrast, conflict dimension (vertical vs. horizontal) did not appear to be a crucial factor (but see Mayr et al., 2003; Lee and Cho, 2013). To account for the current set of data, we proposed a U-shaped function between context similarity and cross-condition conflict adaptation. Therein, we stress the role of task sets and whether or not they can be simultaneously activated in working memory. We believe there are still a number of challenges ahead, opening up new opportunities for further research. Therefore, we will end by identifying some of those challenges and offer a number of tentative guidelines on how one might tackle them.

First, we illustrated how the current state of the art in conflict adaptation research and its specificity can be understood in terms of a U-shaped relation between transfer of the CSE and context similarity. Importantly, whenever both task sets can be simultaneously updated because they are either highly similar or distinctively different and non-interfering, adaptation across tasks and conditions can be observed. We believe this hypothesis is testable or falsifiable by using a design where task sets or conditions are parametrically dissociated. However, in setting up such a design it will be important to develop a paradigm where the task-rules are not overly complicated (participants should still be able to keep both tasks in working memory).

Second, we re-emphasize the importance of taking into account task sets when investigating the specificity of conflict adaptation (Hazeltine et al., 2011). In this respect, a promising new research field on task instructions has developed a number of interesting paradigms that allow us to test the effects of task sets whilst controlling for the repetition or alternation of more low-level stimulus or response feature characteristics. For example, Dreisbach et al. (2007) elegantly demonstrated how it is possible to introduce a difference in task sets by mere instructions, without having to manipulate stimulus features or stimulus-response mappings. Specifically, in their study, Dreisbach et al. (2007; see also Dreisbach and Haider, 2008, 2009) trained participants at certain stimulus-response (S-R) rules between eight stimuli and two responses after which they could either receive, or not receive, an overarching rule that is able to categorize the same S-R rules as belonging to one out of two task sets. Interestingly, while both groups performed sufficiently well at the S-R rules, performance in the late-informed task set group was worsened, relative to the uninformed group, as evidenced by task-switch costs following task-rule instructions. Clearly, this implementation of task sets introduced new task boundaries that interfered with switching between the two groups of stimuli. Importantly, paradigms like these can allow us to investigate the impact of task sets on conflict adaptation, without having to create different stimuli. Moreover, these studies allow us to investigate how exactly task sets are implemented as this remains an under-investigated issue in the cognitive control literature (see also Everaert et al., 2014; Liefooghe et al., 2012).

Third, we discussed a number of recent studies that demonstrated the impact of salient task-irrelevant uninstructed features on the specificity of conflict adaptation. However, these studies are still relatively scarce, and a systematic investigation of these effects seems warranted. Moreover, whereas theories predicting these effects (Verguts and Notebaert, 2008) seem to emphasize these kind of bottom-up effects, others have emphasized top-down effects (Hazeltine et al., 2011). We have stressed the importance of both instructed task sets as well as the impact of these contextual features. Therefore, an interesting challenge remains the investigation of how the impact of task-irrelevant uninstructed features might still differ from the impact of instructed task sets on conflict adaptation, as well as how they interact. We argued that both can co-determine the scope of conflict adaptation. However, future research should offer a more nuanced view by unraveling the complex interplay between the effects of (top-down) task sets vs. (bottom-up) context on conflict adaptation.

Lastly, researchers should engage in trying to test the impact of specificity with paradigms that are free of feature integration or contingency learning confounds (Freitas and Clark, 2014; Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014). This is non-trivial, as most measures of the CSE thus far can be alternatively explained by low-level memory effects. In another review article in this same issue, we outline the specific problems that one needs to take into account, and outline a number of guidelines on how researchers can develop the appropriate paradigms (Duthoo et al., 2014). However, this need for dissociating learning effects from CSEs is purely methodological, and should not be mistaken for a motivation to theoretically distance those from conflict adaptation research. In fact, here, as well as in earlier works (Spapé and Hommel, 2008; Verguts and Notebaert, 2008; Braem et al., 2011; Hazeltine et al., 2011; Jimenez and Méndez, 2014), it has been argued that memory effects and associative learning can offer important insights in the underlying mechanisms and dynamics of conflict adaptation.

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Creatures of habit (and control): a multi-level learning perspective on the modulation of congruency effects

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The congruency sequence effect (CSE) describes the finding that congruency effects in classic probes of selective attention (like the Stroop, Simon, and flanker tasks) are smaller following an incongruent than following a congruent trial. The past two decades have generated a large literature on determinants and boundary conditions for the CSE and similar, congruency-proportion based modulations of congruency effects. A prolonged and heated theoretical discussion has been guided primarily by a historically motivated dichotomy between "top-down control" versus "associative bottom-up" explanations for these effects. In the present article, I attempt to integrate and contextualize the major empirical findings in this field by arguing that CSEs (and related effects) are best understood as reflecting a composite of multiple levels of learning that differ in their level of abstraction. Specifically, learning does not only involve the trial-by-trial encoding, binding, and cued retrieval of specific stimulus–response associations, but also of more abstract trial features. Moreover, these more abstract trial or event features can be both external, such as the spatial and temporal context in which a stimulus occurs, as well as internal, like the experience of difficulty, and the attentional control settings that were employed in dealing with the stimulus. From this perspective, top-down control and bottom-up priming processes work in concert rather than in opposition. They represent different levels of abstraction in the same learning scheme and they serve a single, common goal: forming memory ensembles that will facilitate fast and appropriate responding to recurring stimuli or events in the environment.

Keywords: cognitive control, feature integration, memory, attention, congruency sequence effect, proportion congruent effect, conflict adaptation, contingency learning

INTRODUCTION

Tests of the effectiveness of controlled attention typically require participants to produce a response to a task-relevant stimulus feature (target information) in the presence of task-irrelevant stimulus features (distracter information), which can be either congruent or incongruent with the former. For instance, in the classic color-naming Stroop task (Stroop, 1935; MacLeod, 1991), subjects have to indicate the ink color of written color-words (e.g., RED) while ignoring the word-meaning. The relative success (or failure) of attentional filtering is gauged by contrasting performance on trials where the distracter is congruent (e.g., the word RED written in red ink) with those where it is incongruent (e.g., the word RED written in blue ink) with the target, and may therefore interfere with target processing unless it is effectively ignored. The canonical finding is a marked congruency effect: responses are slower and more error-prone to incongruent compared to congruent stimuli, suggesting imperfect attentional selection. Importantly, the size of the congruency effect, and by implication, the effectiveness of attentional filtering, has been shown to be malleable by a variety of factors, such as the frequency of incongruent stimulus occurrences (Logan and Zbrodoff, 1979), the explicit cueing of forthcoming congruency (Gratton et al., 1992), and the congruency of the previous trial (Gratton et al., 1992). The latter refers to the so-called *congruency sequence effect* (CSE), the finding that the influence of distracters on the processing of target information is typically dampened on trials that follow an incongruent trial compared to those that follow a congruent trial (**Figure 1A**; for reviews, see Egner, 2007; Duthoo et al., 2014a).

Over the past two decades, the CSE has garnered much attention in the Cognitive Psychology and Neuroscience literatures, with a central debate focusing on rival explanations for this phenomenon, which are typically grouped into two major categories, "top-down control-based" versus "bottom-up associative" accounts (see, e.g., Gratton et al., 1992; Botvinick et al., 2001; Mayr et al., 2003; Hommel et al., 2004; Blais et al., 2007; Egner, 2008; Verguts and Notebaert, 2008, 2009; Hazeltine et al., 2011; Schmidt, 2013). In the present hypothesis article, I attempt to provide an integrative perspective on these accounts, as well as related phenomena of contextual modulations of congruency effects. Put simply, I will argue that the different accounts of these effects' origins ultimately describe complementary learning processes operating at different levels of abstraction but driven by a common principle and goal: the matching of incoming

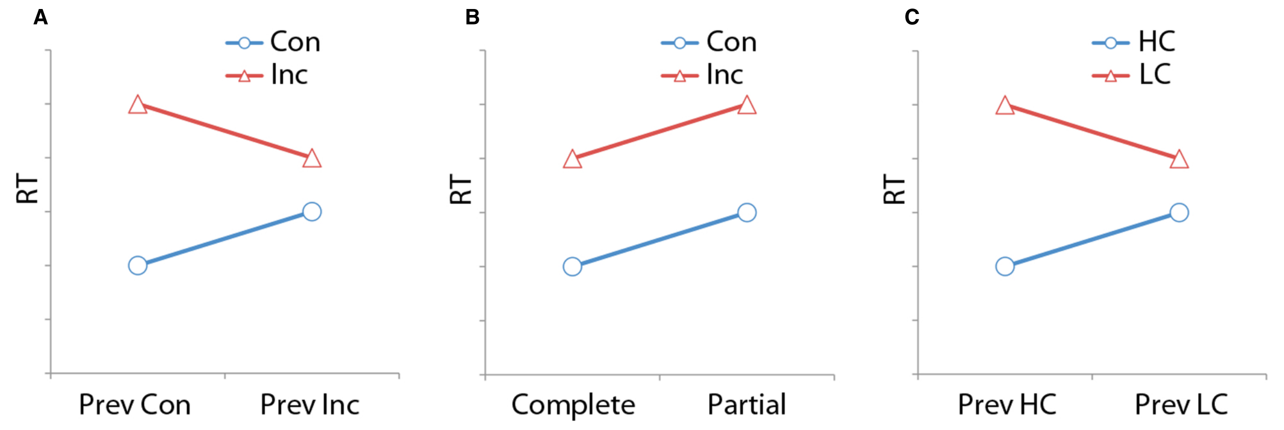

FIGURE 1 | Different interpretations of first-order sequence effects in conflict tasks. (A) Hypothetical data plotted and labeled as a congruency sequence effect (CSE), where response time (RT) varies as a function of an interaction between previous trial congruency and current trial congruency. Con, congruent; Inc, incongruent; Prev, previous trial. **(B)** The same data as in **(A)**, plotted in line with the feature integration account, whereby the data pattern represents two additive effects of congruency and feature repetitions. Complete, complete feature repetitions or complete feature alternations from previous trial; Partial, partial feature repetitions from previous trial. **(C)** The same data and plotting as in **(A)**, but relabeled in line with the interpretation of the contingency learning account, where the data are explained as an interaction between previous trial and current trial contingency effects. LC, low-contingency trials; HC, high-contingency trials.

stimulation (external states) to memories (internal states) in the service of producing fast, goal-conducive action. At the most concrete level, the organism binds together co-occurring physical stimulus and response features, whereas at a more abstract level, we associate complex contextual cues with generalizable control states. I will first provide a brief overview of some rival CSE accounts and the empirical status quo, followed by the main argument for viewing distinct sources of CSEs as describing different, co-occurring levels of a broader learning process aimed at optimizing stimulus processing and action selection.

A SELECTIVE COMPENDIUM OF CSE ACCOUNTS

CONTROL-BASED PERSPECTIVES

In line with the standard interpretation of congruency effects as reflecting a measure of attentional selectivity, the original observation of the CSE was interpreted as the expression of an adjustment in attentional strategy (Gratton et al., 1992). More specifically, Gratton et al. (1992) reasoned that encountering a congruent or incongruent trial would engender an expectation for the forthcoming trial to be of the same congruency (cf. Remington, 1969), which in turn would lead subjects to strategically enhance (following incongruent trials) or decrease (following congruent trials) their attentional focus on the target stimulus feature, thus decreasing the influence of distracters (and, *ergo*, the congruency effect) following an incongruent trial, and increasing the influence of distracters (and, *ergo*, the congruency effect) following a congruent trial.

About a decade later, a related, though much more formalized (and influential), control-based account for the CSE was put forward by Botvinick et al. (2001), who marshaled this effect as evidence to support a "conflict-monitoring" model of cognitive control. Briefly, these authors advanced an elegant computational scheme for a "homunculus-free" regulation of top-down attention, positing that the cognitive apparatus detects internal conflict between mutually incompatible stimulus or response representations (e.g., the simultaneous urge to answer "red" and "blue" when faced with the incongruent Stroop stimulus described above), and uses the degree of conflict to produce commensurate adjustments in top-down attention—the more conflict is experienced, the more control will be applied. Thus, when an incongruent stimulus is encountered, the processing conflict caused by the incongruent distracters triggers an up-regulation of attentional focus toward the target, which results in more efficient attentional selection (and hence, a smaller congruency effect) on the following trial; the opposite is true when encountering a low-conflict, congruent trial, which results in a relaxation of attention and, thus, less efficient attentional selection (and a larger congruency effect) on the following trial. This particular interpretation of the CSE is known as *conflict adaptation*.

While there are clear conceptual differences between the expectation- and conflict-based accounts (see, e.g., Egner, 2007; Egner et al., 2010; Duthoo et al., 2013; Jimenez and Mendez, 2013), for the present purpose they can both be considered core members of the "control-based" model category, along with various proposed refinements and extensions of the basic conflict-monitoring proposal (e.g., Botvinick, 2007; Egner, 2008; Hazeltine et al., 2011; Dreisbach and Fischer, 2012; Jiang et al., 2014). First, these views commonly assume that the CSE results from strategic adjustments in top-down attention or task-set. Second, and more importantly, these accounts all operate at a level of processing adjustment that is, in principle, independent of what the specific stimulus features or responses are that will comprise the subsequent trial. For instance, conflict- (or expectation-) triggered enhanced attentional filtering in the Stroop task would result in improved ink color selection regardless of the exact nature of that color or the distracter word information in the upcoming trial. As will be discussed in more detail later on, this "level of abstraction" of the mechanism that is held responsible for the CSE, concerning either a *generalizable cognitive state* (for instance, attentional focus) or *specific stimulus or response characteristics*, represents the key distinguishing feature between control-based and associative accounts of the CSE.

According to this criterion, we can also subsume under the control-based category CSE accounts that focus on control adjustments at the level of response selection rather than perceptual attention. For instance, the "activation–suppression model" posits a control mechanism that detects and suppresses response activation elicited by distracter stimuli, and assumes this mechanism to work more effectively if it had been recently activated (i.e., by a prior incongruent trial; e.g., Ridderinkhof, 2002; van den Wildenberg et al., 2010). Crucially, this model nevertheless assumes that the process which is facilitated following an encounter with an incongruent stimulus, namely the categorical suppression of "distracter-route" responses, is independent of the specific features of the subsequent stimulus. By contrast, certain "hybrid accounts" that espouse the notion of conflict-enhanced control, but link this mechanism to specific stimulus features (Blais et al., 2007; Verguts and Notebaert, 2008; Blais and Verguts, 2012), defy the present categorization scheme; these models will be discussed in Section "A Multi-level Learning Perspective on the Modulation of Congruency Effects."

ASSOCIATIVE PERSPECTIVES

Associative accounts have proposed that the CSE may stem from memory-driven effects, based on differing frequencies with which specific stimulus and response features repeat over consecutive trials for different congruency sequences (Mayr et al., 2003; Hommel et al., 2004). For instance, Hommel et al.'s (2004) *feature integration* account is grounded in prior work showing that the specific stimulus and response features that co-occur on a given trial of an alternative forced-choice (AFC) task (say, the word RED in blue ink is responded to with a left button press in the above-mentioned Stroop task) become *bound together* in episodic memory as an "event file" (Hommel, 1998, 2004; cf. Treisman and Gelade, 1980). Moreover, the subsequent re-occurrence of any one of these features (e.g., the word RED) appears to trigger the retrieval of the entire prior event file, presumably to supply a potential shortcut to the correct response associated with a previously seen stimulus (cf. Logan, 1988). This feature-binding mechanism leads to a relative facilitation of processing when all of the current trial features match the previous event (*complete repetitions*; see also Pashler and Baylis, 1991; Mayr et al., 2003), or when there is no feature overlap across successive trials (*complete alternations*), relative to cases where some features are repeated but others are not (*partial repetitions*), because in the latter scenario, the retrieved event file has to be either discarded or "unbound" in order for the currently presented stimulus to be responded to correctly (Hommel, 1998, 2004; see also Neill, 1997).

By way of example, consider once more the Stroop task alluded to earlier, consisting of a stimulus set of the words RED and BLUE, printed in either red or blue ink, thus rendering a total of two congruent and incongruent stimuli, and two possible responses. Here, congruent–congruent and incongruent–incongruent trial sequences will consist entirely of complete feature repetitions or complete alternations (and thus, result in fast responses), whereas congruent–incongruent and incongruent–congruent trial sequences will consist entirely of partial feature repetitions (thus resulting in slow responses). Hence, the CSE data pattern of reduced congruency effects following an incongruent trial compared to a congruent trial can be re-interpreted as reflecting a basic congruency effect paired with a relative handicapping of trials where partial feature repetitions impose an "unbinding cost" on performance (**Figure 1B**; Hommel et al., 2004). Evidently, in contrast to the control-based accounts, this associative perspective requires no trial-by-trial adjustments of selective attention to explain the CSE, and, importantly, the proposed mechanism underlying this effect operates at the level of specific stimulus features and responses.

Given that the feature integration account highlights potential associative confounds in the CSE that seem to be specifically inherent to small stimulus and response sets, a natural response to these concerns was a movement toward employing conflict tasks with larger sets (typically, moving from 2-AFC to 4-AFC schemes), such that first-order repetitions of stimulus and response features could be either prophylactically prevented from occurring (e.g., Puccioni and Vallesi, 2012; Jimenez and Mendez, 2013), or removed from analysis after the fact (e.g., Ullsperger et al., 2005; Akcay and Hazeltine, 2007). However, as recently highlighted by several authors (Schmidt and De Houwer, 2011; Mordkoff, 2012), this trend may have introduced a new associative confound to the CSE, in the form of *contingency learning*. Specifically, the expansion of the stimulus set (for instance, going from two to four colors in the Stroop task) creates more possible unique incongruent than congruent stimuli. When researchers then present congruent and incongruent trials with the same frequency (i.e., 50%), each congruent stimulus occurs more frequently (and well above chance) than each incongruent stimulus, which creates a contingency linking each distracter to their congruent response (e.g., the word RED is most frequently paired with the color red, and thus, the response "red"). Since high-contingency (congruent) trials are responded to faster than low-contingency (incongruent) trials, *and* consecutive trials with the same contingency level appear to facilitate performance (Schmidt and De Houwer, 2011), it is possible that the CSE in typical 4-AFC tasks is a reflection of contingency-learning rather than of control-based processing adjustments (**Figure 1C**).

At this point, it is worth to already highlight an overarching commonality between the "control-based" and "associative" mechanisms that have been proposed as explanations of the CSE, namely that they share the same ultimate purpose: the reason for (i.e., the evolutionary selection for) binding together stimulus features and actions is of course that this will facilitate fast and appropriate responding to recurring stimuli (in a world where recurring stimuli and events are the norm). In other words, the organism creates memories and tries to match those memories to external stimulation, such that previously experienced events do not have to be processed "from scratch" like novel events. The same basic purpose, but at a more abstract level, is served by the putative control-based mechanisms noted above; they all describe an adaptation of processing strategies to previously experienced (and likely recurring) events, which serves the goal of being prepared for similar challenges in the future—another instance of matching memories (here, of control states) to external stimulation in order to facilitate fast and accurate responses.

THE EMPIRICAL STATUS QUO

The empirical evaluation of control-based and associative sources of the CSE has produced a substantial literature over the last decade, the nuances of which are discussed elsewhere in much greater detail than I aim to provide here (e.g., Egner, 2007; Hazeltine et al., 2011; Schmidt, 2013; Duthoo et al., 2014a; Weissman et al., 2014); instead, the present section summarizes what I consider to be the key take-home messages of that literature. First, there is little doubt that the nature of overlap in stimulus and response features over successive trials can profoundly affect performance (Hommel, 1998) and it is *impossible* to circumvent the confounding factor of differential feature overlap between different congruency sequences in the CSE when employing small stimulus sets (e.g., only two or three different target and distracter stimuli). It is therefore possible, or even likely, that CSEs observed in studies with such small stimulus sets are partly, predominantly, or entirely driven by feature integration effects (e.g., Mayr et al., 2003; Hommel et al., 2004; Nieuwenhuis et al., 2006; Notebaert et al., 2006).

Second, while the movement toward employing larger stimulus sets has resulted in a number of studies reporting CSEs in the absence of feature repetitions (e.g., Ullsperger et al., 2005; Akcay and Hazeltine, 2007, 2011; Hazeltine et al., 2011), almost all of these studies appear to be open to alternative interpretation based on possible contingency-learning confounds because of above-chance occurrence of congruent stimuli (see Schmidt, 2013). Similarly, 2-AFC studies that require subjects to categorize large sets of unique stimuli (e.g., classifying face stimuli according to gender) have produced CSEs in the absence of any stimulus feature repetitions (e.g., Egner and Hirsch, 2005; Egner et al., 2008, 2010; Lee and Cho, 2013), but they have been criticized as being vulnerable to possible feature integration effects operating at the level of semantic categories (like "male" and "female") rather than specific stimulus features (Schmidt, 2013; but see Jiang et al., 2014).

Third, however, a substantial crop of recent papers with designs that specifically control for both feature integration *and* contingency learning confounds have in fact reported robust CSEs (Kunde and Wuhr, 2006; Freitas and Clark, 2014; Hengstler et al., 2014; Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014; but see Mayr et al., 2003; Jimenez and Mendez, 2013). A typical design of this recent wave of studies circumvents both stimulus and response feature repetitions, as well as contingency-learning confounds, by splitting a 4-AFC task into two alternating 2-AFC tasks with non-overlapping stimulus and response sets (e.g., presenting alternately Stroop stimuli that are made up either of red/blue or of green/yellow combinations; e.g., Schmidt and Weissman, 2014). This approach has produced robust evidence for the basic presence of a "memory confound-free" CSE, though in and of itself this does of course not tell us what other, possibly control-based, mechanism is mediating these effects. Current studies are starting to address this question by exploring the precise boundary conditions for obtaining a CSE under these constraints (e.g., Kim and Cho, 2014; Weissman et al., 2014).

For the present purpose, the key conclusion is that there is solid evidence that CSEs can be produced both by sources that operate at a *feature-specific* level, driven by (re-)occurrences of particular physical stimulus and response characteristics, as well as by sources that must operate at a more *abstract* level, producing CSEs in a manner that is independent of repetitions of specific stimulus and response characteristics. How these distinct contributors to the CSE may be conceptualized most fruitfully within a single framework is the subject of the following section.

A MULTI-LEVEL LEARNING PERSPECTIVE ON THE MODULATION OF CONGRUENCY EFFECTS

MEMORY (OBVIOUSLY) FORMS THE BASIS OF ALL SEQUENCE EFFECTS

First, it is worth emphasizing a point that perhaps seems self-evident, but which has often been lost sight of in the long-running debates over different possible causes for the CSE. That point is that *any* time that a past experience (e.g., encountering an incongruent trial) affects current performance, we are, by definition, dealing with an expression of *memory* (or *learning*). This is true regardless of whether the effect be mediated by some kind of "passive carry-over" of within-trial conflict-resolution dynamics (see, e.g., Egner et al., 2010; van den Wildenberg et al., 2012), or whether the prior trial served to explicitly engender expectations regarding the forthcoming congruency level (e.g., Gratton et al., 1992), or any other previously articulated "control-based" mechanism; at the end of the day, these are all means by which past experience changes current behavior, and are thus instances of memory. Unsurprisingly, therefore, whenever researchers have gone to the effort of constructing formal computational models of the CSE, these were grounded in a *reinforcement learning* algorithm (Botvinick et al., 2001; Blais et al., 2007; Verguts and Notebaert, 2008), where experienced conflict essentially acts as a teaching signal for updating the manner in which forthcoming stimuli will be processed (cf. Jiang et al., 2014). Hence, the basic dichotomy implied in labeling CSE accounts "control-" or "attention-based" versus "associative" or "memory-based" can be misleading. Rather than asking, "is it memory or is it attention?" we must ask: "what *type of learning processes* contribute to the CSE?"

To this end, as has already been alluded to in previous sections, I submit that a useful re-conceptualization of the traditional dichotomy between control-based and associative accounts should not focus on the juxtaposition of attention versus memory, but on the *level of abstraction* at which memory or learning effects are being expressed. The feature-integration and contingency-learning accounts deal with learning that links together *concrete* trial characteristics, namely, particular stimulus features (and perhaps categories) and responses; by contrast, the control-based accounts are concerned with learning aspects of a trial that transcend the physical specifics of the stimuli or responses, dealing instead with more *abstract* properties, like congruency, experienced conflict, and/or the cognitive mechanisms that were

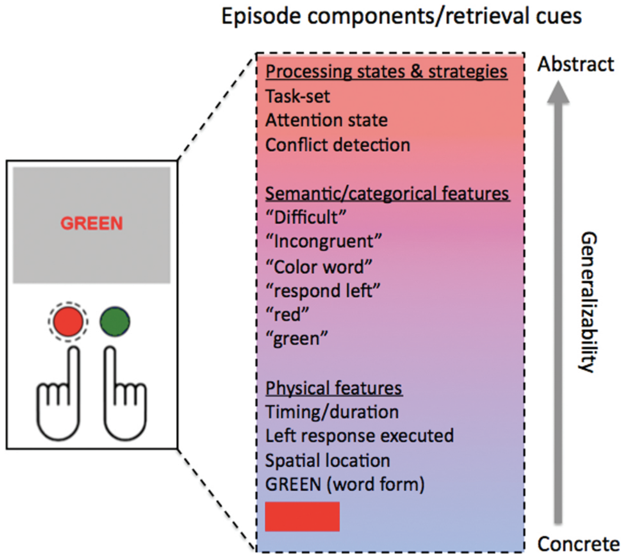

FIGURE 2 | Concrete and abstract features associated with an event or episode file pertaining to a trial on the color-naming Stroop task. The features portrayed here (and possible additional ones) are proposed to be bound together into an "episode file" that will be retrieved when cued by matching features in future trials on the task.

recruited for dealing with the latter (**Figure 2**). As concluded in the brief review of the empirical literature above, there is little doubt that both concrete- and abstract-level learning processes take place and that they are each able to produce CSEs in their own right (as well as presumably contributing to CSEs simultaneously when tasks are not designed to isolate them). Two pertinent questions, then, are what exactly the abstract trial characteristics might be that are being learned, and how we should best conceptualize the relationship between the different levels of learning.

WHAT KIND OF LEARNING MEDIATES CONCRETE-FEATURE INDEPENDENT CSEs?

The "associative" accounts of the CSE explain this effect as a consequence of the binding between concrete stimulus features and responses, which shapes the future processing of similar events. In the absence of concrete feature overlap or biased stimulus–response contingencies, what is it that might be learned or remembered from trial-to-trial that would produce a "control-based" CSE? While the answer is presently not certain, the empirical literature allows us to impose some bounds on possible candidates. First, we can likely reject the notion of a very general effect, whereby exposure to an incongruent trial would lead to broad performance benefits regardless of the specifics of the task-demands on the forthcoming trial. This rejection is based on a large number of studies that have documented the CSE to be *domain-* or *conflict-specific* (e.g., Wendt et al., 2006; Egner et al., 2007; Boy et al., 2010; Funes et al., 2010; Akcay and Hazeltine, 2011; Schlaghecken et al., 2011; Kim et al., 2012; Kunde et al., 2012; for reviews, see Egner, 2008; Braem et al., 2014). Such conflict-specificity is most appropriately assessed in protocols that combine factorially two distinct types of conflict into a single task, such that the independence of conflicts (i.e., additive main effects) and their potential sequential (in)-dependence can be assessed simultaneously, and in the absence of potential task-switching effects when alternating between conflicts in a non-factorial design (for an extended discussion, see Egner, 2008). For example, if one combines the Stroop and Simon tasks, by presenting color-words to the left or right of a central fixation and requiring subjects to respond to ink color using left and right response buttons, one obtains additive Simon and Stroop congruency effects (Simon and Berbaum, 1990; Kornblum, 1994; Jiang and Egner, 2014) and conflict-specific CSEs: Stroop congruency effects are reduced following a Stroop-incongruent stimulus, but not following a Simon-incongruent stimulus, and *vice versa* (Egner et al., 2007). As concluded in a recent, more extensive review of this topic, in studies using designs that assessed the conflict-specificity of CSEs in the absence of potentially confounding task-switch effects (cf. Egner, 2008) there is abundant evidence for such specificity (Braem et al., 2014). Thus, much evidence suggests that abstract learning contributions to the CSE must operate at a relatively "local," trial type-specific level rather than at a global, highly generalizable level.

In this vein, two closely related "hybrid" models of the CSE, alluded to earlier, have argued for a very local, *stimulus-specific* learning process (Blais et al., 2007; Verguts and Notebaert, 2008; see also Blais and Verguts, 2012). For instance, the "adaptation-by-binding" model proposes that the occurrence of conflict triggers an arousal response that enhances the association between top-down attention (task demand units) and the currently activated input units, which would correspond to the *specific stimulus features* of the present incongruent stimulus (Verguts and Notebaert, 2008, 2009). The authors propose that this form of "conflict-modulated Hebbian learning," binding top-down attention to conflict-evoking stimuli[1], can account both for the CSE and a related phenomenon called the "item-specific proportion congruent" (ISPC) effect (Jacoby et al., 2003), where congruency effects are selectively reduced for specific task-irrelevant stimulus features (e.g., the word RED) that are frequently presented as part of incongruent stimuli. The adaptation-by-binding model can indeed easily explain the ISPC effect as reflecting repeated strengthening of connections between task demand units and specific stimulus features (see also Blais et al., 2007). Explaining the (non-specific) CSE, by contrast, is attributed to the model assumption that input units for task-relevant features which are not actually part of the current stimulus are also "slightly activated," such that even non-present stimulus features are held to be subject to conflict-modulated Hebbian learning (Verguts and Notebaert, 2008).

However, this stimulus-specific learning approach appears to be incapable of explaining conflict-specific CSEs in tasks where two conflict types are combined in a factorial design, such that the basic stimulus features do not actually differ between conflict types (see Egner, 2008). Here, the adaptation-by-binding model would predict generalizable benefits of conflict-enhanced binding

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[1] Interestingly, there is in fact growing recent evidence that conflict (or its' resolution) enhances long-term memory for task-relevant stimulus features (target stimuli; Krebs et al., 2013; Mayr et al., 2014; Rosner et al., 2014).

of attention to (especially task-relevant) stimulus features, regardless of which type of conflict elicited the putative arousal response and binding process; this runs counter to empirical findings, however, because even with identical task-relevant features (e.g., ink color), conflict-general CSEs are not observed in such studies (Wendt et al., 2006; Egner et al., 2007; Boy et al., 2010; Akcay and Hazeltine, 2011; Schlaghecken et al., 2011; Kim et al., 2012; Kunde et al., 2012). There are some studies showing CSEs to selectively cross "conflict boundaries" when task-relevant features are shared rather than distinct (e.g., Notebaert and Verguts, 2008), but in these studies, changes between conflict types also represent switches between tasks, such that they cannot directly speak to the conflict-specificity (or lack thereof) of CSEs (see Egner, 2008). These types of findings do suggest, however, that rendering different tasks more similar may promote the likelihood of obtaining cross-task CSEs (Akcay and Hazeltine, 2008; for additional discussion, see Braem et al., 2014). To return to the adaptation-by-binding model though, by conceptualizing conflict as strengthening attentional modulation of *specific input units* (e.g., "red"; Verguts and Notebaert, 2008) rather than of the general task-relevant processing pathway (e.g., "attend to color"), it is also difficult to see how this item-based account can capture other, non-specific effects, such as effects of proportion congruency on completely novel or "unbiased" stimuli, which have been demonstrated both in the domain of the ISPC effect (Bugg et al., 2011) as well as in the related domains of "list-wide" (Bugg and Chanani, 2011) and "context-specific" proportion congruent (CSPC) effects (Crump et al., 2006; Crump and Milliken, 2009; Heinemann et al., 2009; King et al., 2012), which are addressed in more detail below. In sum, while I am sympathetic to the idea of a learning process that directly associates top-down states with bottom-up trial features (see below), accounts that focus on binding attention only to *concrete* stimulus features (Blais et al., 2007; Verguts and Notebaert, 2008) appear to be too narrow to explain a wide array of relevant findings.

From this discussion, we can conclude that the level of (abstract) learning that might mediate CSEs in the absence of concrete feature memory effects can neither be so broad as to cross conflict or task boundaries, nor so narrow as to prevent generalization to novel or unbiased stimuli within the same task or conflict type. As elaborated in the following sections, a parsimonious level of learning therefore would consist of the binding of task- (or conflict-) specific but feature-independent top-down states (e.g., "attend to color," or "suppress responses to location") to contextual cues, which can range from specific stimulus features to temporal episodes.

EVENT FILES THAT BIND CONTROL TO CONTEXT

In order to appreciate the level of abstraction that in my view best captures the type of learning that contributes to feature-independent CSEs (and a range of similar effects), consider the phenomenon of the CSPC effect (for a recent review, see Bugg and Crump, 2012): for example, Crump and Milliken (2009) displayed Stroop stimuli in two different locations, either at the top or bottom of the screen, and unbeknownst to the subjects, the likelihood of incongruent stimuli was high at one location and low at the other (but 0.5 overall). Under this set-up, subjects displayed smaller congruency effects in the location with a high incidence of incongruent trials, suggesting they had learned (implicitly, as it turns out) to associate the two spatial contexts with different attentional requirements or settings. Most importantly, this effect held even for unbiased "transfer" stimuli that had occurred with equal frequency at the two locations. These and similar findings in the context of the ISPC (Bugg et al., 2011), CSPC (Heinemann et al., 2009; King et al., 2012), and "list-wide" proportion congruent effects (Bugg and Chanani, 2011), are all pointing toward the same conclusion: that *generalizable* (i.e., abstract) control states can be bound to contextual cues (location, in the above example). I argue here that the "control-based" CSE represents but one particular instance of the workings of this fundamental associative mechanism that binds external cues to internal states.

One simple and parsimonious way of thinking about the more abstract levels of learning mediating CSEs, therefore, is to extend the idea of event files to encompass not just the forming of associations between concrete stimulus and response features, but also of more abstract, categorical stimulus features and, importantly, the linking of these features with co-occurring internal cognitive states, most pertinently, with the *attentional* or *control state* that was being engaged during the processing of said stimulus event (**Figure 2**). Note that while that control state may in fact be elicited by, and serve the resolution of, "conflict," the idea that internal control settings are being bound to contextual cues is not in any way dependent on conflict being a driving factor; it could equally well be another representation of task difficulty or task requirements that is being bound to, and retrieved in response to, a contextual cue (e.g., Crump and Logan, 2010). In either case, a subsequent cued retrieval of that event file, for example, by one of the stimulus features, would not just prime other associated physical stimulus and response features, but also the retrieval of the associated top-down attentional set which, given its abstract nature (for instance, "highly focused attention on ink color"), can be generalized to novel or unbiased "transfer" items (for similar arguments, see Spape and Hommel, 2008; Crump and Logan, 2010; Hazeltine et al., 2011; King et al., 2012). At the same time, as noted in the discussion of conflict- and task-specificity of CSEs above, the control states that are being incorporated into the event file appear to be constrained in their generality, in that they reflect the particular settings of the current task-set or control process. Accordingly, for example, contextual cueing of the top-down process involved in resolving Stroop conflict, thought to involve primarily the biasing of stimulus processing stages (Kornblum et al., 1990; Egner and Hirsch, 2005; Egner et al., 2007), will be of little use when faced with Simon conflict, which is thought to be incurred and resolved at the response-selection stage (Kornblum et al., 1990; Sturmer et al., 2002; Sturmer and Leuthold, 2003; Egner et al., 2007; Shin et al., 2010), and *vice versa*.

TEMPORAL CONTEXT: MOVING FROM EVENTS TO EPISODES

It is easy to appreciate how this kind of mechanism could account for "unbiased" ISPC and CSPC effects (e.g., Crump and Milliken, 2009; Bugg et al., 2011), where a stimulus or contextual feature, over repeated pairings, becomes associated with a task-appropriate control state. It is less obvious, however, how such context-control binding mechanism would account for a CSE in the absence of any concrete feature overlap (and thus, bottom-up cue) across trials. What if there were no such matching cue or, put another way, what would lead to an appropriate and generalizable control state being engaged *in anticipation* of the arrival of a stimulus? The answer brings us all the way back to the notion of *expectations* or *predictions* which are inherent in both the Gratton et al. (1992) and the Botvinick et al. (2001) explanations of the CSE, but I will here attempt to integrate these ideas within the same framework as the event files: specifically, in order to explain *generalizable* control effects at the trial-by-trial level in the absence of concrete feature repetition, a second conceptual expansion of the event file scheme is required. In particular, while "context" in the empirical studies cited above has been operationalized in the limited terms of concrete stimulus features, such as stimulus location or color, it is fruitful to abstract this notion further, so as to include the concept of a *temporal context*, meaning that the retrieval or priming of particular stimulus–response links and/or control states can be based on a temporally defined frame the organism believes itself to be in (e.g., Braver and Barch, 2002; Koechlin et al., 2003). This suggestion essentially equates to extending the discrete and instantaneous nature of an "event file" to a more dynamic and extended form of an "episode file," which can encode and (upon retrieval) apply temporally extended contingencies and task sets (akin to *schemas*). This broader conception of context would allow for stimuli in the spatial and/or temporal vicinity of the current focus of processing to guide appropriate event/episode file retrieval.

Temporal context can be relatively local (informed primarily by the most recent events) or global (based on a longer sequence of events). In a laboratory setting, a *global* temporal contextual cue would correspond to a particular task phase or block of trials that is predictive of concrete (e.g., color) or abstract (e.g., congruency) stimulus characteristics; "list-wide" proportion congruent effects are one obvious case in point (e.g., Bugg and Chanani, 2011). In real life, episodic frames (e.g., "am I at my own house or at my grandmother's place?") routinely determine the retrieval of extended schemas that include appropriate control states (cf. Miller and Cohen, 2001; Koechlin and Summerfield, 2007). By contrast, the most *local* contextual cue corresponds to the most recently sampled observation (i.e., the previous trial), which serves to update the organism's running estimate of the likely nature of forthcoming events (e.g., Sutton and Barto, 1998). Thus, in the context of the CSE, encountering an incongruent trial sets a context under which the subjective likelihood of a forthcoming trial also being incongruent is enhanced, as is inherent in the accounts of Gratton et al. (1992) and Botvinick et al. (2001). Importantly, the degree to which a given event drives the updating of the organism's belief of the nature of its current temporal context is conditional on the statistics of the environment. In a fast-changing (volatile) environment, local temporal context (e.g., the last trial) should be more influential, whereas in a more stationary (stable) environment, a more global temporal context (e.g., a block of trials) provides a more reliable cue for forthcoming stimulation (Behrens et al., 2007). In line with this proposition, it was recently shown that a model using a volatility-modulated learning rate in its prediction of forthcoming task demands could capture both the CSE as well as a simultaneous proportion congruent effect[2] in a Stroop-like task (Jiang et al., 2014).

According to the current proposal, the local temporal context will prime (or maintain) the activation of the control state (as well as the lower-level stimulus and response features) that characterized the context-updating event (i.e., the previous trial). Therefore, even in the absence of any physical feature overlap across consecutive trials in a conflict task (say, moving from "RED in blue" to "GREEN in yellow"), the temporal context cue provided by an incongruent trial will facilitate the retrieval (or foster the maintenance) of the control state or task-set associated with that trial for a period of time, whose extent likely depends on the temporal statistics of the task environment (Egner et al., 2010). Note again that, as with the suggestion that internal control states can be bound into event files, the idea that a *temporal* context cue can determine the retrieval and temporary application of suitable processing strategies is not wedded to any particular view of what the exact trigger (e.g., conflict) or nature of the control state might be, and it naturally extends beyond the confines of the CSE. Thus, the same logic applies to all manner of local sequence effects, like enhanced response inhibition following stop trials in the stop-signal task (Bissett and Logan, 2012), or the proposal that subjects might adjust decision or response thresholds to resemble response times on a previous trial (Schmidt, 2013). These are all instances of the broader category of associations between temporal context and internal processing strategies.

CREATURES OF HABIT: THE PAST IS (MOST OFTEN) THE FUTURE

Colloquially speaking, the role of temporal context cues as described above corresponds to fostering "expectations," and the notion of the previous trial setting a local temporal context cue could be equated to the idea of "repetition expectancy" (Gratton et al., 1992; Egner, 2007); i.e., subjects, either explicitly or implicitly, expect forthcoming stimulation to resemble that of the recent past. The empirical literature on the role of expectations in the context of the CSE at first blush appears somewhat mixed, in part because the question of whether participants inherently expect successive trials to be similar is easily confused with the (orthogonal) question of whether manipulating expectations for congruent or incongruent trials can modulate congruency effects (e.g., Gratton et al., 1992; Aarts and Roelofs, 2011). Studies that manipulated expectancies by varying the relative probability of encountering different trial type transitions (congruency repetitions versus changes) have sometimes observed a modulation of the CSE by expectations (Duthoo et al., 2013, 2014b; Jimenez and Mendez, 2014) and sometimes not (Duthoo and Notebaert, 2012; Jimenez and Mendez, 2013, 2014). Importantly, though, a lack of expectation effects in these studies was essentially expressed as a failure to override the apparent default tendency to adapt to the previous trial type, i.e., of repetition expectancy (Duthoo and Notebaert, 2012; Jimenez and Mendez, 2013, 2014). In line

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[2] The findings of Jiang et al. (2014) also highlight the fact that observing additive effects of proportion congruent and previous trial congruency factors on performance does not necessarily imply that two independent processes are involved in bringing about these effects (e.g., Torres-Quesada et al., 2014). Both of these (additive) effects can be produced by a single mechanism that adapts its learning rate to the task environment.

with these findings, when participants are probed explicitly about their expectations of the forthcoming congruency, they exhibit a reliable bias toward expecting the present congruency type to be repeated (Duthoo et al., 2013; for equivalent findings in the domain of task-switching, see Duthoo et al., 2012). In sum, it can be argued that there is in fact consistent evidence for an inherent default setting whereby the organism expects a high degree of short-term autocorrelation (stability) in the environment—a *continuity bias*, which here is expressed in the form of an expectation for the congruency (or difficulty, etc.) of the next trial to resemble that of the previous one.

In line with the assumption of repetition expectancy, a pervasive bias for ongoing perception and decision-making to be strongly conditioned by (serially dependent on) the recent past has been well-documented by a number of elegant recent psychophysical studies (Cheadle et al., 2014; Fischer and Whitney, 2014). The idea that, on a moment-by-moment basis, we tend to (implicitly) assume that the immediate future resembles the immediate past, should in fact be unsurprising, as our cognitive apparatus is the result of evolutionary adaptation to a sensory world of high temporal autocorrelation: in the wild, what we see at one moment is a highly reliable predictor (combined with efference copy signals of eye-movements and other motor acts) of the visual input for the next moment (e.g., Dong and Atick, 1995). A particularly efficient way of capitalizing on this fact is to retrieve episodic memories that resemble the present situation, and which therefore allow us to predict the unfolding events ahead of time (for excellent, more extensive treatments of such a "memory-prediction" scheme, see Hawkins and Blakeslee, 2004; Clark, 2013). Anticipating our environment, through the use of episodic contextual cues, allows us to match, as closely as possible, previous experiences to incoming stimulation, and thus to optimize the nature and speed of our responses to our surroundings. As I highlighted above, this represents the shared purpose of low-level and high-level binding processes that link stimuli and contexts to suitable internal states and actions.

SUMMARY

To summarize, the present proposal attempts to integrate distinct "associative" and "control-based" perspectives on the CSE (and related phenomena), by arguing that in the bigger picture these accounts can all be seen as describing complementary levels of learning with a shared goal; specifically, learning to link external stimulation to appropriate internal states (including appropriate action selection). At the most concrete level, this corresponds to associating physical stimulus features with specific motor responses; at a higher level of abstraction it involves binding contextual cues (including temporal frames) to internal attentional states and processing strategies (e.g., task sets). The encoding of an event into memory thus incorporates not only the associative binding of concrete stimulus features and actions, but also of concurrent internal states (including control settings), as well as more abstract external features that create a situational context (location, background, etc.), including an episodic context that places the present experience within a temporally extended reference frame. Translated back to the example of trial sequences in the Stroop task, participants continually encode (or rather, update) memories of event ensembles. As assumed by the feature integration and contingency learning perspectives, these ensembles include associations between concrete, physical stimulus and response features (e.g., "RED," "blue ink," "left index finger response"), as well as more abstract stimulus features (e.g., the categorization of the stimulus as "incongruent" or "difficult"). Importantly, these event ensembles furthermore incorporate contextual information, like stimulus location, both in the spatial and temporal reference frames, and all of these "external" event features become associated with the internal processing states that are engaged when dealing with the stimuli in question, like task- or conflict-specific top-down biasing strategies (e.g., "attend to color")[3]. Retrieval of these internal states, along with the lower-level aspects of each episode file, is triggered by the encounter or prediction of event ensembles of (complete or partial) matching contextual cues, again ranging from physical stimulus characteristics to temporal context.

In this scheme, stimulus–response, categorical, and context-control associations can all occur simultaneously, though the degree to which any type of association is acquired and retrieved to drive task performance will naturally be determined in large part by the statistical structure of the task environment. An attractive proposal in this regard is that the cognitive apparatus is a "miser," attempting to produce appropriate action while exerting the least mental effort possible (e.g., Botvinick, 2007; Kool et al., 2010). (This is of course also the point of forming stimulus–response and context-control links). The brain will therefore exploit correlational task structure any way it can (e.g., Melara and Algom, 2003): if we encounter cues that predict responses directly, we use them to bypass more complex and energetically expensive processing (e.g., in task designs that allow for contingency-learning), thus enhancing speed and saving effort. If stimulus–response learning is rendered impractical but there are cues that predict, for example, trial difficulty, then we use those cues to adapt top-down control settings to task demands. In other words, the acquisition and retrieval of context-control associations may represent something of a "last resort" for the organism (Bugg, 2014). In line with this proposition, it has been shown in proportion congruent manipulations in the Stroop task that when item-specific stimulus–response linkages are relatively frequent or salient, no "list-wide" effects of control are observed (Bugg et al., 2008; Blais and Bunge, 2010), but when direct stimulus–response learning is rendered less efficient, a list-wide control effect can be seen to emerge (Bugg and Chanani, 2011).

OUTLOOK

More formal, empirical and simulation-based testing of the precise determinants of, and relationships between, the putative different levels of learning that I have proposed here will hopefully prove a fruitful endeavor for future investigations. Some particularly important lines of inquiry include the following: first, as emphasized throughout the paper, we do not presently

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[3] This proposal is compatible with the possibility that other internal states will also become associated with the stimulus event, such as affective states, which may bring with them the acquisition of avoidance learning (e.g., Dreisbach and Fischer, 2012).

know what exactly the more abstract trial properties or control processes are that are putatively being incorporated into episodic memory ensembles. Ongoing CSE research with experimental protocols that avoid low-level memory effects so far appears to point in the direction of a response-focused mechanism (Lee and Cho, 2013; Weissman et al., 2014), but the jury is still out on a definitive answer to this question. Secondly, the interplay between the different levels of learning advocated here is presently not well understood. In order to improve on this situation, researchers in this area will need to move from viewing low-level memory effects as "confounds" to incorporating independent manipulations of trial feature repetitions at different levels of abstraction, such that separate as well as potentially interactive contributions between, say, physical feature repetitions and control state repetitions can be assessed. A related question of great interest concerns the manner in which the binding of concrete and abstract episode features occurs at the level of neural mechanisms. The fast-paced (trial-by-trial) CSE should likely involve the type of quick episodic encoding (and retrieval) of trial features typically associated with medial temporal lobe (MTL) structures, including the hippocampus (e.g., Squire, 1992). An intriguing possibility, based on extant literature, is that as the binding process moves from more concrete (physical stimulus) features to more abstract (e.g., attentional state) features, the interaction between the MTL and cortical regions may shift along a posterior to anterior gradient—specifically, interactions with posterior ventral visual stream regions (and motor cortex) for concrete features (e.g., Kühn et al., 2011), the anterior temporal lobe stream for categorical stimulus features (e.g., Kensinger et al., 2003), and lateral prefrontal cortex and posterior parietal cortex in the binding of control-level features (e.g., Egner and Hirsch, 2005; King et al., 2012). A thorough examination of the mechanisms underlying multi-level learning effects that transcend the traditional separation of associative versus control-based cognitive processing should make for an exciting and highly important future avenue of research.

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Sequential modulations of the Simon effect depend on episodic retrieval

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Sequential modulations of conflict effects, like the reduction of the Simon effect after incompatible trials, have been taken to reflect the operation of a proactive control mechanism commonly called conflict monitoring. However, such modulations are often contaminated by episodic effects like priming and stimulus-response feature integration. It has previously been observed that if the episodic representation of a conflicting trial is altered by rotating the stimulus framing 180° around its axis, the subsequent "conflict adaptation" pattern is eliminated. In Experiment 1, we replicate the findings and provide the basic episodic interpretation. In Experiment 2, we extend the framework to include rotations of 90°, and verify that the episodic effects generalize to scenarios of neutral compatibility. Finally, in Experiment 3, we add complete, 360° rotations, and show that the episodic manipulation by itself does not eliminate the conflict adaptation patterns – as long as conditions favor episodic retrieval. The experiments are argued to demonstrate that an episodic account of the conflict adaptation effect can most parsimoniously account for the behavioral effects without relying on higher order cognition. Accordingly, we conclude that conflict adaptation can be understood either as critically depending on episodic retrieval, or alternatively reflecting only episodic retrieval itself.

Keywords: Simon effect, cognitive control, action control, episodic retrieval, S-R compatibility

INTRODUCTION

The time it takes to act is strongly affected by the compatibility between the stimulus and response (Fitts and Seeger, 1953). Simon and Rudell (1967), for example, showed that processing the location of a stimulus automatically triggers a response toward the source of the stimulus, resulting in frequent errors and increased reaction latencies if a stimulus appears in a location opposite to the response. This effect later became known as the *Simon effect* (coined by Hedge and Marsh, 1975), and is one of the more popular effects amongst the range of stimulus-response compatibility phenomena (see for an overview Alluisi and Warm, 1990). Like the Stroop effect (Stroop, 1935), and the flanker-compatibility effect (Eriksen and Eriksen, 1974), the Simon effect follows the general rule that if a task-irrelevant dimension of a stimulus suggests a different response than the relevant dimension, performance will be impaired.

CONFLICT CONTROL

The location of a stimulus may attract a response in the wrong direction, a word's meaning sometimes prompts an incorrect verbalization and a peripheral stimulus can distract by cueing inaccurate actions, yet, overall, we are capable of withstanding temptation and can carry out Simon, Stroop, and Eriksen tasks eventually. Models of cognition typically account for this ability by implementing a function which detects and resolves the conflicting responses using executive or conflict control. A conflicting readiness of the motor cortex can indeed be detected using EEG (Stürmer et al., 2002), which might act as a trigger for the cognitive system to utilize cortical areas associated with cognitive control. Then, the control itself could be achieved by facilitating task-related information, thereby supporting the correct response (e.g., Botvinick et al., 1999; Egner and Hirsch, 2005). Alternatively, incorrectly triggered response alternatives could be actively suppressed, similarly biasing the response competition (e.g., Band et al., 2003; Ridderinkhof et al., 2004). Either approach thus assumes that the cognitive system continuously monitors for conflict and enhances control upon detection.

One of the most important sources of evidence for these conflict-control models is provided by the so-called "Gratton Effect," concerning sequential effects in conflict-inducing tasks (Sheth et al., 2012). This was named after Gratton et al. (1992), who showed that the impact of response-compatible and incompatible flankers on performance is reduced in trials that follow trials with incompatible flankers as compared to trials with compatible flankers. Comparable observations have been made with the Stroop task (e.g., Kerns et al., 2004) and the Simon task (e.g., Praamstra et al., 1999), with either effect being reduced, eliminated and sometimes even reversed after incompatible trials. These observations have been taken as evidence that facing a conflict trial induces an increase of cognitive control, which then proactively facilitates the resolution of conflict in the next trial – resulting in the observed reduction of subsequent conflict effects.

EVENT FILES

Later considerations and findings have, however, raised some doubts on the interpretation of sequential conflict effects as reflecting a universal, conflict monitoring function. As pointed out by Mayr et al. (2003) and Hommel et al. (2004), sequential relationships between compatibility and incompatibility are naturally confounded with particular patterns of stimulus and response repetitions and alternations. Approximately half a century of research on the effect of priming shows that simply repeating a stimulus or response markedly affects reaction times (Bertelson, 1963; Meyer and Schvaneveldt, 1971) and during sequence modulations, such effects are always present. Given that the combinations of stimulus and response repetitions are not equally distributed across the possible transitions between compatibility conditions, it is possible that at least some sequential modulation effects are due to feature – rather than conflict – repetition (Mayr et al., 2003; see also **Figure 1** row 1).

Sequences of stimulus-response combinations do not only invite priming effects. According to Treisman and colleagues, processing the features of an object leads to a binding of the corresponding feature codes (e.g., Treisman, 1996). They found that the priming effect obtained by repeating an object is enhanced if this object also appears in the same location, suggesting that object features get bound to location codes (Kahneman et al., 1992). Hommel (1998) and Hommel et al. (2001) extended this concept to include action and suggested that object features and action features may be spontaneously integrated into what they call *event files*. If, for instance, a stimulus like a cup of coffee is accompanied by an action like grasping or drinking, the codes of the sensory features (BROWN, WARM, etc.) become integrated with action features (moving the hand toward the object, the typical type of grasp, etc.), resulting in an event file of "drinking coffee." If one or more features are encountered again the event file is retrieved automatically (cf., Logan, 1988) in a kind of pattern-completion process. This commonly leads to benefits (e.g., if recognition is hampered by suboptimal visibility), but to costs if some features are repeated but now combined with other features. Thus, perceiving the coffee cup again and immediately retrieving the drinking action may lead to an unpleasant surprise if the contents turned cold in the meantime.

These *partial repetition costs* also come into play during sequential conflict studies. Hommel et al. (2004) showed that partial-overlap costs are commonly confounded with the sequence of compatible and incompatible trials in the Simon task (and other conflict tasks). For instance, if a participant responds with *left* to a right-sided stimulus, the event is incompatible (I), whereas responding *left* to a *left* stimulus is compatible (C). Consider how each of these cases could affect subsequent conflict, such as when a *right* response would be required to a *left* location. In terms of the conflict adaptation effect, a compatible trial followed by

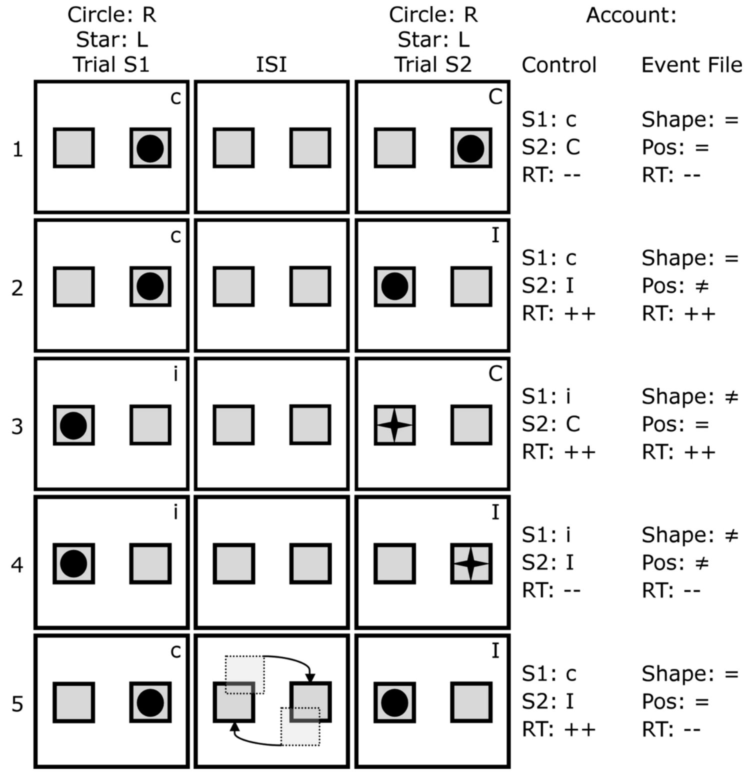

FIGURE 1 | Sequence of events in five trial pairs and coding in terms of conflict (Control account) and features (Event File account). Given that the participant responds right (R) to circles and left (L) to stars, the initially compatible (c) trial (S1) in pair 2 is followed by an incompatible (I) trial (S2), which usually results in increased reaction times (RT: ++). Increased reaction times are also predictable in this scenario from an Event File perspective, as the shape is repeated (=) between trials, but not (≠) the position (Pos). This holds for trial pairs 1, 3, and 4 as well. However, divergent predictions were based on the gradual rotation as is depicted in row 5: whereas nothing changes in terms of conflict, the Event File model would predict performance gains (RT: −−).

an incompatible trial (c-I) results in slow reaction times, but an incompatible trial followed by another incompatible trial (i-I) does not. However, if we deconstruct each sequence in terms of their features, it turns out that in the C-I case (e.g., right| right → left| right), one features is repeated, but the other one is not, resulting in partial repetition costs and slow performance (see **Figure 1** row 2). In the I-I case, either the features both change (left| right → right| left, see **Figure 1** row 4), or the features both repeat (left| right → left| right), so there would be no partial repetition costs and faster performance is predicted. Thus, a "conflict adaptation" pattern is elicited by matters entirely unrelated to the change in conflict itself.

EXCLUDING REPETITIONS IN CONFLICT ADAPTATION

Given that conflict tasks rely on the interrelationship of stimulus and response features, the confound between feature repetitions and the repetitions of compatibility conditions is to some degree unavoidable – at least if the original tasks are left more or less intact. A common workaround solution is to add a layer of complexity to the simple tasks by adding constraints to the randomized design. For instance, some studies have considered only those conditions where no stimulus or response feature is repeated (e.g., Akçay and Hazeltine, 2007) and, given that sequential effects were still obtained, been taken to demonstrate purely executive effects. Even though this approach seems straightforward, it creates two somewhat related problems.

One problem is that excluding any feature overlap between two successive stimuli or stimulus-response episodes does not exclude proactive effects of episodic integration and retrieval (Dutzi and Hommel, 2009). Consider stimuli that vary on two dimensions, such as the visual letters "X" and "O" appearing in red or green. According to the available models of feature integration (e.g., Duncan and Humphreys, 1989), processing a green "X," for instance, would lead to the competition between codes of the colors GREEN and RED and between codes of the shapes X and O. Collecting sufficient visual evidence should provide sufficient support for GREEN and X, which helps them to outcompete the possible alternatives. Now consider that you process the green X after having seen a red O. If having processed the red O led to the integration of the codes RED and O (Kahneman et al., 1992), they can be assumed to act as a unit and engage in what Duncan (1996) and Duncan et al. (1997) called *integrated competition*. This has advantages for the integrated unit if the stimulus it encodes is repeated but a competitive disadvantage if the stimulus changes: having integrated RED and O makes it easier to reject them as a unit (Duncan and Humphreys, 1989). Any loss of RED in the competition with GREEN will propagate to and thus weaken O as well, and losses of O in the competition with X will propagate to and weaken RED. Empirical evidence for this mechanism has been obtained in search tasks, where nontargets can be more easily rejected if they share features that do not overlap with the target, so that they can be grouped together and rejected as a group (Duncan and Humphreys, 1989). Also in line with expectations from an integrated competition approach is the observation that alternating all features and aspects of a stimulus or stimulus-response episode leads to performance that is as good as (Hommel, 1998) and sometimes even better than performance with complete repetitions (Hommel and Colzato, 2004; Colzato et al., 2008). In any case, it seems clear that avoiding feature overlap between successive trials does not allow one to exclude contributions from feature integration and episodic retrieval.

Another problem in restricting analyses to alternation trials is that this amounts to selecting a single data point which ignores all other interactions between repetitions and alternations of features and leaves out the possibility that control and retrieval might interact, as suggested by Spapé and Hommel (2008). In their study, participants responded to high and low-pitched tones by saying "high" and "low," while ignoring voices saying "high" and "low." Unsurprisingly, this created a Stroop-like effect if a word was incompatible with the tone. A typical sequential modulation effect was also obtained, with reduced Stroop effects after incompatible trials. However, if the voice changed between the two successive trials, no such effect remained. Thus, they argued, control information was integrated with the episodic context – i.e., the voice. Only if the episodic context was retrieved did control adaptation affect performance.

Task-switching studies provide support for this interpretation. While switching to a new task is difficult in general (Allport et al., 1994), switching costs are particularly pronounced if the current stimulus was previously encountered in a competing task (e.g., Waszak et al., 2003). This suggests that stimuli and task information are integrated into episodic bindings that are retrieved if the stimulus is encountered another time – which is beneficial if the task has remained the same (as is usual in everyday life) but problematic if the task is different.

Another converging line of evidence comes from task-switching studies that employ a type of conflict task. Evidence from such tasks suggests that a task-switch can result in an elimination of the conflict adaptation effect (Kiesel et al., 2006). In the absence of any similarity between tasks, however, Notebaert and Verguts (2008) observed no sequential effects, suggesting to them that conflict-monitoring acts *locally* (see also Blais et al., 2007).

To summarize, there are reasons to assume that at least some of the effects that are commonly taken to reflect adaptive control actually reflect stimulus-response integration. These effects cannot be avoided by restricting one's analyses to alternation trials.

AIM OF STUDY AND RATIONALE

The aim of the present study was to investigate the relationship between adaptive control processes and episodic retrieval in producing sequential modulations in a conflict task, and to test the hypothesis that the former may depend on the latter. To do this, we used an effect that has before been shown to selectively affect episodic retrieval. We replicate previous findings that demonstrate how this simple, episodic manipulation can have strong effects on conflict control. Here, and throughout the article, we provide a side-by-side comparison of the episodic effect in terms of conflict control and feature integration. We then report two additional experiments which confirm separate predictions that relate episodic retrieval to conflict control. In Experiment 2, we show how feature integration effects can be found even if stimuli are repeated in entirely new positions of neutral compatibility. In Experiment 3, we show that the manipulation itself does not eliminate the conflict adaptation pattern by providing evidence that under conditions that favor episodic retrieval, this pattern can be re-established. Accordingly, each experiment concerns the primacy of episodic retrieval: if the conflict adaptation is determined by episodic retrieval, we should conclude it to either require, or be redundant to, episodic retrieval. But let us first consider the episodic effect under consideration.

Already as part of the first studies on feature integration by Kahneman et al. (1992), it has been shown that if a cue is displayed within a bounding box, a priming effect can be observed if a probe appears inside the same box, even if the box has gradually moved between prime and probe to the new position. Thus, feature integration theory allows that the letters and boxes were bound into enduring representations that were updated along with the boxes' gradual change of position. Simplifying the task considerably, we showed that action features are likewise bound with the object: after a rotation of the stimulus presentation along its axis, a repetition of the action and shape still resulted in performance benefits (Spapé and Hommel, 2010).

Spapé et al. (2011) made use of this effect by transforming the object-reviewing or tracking task into a sequential Simon task. **Figure 1** gives a brief overview of the task and of how the conditions relate to the conflict-control and event-file accounts. If we imagine the task requires a right response for each circle that is portrayed, and a left one for each star, the first row shows a *cI* trial sequence (a compatible initial trial, S1, followed by an Incompatible paired trial, S2) – which typically elicits maximal errors and reaction times. Since the same shape (shape=) is displayed in a new location (pos≠), the event file account likewise predicts poor performance for the same trial sequence. In the second row – and, in fact, in all 8 different combinations of trials (see Hommel et al., 2004) – the same configurations of features cannot distinguish between the two accounts.

However, by gradually having the two boxes exchange positions, as schematically portrayed in the third row, the same predictable effect demonstrated by Spapé and Hommel (2010), should occur. That is, the circle initially displayed right should, following the gradual migration, be represented on the left side, resulting in a complete repetition, which usually results in performance benefits. Meanwhile, there is no reason to assume that the gradual change affects control processes: if registering conflict upon R1 selection suppresses or prevents the processing of stimulus location on S2 presentation, this should not be affected by the presence or absence of a rotation of actually task-irrelevant boxes in between two trials. Therefore, the episodic manipulation of rotating the boxes was not predicted to affect performance from a conflict-control account but only from an event file perspective.

Although neither the boxes, nor the rotation thereof, was relevant to the task or conflict, Spapé et al. (2011) observed strong effects of rotation on conflict adaptation in terms of behavior, as well as event related potentials of the EEG known to be involved in attention and control (the N2) and response readiness (the lateralized readiness potential, LRP): nearly all costs associated with conflict adaptation were removed. However, they did not report the event file analyses, making it difficult to assess whether all predictions from that perspective were fulfilled. Secondly, they also note some curious effects that do not immediately follow from a pure event-file approach. For instance, though they reported that rotation disrupted conflict-adaptation effects, the conflict adaptation effect in terms of psychophysiological indicators did not reverse. That is, in terms of LRP and N2, rotation, the compatibility effect no longer depended on the preceding trial. One could therefore argue that the rotation, rather than causing a change in represented position, induced a *cognitive reset,* undoing both the conflict adaptation *and* the event file.

In order to better understand the effects of rotation on both conflict control and event files, we report in this series of experiments both types of analysis side-by-side. In the *conflict-control* analysis, we examined the data in terms of the sequence of compatibility conditions, testing whether S1 incompatibility would reduce the S2 compatibility effect, and aiming to replicate that rotation, a factor that should be meaningless from a conflict-control point of view, affected the interaction between S1 and S2 compatibility. In the *event-file* analysis, we examined whether response-repetition and stimulus-location would interact in the standard partial repetition-cost pattern: complete repetitions and alternations should result in better performance than repeating one feature, but not the other (Hommel and Colzato, 2004). Of particular theoretical interest here was whether rotation would tend to eliminate these effects (as the two-event-files account of Spapé and Hommel, 2010, would suggest) or even reverse their sign (as a one-file extension of the approach of Kahneman et al., 1992, might imply).

In Experiment 1, we will first demonstrate these two types of analyses in a straight replication study. In the subsequent two experiments, we continue this pattern but extend it to new territories. In Experiment 2, we show the effects of rotating conflicting objects to completely new (in event file terms) and sometimes neutral (in conflict terms) locations by including rotations of 90°. Finally, in Experiment 3, we allow rotations to return to their initial position as well, using a full 360° rotation which completes the picture and confirms that the rotation by itself does not destroy the conflict adaptation effect.

EXPERIMENT 1

In a Simon task, participants respond to a non-spatial stimulus feature by carrying out a left or right response, whilst ignoring the irrelevant location of the stimulus. In our version, participants responded to circles and stars by pressing a left or right key. The stimuli appeared in the left or right of two constantly visible boxes. Trials were presented in pairs, so that one circle or star was presented (S1) and responded to (R1) before a second circle or star (S2) appeared to signal a second response (R2). The boxes remained visible in between the two trials of a pair and were rotated by 180° in 50% of the trials.

METHOD

Participants

Eighteen students from Leiden University voluntarily participated in this experiment in exchange for money or course credits.

Apparatus and stimuli

Stimuli were presented on a flat-screen 17″ CRT monitor in 800 × 600 pixels resolution and a refresh-rate of 120 Hz. A

Pentium-IV dual 1.67 GHz PC running E-Prime (1.1, SP3) on Windows XP SP2 controlled stimulus-presentation and recorded reactions via the USB connected keyboard. The boxes were gray (RGB value of 128, 128, 128), black-lined squares of 60 × 60 pixels or approximately 32 × 32 mm presented against a silver (RGB value of 191, 191, 191) background. The target itself was also 60 × 60 and was either a circle or a four-pointed star. Boxes were presented 180 pixels (approximately 96 mm) left and right from the center of the screen and also kept at this distance during the gradual shifts in location.

Procedure

As outlined in **Figure 2**, a fixation cross was presented for 500 ms, after which the two boxes were presented on the left and right of the screen, one of them containing the target shape (S1) to which participants were required to respond. After 500 ms, the targets were no longer shown on the screen. In the "static" condition, the boxes stood still, without targets, for 800 ms, whereas in the rotating condition, they rotated around their axis at a speed of approximately 4° with each 44 ms. After the 800 ms, both in the static condition and the rotating condition, the boxes were presented for another 200 ms before the second target (S2) was presented. S2 was shown for 700 ms before a screen with feedback informed the participant of the performance. This last screen also comprised the inter-trial interval and was shown for 1100 ms.

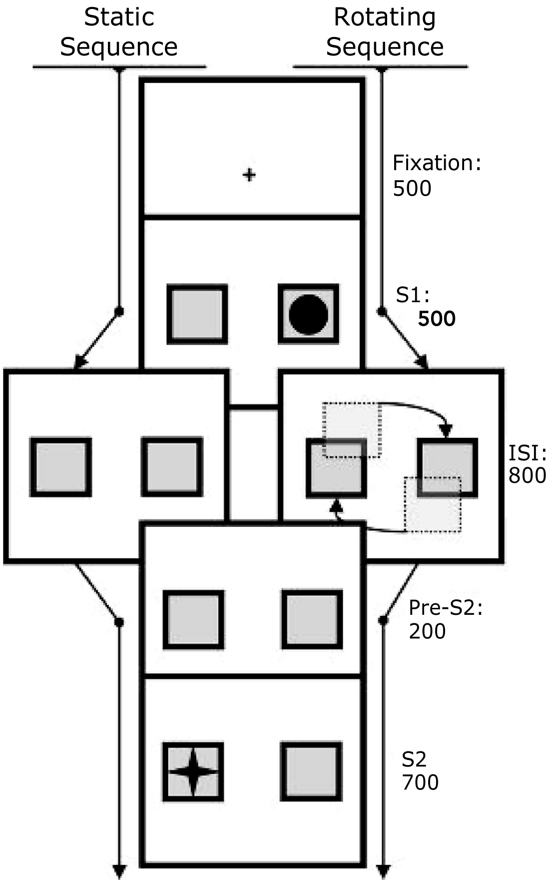

FIGURE 2 | Schematic depiction of the trial-sequence of two example trials. After presenting a fixation crosshair, two boxes were presented for 500 ms in the left and right of the screen, one containing the shape (S1) to which participants were required to respond. In the "static" condition (left), an inter-stimulus interval (ISI) followed in which the boxes stood still for 800 ms, whereas in the rotating condition, they rotated around their axis during this ISI. In both conditions, the boxes were statically presented for another 200 ms before the second target (S2) was shown. S2 was shown for 700 ms before an inter-trial interval of 1100 ms ended the trial.

Following the instruction, the first 20 trials of the experiment were considered practice of acquiring the mapping between circles or stars with a <Q> or <P> keypress. Half of the participants were to press <Q> for stars and <P> for circles, the other half received the opposite stimulus-response mapping. They were required to respond as quickly and accurately as possible and were shown a personal score next to a high score which they were encouraged to break. Points were awarded for responding fast (1 point for each reaction below 600 ms) and accurately (1 point for each accurate reaction). Although there was no monetary or other incentive for breaking the high score, most participants did indicate being positively motivated to aim to break the (fictional, computed as 3 × number of trials) high score. The experimented lasted about 30 min.

Design

Results were coded so as to analyze them with two different three-factor repeated measures designs. First, predictions from the conflict adaptation approach were tested by considering rotation (static versus rotating), compatibility of the first stimulus and response, and compatibility of the second stimulus and response. Second, predictions from the event coding approach were tested by considering rotation, shape/response repetition, and stimulus-location repetition. For both types of analyses, the eight design cells resulting from crossing these factors were replicated an even 32 times. The four blocks consisted of the 64 possible, randomly presented, combinations of rotation (versus non-rotation), direction of rotation (clockwise versus counter-clockwise), target shapes (in S1 and S2) and the location of the targets (in S1 and S2).

RESULTS

Responses (in this as well as Experiments 2 and 3) with latencies longer than 1000 ms were not considered (S1: 2.0%, S2: 1.2%), and all incorrect reactions to S1 or S2 were excluded from RT analyses. Few errors were made during S1 ($M = 6.7\%$, SD = 5.7%) and S2| S1 ($M = 4.3\%$, SD = 2.7%, S2 errors given correct S1).

Conflict-control analysis

In a repeated measures analysis of variance on the RT to S2 with rotation, S1 compatibility and S2 compatibility as factors, reactions were some 30 ms faster after seeing the boxes rotate, $F(1,17) = 52.09$, MSE = 31661.46, $p < 0.001$, while error rates were not affected, $F(1,17) = 2.03$, MSE = 0.01, $p > 0.1$. Compatibility on S1 had no effect on RT, $F(1,17) = 0.08$, MSE = 34.73, $p > 0.7$, but increased accuracy, $F(1,17) = 4.54$, MSE = 0.01, $p < 0.05$. Participants were 30 ms slower if S2 and R2 were incompatible, $F(1,17) = 73.05$, MSE = 32753.76, $p < 0.001$, and made 4% more errors, $F(1,17) = 24.784$, MSE = 0.06, $p < 0.001$. The conflict-adaptation-type effect was replicated, as evidenced by a significant interaction between S1 compatibility and S2 compatibility on RT, $F(1,17) = 25.49$, MSE = 16578.71, $p < 0.001$, and errors, $F(1,17) = 21.11$, MSE = 0.02, $p < 0.001$: the compatibility effect was larger after compatible than after incompatible trials. Indeed, on static trials, the Simon effect was significantly *inverted* after incompatible trials, $t(17) = 2.27$, $p < 0.04$, although not for errors, $t(17) = 0.29$, $p > 0.7$. However, this effect was modulated by rotation in both RTs, $F(1,17) = 40.93$, MSE = 14174.49, $p < 0.001$, and errors, $F(1,17) = 12.63$, MSE = 0.02, $p < 0.005$. To test the effect of conflict-adaptation under static and rotating conditions, two t-tests of the interactions between S1 and S2 compatibility were computed. Whereas the interaction between S1 compatibility and S2 compatibility was very pronounced and reliable with static boxes, RTs: $t(17) = 6.17$, $p < 0.001$, errors: $t(17) = 4.72$, $p < 0.001$, it entirely disappeared with rotating boxes, RTs: $t(17) = 0.50$, $p > 0.6$, errors: $t(17) = 0.59$, $p > 0.5$, see **Table 1A**.

Event-file analysis

Rotation had a comparable effects here, both on RTs, $F(1,17) = 52.39$, MSe = 32831.63, $p < 0.001$, and errors, $F(1,17) = 2.23$, MSe = 0.01, $p > 0.2$. The only other main effect indicated that responses were faster if the shape/response was repeated, $F(1,17) = 18.77$, MSe = 15887.34, $p < 0.001$. As expected (Hommel et al., 2004), stimulus-location repetition interacted significantly with shape/response repetition in RTs, $F(1,17) = 25.34$, MSe = 17916.27, $p < 0.001$, and errors, $F(1,17) = 23.84$, MSe = 0.03, $p < 0.001$. The standard cross-over interaction indicated that performance was better with complete repetitions and alternations than with partial-repetitions (see **Table 1B**). In other words, performance was good if stimulus shape, stimulus location, and the response was repeated or if all three features changed, but comparatively bad if shape and response were repeated while stimulus location alternated or if shape and response alternated while stimulus location repeated. This interaction was further modified by rotation in both RTs, $F(1,17) = 43.47$, MSe = 14077.43, $p < 0.001$, and errors, $F(1,17) = 12.83$, MSe = 0.02, $p < 0.005$. As shown in **Table 1B** and **Figure 3**, partial-repetition costs and, thus, the interaction of location and shape/response repetition) were restricted to static boxes and disappeared with rotating boxes. Interestingly, overlap costs were not negative in the rotation condition.

DISCUSSION

The outcome of Experiment 1 is rather clear-cut. With static boxes, we replicated the earlier finding that the Simon effect is larger after compatible than after incompatible trials (Stürmer et al., 2002). As explained in the introduction, it has often been suggested (cf. Stürmer et al., 2002) that Simon stimuli are processed via two pathways, with the detection of a conflict triggering the gating or blocking of the automatic route (Botvinick et al., 1999). This account would predict less or no Simon effect after incompatible trials: after encountering an incompatible trial, the automatic location-to-response route becomes to some extent tagged as uninformative. However, this account cannot easily explain the *reverse* Simon effect (compatible trials with slower responses) observed after incompatible trials as observed here (and by others, e.g., Hommel et al., 2004). Alternatively, the automatic route may be actively suppressed (e.g., Ridderinkhof, 2002), so that after incompatible trials, it becomes harder to follow any route that coincides with the response that is suggested by the automatic route. This model can therefore account for reduced performance for compatible trials after incompatible trials, resulting in the reverse Simon effect observed here. In any case, our findings for static conditions are consistent with at least some versions of the conflict-control approach.

The rotation conditions show a close replication of Spapé et al. (2011) in terms of their strong effect on the conflict adaptation effect, which remains harder to understand from a conflict-control perspective. Since the boxes themselves are unlikely to cause any conflict, and as they do not even contain any targets while being rotated, there is no reason why moving the boxes should have any effect on conflict-adaptation. Accordingly, we see no way how conflict-monitoring theories can account for our observations. Would the sequential effects have been only smaller (but

Table 1A | Experiment 1, compatibility and conflict-adaptation results.

					Compatibility effect		**Conflict**
S1	**Compatible (c)**		**Incompatible (i)**		**After c**	**After i**	**Adaptation**
S2	**C**	**I**	**C**	**I**	**cI – cC**	**iI – iC**	**(cI – cC) – (iI – iC)**
Reaction times							
Static	448 (9)	515 (15)	489 (13)	473 (11)	66	−16	83
Rotating	432 (10)	469 (12)	436 (12)	470 (11)	37	34	3
Error rates							
Static	2 (1)	11 (2)	4 (1)	3 (1)	9	0	10
Rotating	2 (1)	6 (1)	2 (1)	5 (1)	4	3	1

Reaction times, error rates and standard errors (in parentheses) for S2 (the probe or "current trial") as a function of S1 compatibility, S2 compatibility, and rotation. Effect sizes to the right show the compatibility (Simon) effect and how it is affected by preceding (S1) compatibility. The conflict-adaptation effect is measured as the degree to which the compatibility-effect of S2 is attenuated after incompatible S1s.

Table 1B | Experiment 1, response-priming and Event-file results.

	Location				**Response priming**		**Partial**
	Repeated (R)		**Alternated (A)**		**Loc. R**	**Loc. A**	**Overlap-costs**
Response	**R**	**A**	**R**	**A**	**RA – RR**	**AA – AR**	**(RA-RR) – (AA-AR)**
Reaction times							
Static	451 (9)	519 (15)	487 (14)	470 (12)	68	−16	84
Rotating	441 (7)	460 (13)	446 (10)	459 (13)	18	13	5
Error rates							
Static	2 (1)	8 (2)	7 (2)	3 (1)	5	−5	10
Rotating	3 (1)	4 (1)	4 (1)	4 (1)	1	0	0

Reaction times, error rates and standard errors (in parentheses) for responses to S2 (the probe or "current trial") as a function of rotation, S1-S2 location- and response-repetition. Effect sizes to the right show response-priming effects and how they are modulated by location-repetition. Partial Overlap-costs (the effect of repeating only the location or response between S1 and S2) were measured as the difference between response-priming effects of location-repetitions (Loc. R) and location-alternations (Loc. A).

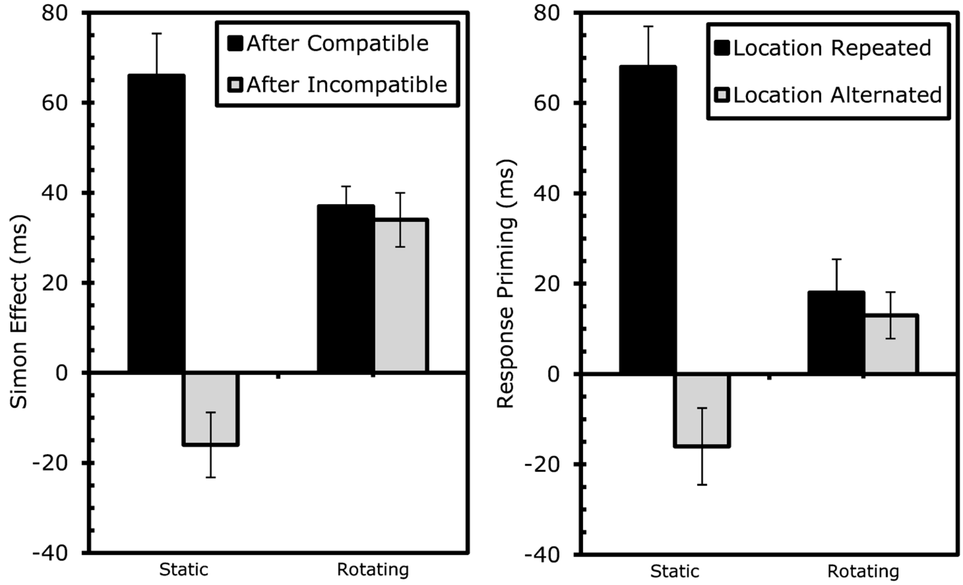

FIGURE 3 | Results Experiment 1: effects of rotation on sequential Simon effects (left) and event-coding (right). Conflict-adaptation was measured as the reduction in Simon effect after incompatibility. Event-coding effects were measured as the decrease in response-priming benefits if the location did not repeat along with the required response. Error bars represent standard error of the Simon (left) or response-priming (right) effect within the specific condition.

not absent) in the rotation condition, one could have argued that they consist of an adaptation component and an independently operating episodic component – with the latter being eliminated and the former being constant. Given the total elimination of the effect, however, this does not seem to be an option.

From an event-file perspective (Hommel et al., 2001; Hommel, 2004) the outcome pattern makes more sense. As predicted, rotating the boxes strongly affected the interactions between stimulus- and response-repetition effects. To the degree that these interactions reflect the creation and later retrieval of feature bindings, this suggests that rotation at least co-determined how the features were coded. There are two hypotheses of how the gradual rotation affects the feature representation. The first would be an extension of Kahneman et al.'s (1992) logic to event files that contain response information and holds that R2 performance is affected by the retrieval of one event file only. In particular, it assumes that rotating the boxes leads to an update of the event file that had just been formed to represent the S1-R1 episode: left stimulus codes are turned into right and right stimulus codes into left codes. If so, the event-file analysis should show regular partial-overlap costs under static conditions, but "negative costs" under rotation conditions, indicating a performance gain if, for instance, the same response is required in the new, updated location.

However, given that this was clearly not the case, the findings may be better understood in terms of our previous suggestion of a two-event-file account (Spapé and Hommel, 2010). This second hypothesis assumes that R2 performance under rotation conditions is affected by two event files simultaneously: one that codes the original S1-R1 episode and another that represents the post-rotation state of affairs. Given that the spatial stimulus codes in these two files are always inconsistent; their effects will tend to cancel each other out. If so, one would expect positive partial-overlap costs under static conditions costs but no overlap costs after rotation. This is exactly the pattern we have obtained, suggesting that the two-file account is more realistic.

To summarize, Experiment 1 clearly replicates Spapé et al. (2011), providing evidence that, at least under the conditions tested here, sequential modulations of Simon effects are entirely due to episodic binding and retrieval. There is one fly in the ointment, however: Although the null effect of event coding in the rotation condition may result from the counteracting effects from two event files, we have no direct evidence that it does. Rather than creating a second, updated file when the boxes move, the cognitive system may simply erase the previous (or any) file whenever a movement or any other dramatic change of the visual display is encountered (Zacks et al., 2007). In the next experiment, we therefore aimed for positive evidence that event files are actually updated and that the updated files affect performance.

EXPERIMENT 2

In Experiment 2 we also rotated the boxes in which stimuli appeared in between S1 and S2 presentation. Two of the three rotation conditions conceptually replicated Experiment 1: A 0° rotation condition corresponded to the static condition of Experiment 1, in which the boxes were not moving, and a 180° rotation condition corresponded to the rotation condition of Experiment 1. The outcomes of these two conditions were expected to replicate the basic finding that conflict-adaptation-type effect should be restricted to the 0° condition and be eliminated in the 180° rotation condition. The more important manipulation, however, was the introduction of a third rotation condition. Here, the boxes were rotated only 90°, so that boxes did not overlap between S1 and S2 displays. S2 could still appear either in the same box (e.g., in the location toward which the box where S1 appeared was rotated) or in the other box (i.e., in the location opposite to the box where S1 appeared was rotated). However, given that the 90° rotation always moved the boxes to locations that were not occupied by the boxes in the S1 display, old and new event files could no longer cancel each other out. Accordingly, their effects should be reliable and more pronounced than in the 180° condition.

METHOD

Twenty-two students from Leiden University between the age of 19 and 25 took part in the study in exchange for money or course credits. The procedure was the same as in Experiment 1, except that S1 and S2 could also appear above and below the screen center, that the boxes could be rotated by 0, 90, or 180°, and that the rotation could take 800 or 1200 ms (a factor that was introduced for reasons unrelated to the present study and that therefore was not considered further in the analyses). The two boxes could thus be either horizontally or vertically oriented in both the S1 and S2 displays, which created four types of transition: horizontal-to-horizontal (H–H) and vertical-to-vertical (V–V), the two 180° conditions, and horizontal-to-vertical (H–V) and vertical-to-horizontal (V–H), the two 90° conditions. The experiment lasted about 40 min.

RESULTS

Trials with incorrect responses to S1 (11.6%) were excluded from the error analyses, and trials with incorrect responses to S1 or S2 (another 11.6%) were excluded from RT analyses.

Conflict-control analysis

The factors were again rotation (rotated vs. static) and compatibility of S1 and S2 (compatible vs. neutral vs. incompatible), where the compatible and incompatible conditions were taken from the horizontal displays and the neutral conditions from the vertical displays. In repeated measures ANOVAs, S1 compatibility approached significance in RTs, $F(2,42) = 3.01$, MSe = 575.99, $p < 0.07$, but not in error proportions, $F(2,42) = 0.79$, MSe = 0.002, $p > 0.7$; while S2 compatibility had a significant effect on both RTs, $F(2,42) = 42.82$, MSe = 20235.00, $p < 0.001$, and errors, $F(2,42) = 48.85$, MSe = 0.21, $p < 0.001$. Participants were 15 ms faster with rotating trials, $F(1,21) = 26.19$, MSe = 13974.68, $p < 0.001$, but not more often correct, $F(1,21) = 1.75$, MSe = 0.01, $p > 0.2$. Rotation modulated the effect of S1 compatibility, $F(2,42) = 4.55$, MSe = 626.31, $p < 0.02$, for RTs, but not errors, $F(2,42) = 0.46$, MSe = 0.001, $p > 0.6$. The standard conflict-adaptation pattern was found for RTs, $F(4,84) = 10.54$, MSe = 2521.80, $p < 0.001$, and errors, $F(4,84) = 8.60$, MSe = 0.03, $p < 0.001$, with larger S2 compatibility effects after compatible than incompatible S1 (effect sizes: 39 ms and 13% as opposed to 12 ms and 2% respectively). As can be seen in **Table 2A** and **Figure 4**, adaptation-type patterns after a neutral S1 were in between (24 ms, 7%). The three-way interaction was also significant in RTs, $F(4,72) = 14.65$, MSe = 3527.93, $p < 0.001$, again showing that rotation eliminated all adaptation-type effects: strong conflict-adaptation was found under static conditions, RTs: $t(21) = 5.57$, $p < 0.001$, errors: $t(21) = 4.59$, $p < 0.001$, but insignificant under rotating conditions, RTs: $t(21) = 1.10$, $p > 0.1$, errors: $t(21) = 0.15$, $p > 0.8$.

Event-file analysis

To establish whether we were able to replicate our findings of Experiment 1, we conducted repeated measures ANOVAs with rotation (rotated vs. static), location-repetition and response-repetition on the conditions where the displays were horizontally aligned and rotated either 180° or not at all. Rotation had a significant effect on RTs, $F(1,21) = 19.65$, MSe = 7459.94, $p < 0.001$, and a marginally significant effect on errors, $F(1,21) = 4.19$, MSe = 0.03, $p < 0.06$, with rotated conditions being 13 ms faster and 2.7% more often correct. Location repetitions were slightly (7 ms) slower, $F(1,21) = 12.33$, MSe = 2404.12, $p < 0.005$, but not less often accurate, $p > 0.6$, than location

Table 2A | Experiment 2, compatibility and conflict-adaptation results.

		S2			Conflict Adaptation	
Rotation	**S1**	**Compatible**	**Incompatible**	**Neutral**	**C – I**	**(cI-cC) – (iI-iC)**
Reaction times						
Static	Compatible	378 (4)	428 (9)	410 (6)	51	
	Incompatible	415 (9)	405 (7)	418 (8)	−10	61
	Neutral	402 (6)	428 (7)	406 (6)	26	
Rotating	Compatible	386 (5)	413 (6)	396 (6)	27	
	Incompatible	379 (5)	412 (6)	398 (6)	33	−6
	Neutral	389 (5)	411 (6)	399 (6)	22	
Error rates						
Static	Compatible	2 (1)	19 (3)	6 (2)	17	
	Incompatible	10 (2)	6 (2)	5 (1)	−4	21
	Neutral	3 (1)	11 (2)	8 (1)	7	
Rotating	Compatible	3 (1)	12 (2)	6 (1)	9	
	Incompatible	4 (1)	12 (2)	5 (1)	8	0
	Neutral	4 (1)	11 (1)	5 (1)	8	

Reaction times, error rates and standard errors (in parentheses) for responses to S2 as a function of S2 compatibility, S1 compatibility, and rotation. Effect sizes to the right show how the compatibility effect is affected by preceding (S1) compatibility (see ***Table 1A****). Note that Neutral S1s indicate trials in which the stimuli were vertically aligned.*

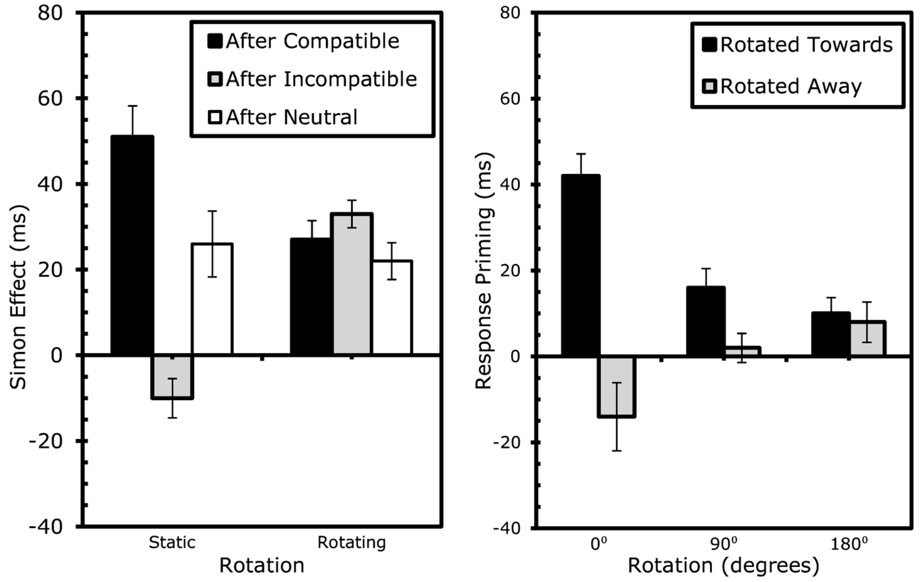

FIGURE 4 | Results Experiment 2: effects of rotation on sequential Simon effects (left) and event-coding (right). Error bars represent standard error of the Simon (left) or response-priming (right) effect within the specific condition.

alternations, whereas response repetitions were significantly faster (10 ms), $F(1,21) = 5.81$, MSe = 4536.24, $p < 0.03$, but not more often accurate, $p > 0.2$, than response alternations. In a similar fashion to Experiment 1, response-repetition interacted significantly with location-repetition for both RTs, $F(1,21) = 42.02$, MSe = 13192.65, $p < 0.001$, and errors, $F(1,21) = 24.17$, MSe = 0.24, $p < 0.001$, the effect of which itself was modulated by rotation for RTs, $F(1,21) = 29.89$, MSe = 13046.56, $p < 0.001$, and errors, $F(1,21) = 16.23$, MSe = 0.16, $p < 0.001$. These findings replicate our observations in Experiment 1 and confirm that they represent a robust pattern.

The next analysis compared the two rotation conditions, which required us to recode the data. We compared trials where S1 location (i.e., the box that contained S1) was *rotated toward* the location of the upcoming S2 (e.g., if S1 appeared in the top box, this box was rotated to the right, and S2 appeared in the right box) with trials where the box holding S1 was *rotated away* from the location where S2 would appear (e.g., if S1 appeared in the top box, this box was rotated to the right, and S2 appeared in the left box). ANOVAs were run with the factors shape/response repetition (repetition vs. alternation), direction of rotation (toward vs. away the location of S2), and degree of rotation (0° vs. 90° – taken from V–H and H–V transitions – vs. 180° – taken from V–V and H–H transitions). Repeated responses were faster, $F(1,21) = 7.28$, MSe = 3347.00, $p < 0.02$, but not more accurate, $F(1,21) = 0.001$, MSe = 0.00, $p > 0.9$. Direction of rotation had no effect on RT, $F(1,21) = 0.09$, MSe = 9.83, $p > 0.7$ or errors, $F(1,21) = 0.01$, MSe = 0.00, $p > 0.9$. Degrees of rotation had no effect on RT, $F(1,21) = 0.10$, MSe = 9.09, $p > 0.7$, but had a small effect on errors, $F(1,21) = 6.32$, MSe = 0.00, $p < 0.03$, with 90° rotations eliciting 1.0% more errors than rotations of 180°. More importantly, shape/response repetition significantly interacted with direction of rotation in RTs, $F(1,21) = 6.76$, MSe = 752.28, $p < 0.02$, and marginally in errors, $F(1,21) = 3.13$, MSe = 0.00, $p < 0.09$. While rotations toward the target location generally resulted in (4 ms) faster, more (0.7%) accurate reactions than with rotations away with repeated shapes/responses, rotations away yielded (5 ms) faster, more (0.6%) accurate responses than rotations toward with alternated shapes/responses. This effect itself, however, was modulated by the degrees of rotation, for both RTs, $F(1,21) = 7.82$, MSe = 436.06, $p < 0.02$, and errors, $F(1,21) = 8.57$, MSe = 0.01, $p < 0.01$. *Post hoc* tests comparing the partial-repetition costs (see **Table 2B** for calculus) for the 90° and 180° revealed that partial-repetition costs were larger in the 90 than in the 180° condition, for both RTs, $t(21) = 2.80$, $p < 0.02$, and errors, $t(21) = 2.93$, $p < 0.01$ (see **Table 2B** and **Figure 4**).

DISCUSSION

The findings of Experiment 2 demonstrate that the degree of rotation matters and that, as expected, the 90° rotation condition produces stronger binding effects. The results of Experiment 1 showed that after rotating the stimulus display for 180°, both conflict-adaptation and partial-repetition costs were reduced to numbers around zero. This was explained as either the result of rotation resulting in two-event-files, or it effectively removing the (memory of the) previous event. Experiment 2 shows that after a 90° rotation, in which S2 appeared at a new location, partial repetition costs increase once again to levels clearly above 0, demonstrating clear episodic effects even after the rotation.

One might argue, however, that the results of Experiment 2, merely show that rotation in and of itself reduces feature-integration, and/or conflict-adaptation. The results of both Experiments 1 and 2 suggested that this may be so, since partial-repetition costs were found to be smaller with each 'extra degree of rotation': from a sizable 80 ms in 0° (i.e., static) conditions, via a smaller but significant 15 ms in 90° conditions to insignificant near-zero in 180° conditions. Thus, one could simply argue that the more the boxes rotate, the lesser be the binding. Likewise, rotation itself could have disrupted conflict adaptation, as after rotating the boxes, no conflict-adaptation was found. If rotation in and of itself eliminates both conflict-adaptation and feature-integration, however, this would predict that neither partial-repetition costs, nor conflict-adaptation should occur after rotating the boxes 360°. In our third experiment, we sought to test this hypothesis.

EXPERIMENT 3

In Experiment 3, the boxes in which stimuli appeared were rotated in similar fashion to Experiment 1, thereby again conceptually replicating two of the three rotation conditions: in one third of the trials, the boxes did not move at all (the static

Table 2B | Experiment 2, response-priming and Event-file results.

	Location / Rotation				Response Priming		Partial
	Toward (R)		Away (A)		Loc. R	Loc. A	Repetition costs
Response	R	A	R	A	RA – RR	AA – AR	(RA-RR) – (AA-AR)
Degrees	**Reaction times (ms)**						
0°	385 (5)	427 (8)	403 (7)	389 (8)	42	−14	56
90°	391 (5)	407 (7)	398 (6)	399 (6)	16	2	15
180°	394 (5)	403 (6)	394 (5)	402 (7)	10	8	2
Degrees	**Error rates (%)**						
0°	2 (1)	20 (3)	10 (2)	4 (1)	18	−6	24
90°	5 (1)	8 (1)	7 (1)	6 (1)	3	−1	4
180°	7 (1)	5 (1)	6 (1)	5 (1)	−2	0	−2

*Reaction times, error rates and standard errors (in parentheses) for S2 as a function of degrees of rotation (0° indicating static conditions), response-repetition and (rotated) location. Note that, different from **Table 1B**, rotating is either "toward" – as with conditions where the box containing the stimulus in S1 gradually rotated toward the location in which S2 was presented – or "away" – under conditions in which the box containing S1 rotated away from the location in which S2 was presented. Thus, with rotations of 0°, rotating toward and away are tantamount to location-repetitions and alternations respectively.*

condition of Experiment 1 or the 0° condition of Experiment 2) and in another third of the trials, they rotated 180°. Crucially for this experiment, however, was the new 360° condition in which the boxes rotated fully around their axis. Thus, if a conflict-inducing stimulus first appeared left, it would rotate to its original location. If rotating itself eliminates conflict-control, no conflict-adaptation was predicted after a 360° rotation. However, if conflict-adaptation would depend on episodic retrieval, significant conflict-adaptation should still be present.

METHOD

Twenty students from Leiden University between the age of 18 and 27 took part in the study in exchange for course credits or money. As in Experiment 1, S1 and S2 could only appear to the left and right of the screen. Also similar to Experiment 1, the boxes in which S1 initially appeared either kept their fixed positions or gradually rotated around their axis during the ISI. Unlike the previous experiments, however, the ISI was either 800 or 1600 ms to examine whether there could be a confounding effect of rotation (in degrees) on rotation-speed (which should be important for tracking, cf. Pylyshyn and Storm, 1988). Two thirds of the trials replicated the static and rotating conditions of Experiment 1 – the boxes rotating 0° or 180° – whereas in the other third, the boxes rotated 360°. The experiment lasted for approximately 50 min.

RESULTS AND DISCUSSION

Trials with incorrect responses to S1 (10.9%) were excluded from the error analyses, and trials with incorrect responses to S1 or S2 (another 9.6%) were excluded from RT analyses.

Conflict-control analysis

In repeated measures ANOVAs with rotation (static vs. 180° vs. 360°), ISI (800 vs. 1600 ms) and compatibility of S1 and S2, S1 compatibility had a significant effect on error proportions, $F(1,19) = 4.50$, MSe = 0.005, $p < 0.05$, but not on RTs, $F(1,19) = 1.42$, MSe = 323.01, $p > 0.2$ whereas S2 compatibility affected both RTs, $F(1,19) = 165.86$, MSe = 125054.10, $p < 0.001$, and errors, $F(1,19) = 35.42$, MSe = 0.57, $p < 0.001$. Rotation had no significant effect on RTs, $F(2,38) = 1.91$, MSe = 1578.36, $p > 0.1$ and only approached significance on errors, $F(2,38) = 2.65$, MSe = 0.02, $p < 0.09$. ISI significantly affected RTs, $F(1,19) = 18.24$, MSe = 10520.22, $p < 0.001$, and errors, $F(1,19) = 29.28$, MSe = 0.09, $p < 0.001$, with longer ISIs being 9 ms faster, but 2.7% more often incorrect. Furthermore, ISI interacted with S2 compatibility on RTs, $F(1,19) = 6.48$, MSe = 1719.27, $p < 0.02$, and errors, $F(1,19) = 5.26$, MSe = 0.04, $p < 0.04$. The effect of S2 compatibility was greater after longer ISIs (36 ms, 8.8%) than after shorter ISIs (28 ms, 5.1%). Rotation interacted with ISI on RTs, $F(2,38) = 4.48$, MSe = 1578.84, $p < 0.02$, but not on errors, $F(2,38) = 0.61$, MSe = 0.004, $p > 0.5$. Also, rotation interacted with S1 compatibility on errors, $F(2,38) = 4.07$, MSe = 0.01, $p < 0.03$, but not on RTs, $F(2,38) = 0.85$, MSe = 92.33, $p > 0.4$, and with S2 on RTs, $F(2,38) = 12.60$, MSe = 3525.34, $p < 0.001$, but not on errors, $F(2,38) = 2.65$, MSe = 0.01, $p > 0.08$.

S1 and S2 compatibility significantly interacted on RTs, $F(1,19) = 147.53$, MSe = 32287.75, $p < 0.001$ and errors, $F(1,19) = 91.36$, MSe = 0.39, $p < 0.001$. Larger S2 compatibility effects were found after compatible than incompatible S1s (50 ms and 12.6% as opposed to 16 ms and 1.2%, respectively). The three-way interaction between rotation, S1 compatibility and S2 compatibility was again significant on RTs, $F(2,38) = 69.07$, MSe = 19484.08, $p < 0.001$ and errors, $F(2,38) = 35.69$, MSe = 0.19, $p < 0.001$, showing rotating had a great effect on conflict-adaption.

To further analyze the effects of rotation on conflict-adaptation, separate ANOVAs testing the degree to which S1 and S2 compatibility significantly interacted were conducted for each type of rotation. This interaction proved significant for static trials on RTs, $F(1,19) = 188.91$, MSe = 68587.80, $p < 0.001$, and errors, $F(1,19) = 81.08$, MSe = 0.74, $p < 0.001$. Again, after rotations of 180°, the conflict adaptation pattern was completely eliminated after rotating the boxes 180° for RTs, $F(1,19) = 0.03$, MSe = 5.14, $p > 0.8$, and errors, $F(1,19) = 0.43$, MSe = 0.001, $p > 0.5$. Finally, a significant interaction was observed for trials in which the boxes rotated 360° for both RTs, $F(1,19) = 12.57$, MSe = 2662.96, $p < 0.003$, and errors, $F(1,19) = 11.82$, MSe = 0.03, $p < 0.003$. An overview of the conflict control effects is provided in **Table 3A** and **Figure 5**.

Event-file analysis

In repeated measures ANOVAs with rotation (static vs. 180° vs. 360°), ISI (800 vs. 1600 ms), location-repetition and response-repetition, rotation had marginally significant effect on RTs, $F(2,38) = 2.62$, MSe = 2226.78, $p < 0.09$ and error proportions, $F(2,38) = 3.11$, MSe = 0.02, $p < 0.06$. Longer ISIs were significantly faster, $F(1,19) = 22.53$, MSe = 12788.14, $p < 0.001$, but also more often incorrect, $F(1,19) = 29.68$, MSe = 0.09, $p < 0.001$. Location repetition was significant for RTs, $F(1,19) = 9.35$, MSe = 1650.65, $p < 0.01$, but not for errors, $F(1,19) = 2.85$, MSe = 0.01, $p > 0.1$. Response repetition was significant for RTs, $F(1,19) = 5.53$, MSe = 9163.96, $p < 0.03$, but only marginally for errors, $F(1,19) = 4.10$, MSe = 0.02, $p < 0.06$. Repeating the response significantly interacted with ISI on RTs, $F(1,19) = 12.89$, MSe = 3474.06, $p < 0.002$, but not errors, $F(1,19) = 0.34$, MSe = 0.0004, $p > 0.5$. ISI interacted significantly with rotation on RTs, $F(1,19) = 4.69$, MSe = 1623.81, $p < 0.02$, but not on errors, $F(1,19) = 0.62$, MSe = 0.004, $p > 0.5$.

More interestingly, we replicated the overall pattern Experiment 1 and 2: location-repetition significantly interacted with response-repetition for RTs, $F(1,19) = 178.94$, MSe = 38853.61, $p < 0.001$ and errors, $F(1,19) = 80.07$, MSe = 0.46, $p < 0.001$. This interaction was modulated significantly by rotation for RTs, $F(1,19) = 65.14$, MSe = 18809.87, $p < 0.001$, and errors, $F(1,19) = 34.41$, MSe = 0.20, $p < 0.001$.

To evaluate whether the cost of partially repeating location or response was dependent on rotation, separate ANOVAs were conducted for each type of rotation (or lack thereof). For static trials, the interaction between repeating location and response was significant for RTs, $F(1,19) = 191.64$, MSe = 72363.01, $p < 0.001$ and errors, $F(1,19) = 75.10$, MSe = 0.80, $p < 0.001$, with partial-repetition costs of approximately 85 ms or 28.3%. As before, with rotations of 180°, the costs were almost non-existent (2 ms or

Table 3A | Experiment 3, compatibility and conflict-adaptation results.

					Compatibility effect		Conflict
S1	**Compatible (c)**		**Incompatible (i)**		**After c**	**After i**	**Adaptation**
S2	**C**	**I**	**C**	**I**	**cI – cC**	**iI – iC**	**(cI – cC) – (iI – iC)**
Reaction times							
Static	375 (7)	438 (7)	413 (8)	393 (6)	63	−20	83
180°	381 (9)	416 (7)	381 (9)	417 (8)	35	36	−1
360°	377 (9)	424 (8)	383 (8)	415 (9)	48	32	16
Error rates							
Static	2 (1)	21 (2)	12 (2)	4 (1)	19	−8	27
180°	3 (1)	12 (2)	4 (1)	12 (2)	9	8	1
360°	3 (1)	13 (1)	5 (1)	9 (1)	10	4	6

*Reaction times, error rates and standard errors (in parentheses) for responses to S2 as a function of S2 compatibility, S1 compatibility, and rotation. Effect sizes to the right show how the compatibility effect is affected by preceding (S1) compatibility (see **Table 1A**).*

2.3%) for RTs, $F(1,19) = 0.37$, MSe = 79.52, $p > 0.5$, or errors, $F(1,19) = 1.55$, MSe = 0.005, $p > 0.2$. However, with rotations of 360°, the costs were once again clearly present (20 ms or 6.5%), for both RTs, $F(1,19) = 19.72$, MSe = 4030.82, $p < 0.001$, and errors, $F(1,19) = 14.24$, MSe = 04, $p < 0.002$. An overview of the event file analysis is provided in **Table 3B** and **Figure 5**.

DISCUSSION

The event-file analysis shows us that rotation in and of itself does not reduce binding cost. In Experiment 2, more degrees of rotation resulted in lower partial-repetition costs; leading to the hypothesis that rotation itself might reduce binding. Experiment 3 falsified this hypothesis: only in the 180° condition, the partial-repetition costs were completely eliminated, whereas in the 360° condition, they were again present.

More importantly, the conflict-control analysis provides evidence that rotation itself does not eliminate conflict-control. If conflict-inducing stimuli rotated back to their original location, a normal – albeit smaller – conflict adaptation pattern emerged. The previous experiments show that there is ample reason for them to be smaller. For one, if the previous location of a stimulus leaves an episodic trace of both where the box *is* and where it *had been* (as suggested by Spapé and Hommel, 2010), instances of the objects could have formed all around their axis. Since the object traveled via the opposite (180 degree) location to its former (360 or 0°) place, an instance of its 180

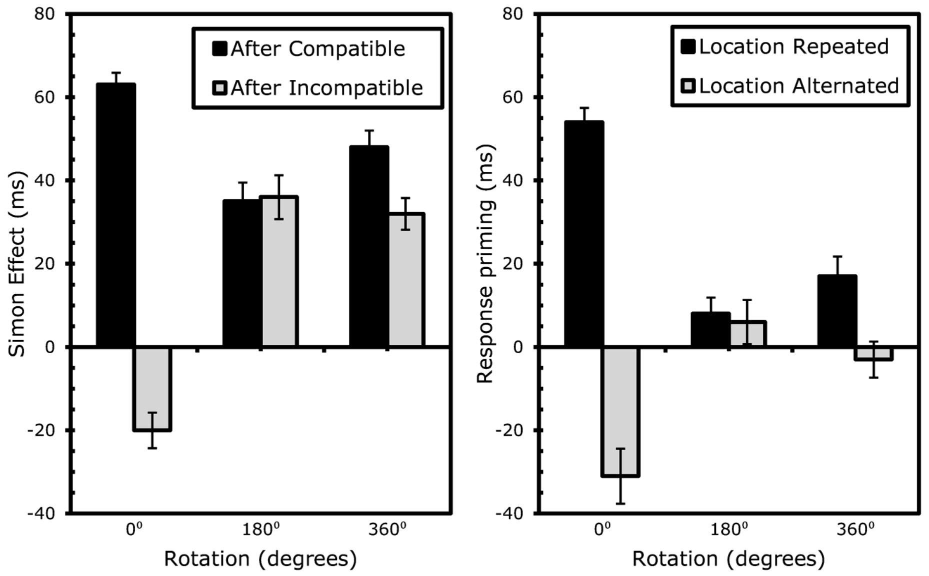

FIGURE 5 | Results Experiment 3: effects of rotation on sequential Simon effects (left) and event-coding (right). Error bars represent standard error of the Simon (left) or response-priming (right) effect within the specific condition.

Table 3B | Experiment 3, response-priming and Event-file results.

	Location				Response priming		Partial
	Repeated (R)		Alternated (A)		Loc. R	Loc. A	Repetition costs
Response	R	A	R	A	RA – RR	AA – AR	(RA-RR) – (AA-AR)
Reaction times							
Static	381 (8)	436 (7)	416 (8)	386 (7)	54	−31	85
180°	395 (8)	404 (9)	393 (8)	398 (9)	8	6	3
360°	390 (9)	407 (9)	400 (7)	398 (9)	17	−3	20
Error rates							
Static	1 (1)	19 (2)	14 (1)	4 (1)	18	−10	28
180°	8 (1)	10 (1)	7 (1)	6 (1)	2	−1	3
360°	6 (1)	8 (1)	10 (1)	6 (1)	2	−4	7

Reaction times, error rates and standard errors (in parentheses) for responses to S2 as a function of rotation, S1-S2 location- and response-repetition. Effect sizes to the right show response-priming effects and partial-repetition costs.

degree position may well have been created. Second, if a participant "lost track" halfway during the rotation – i.e., paying more attention to the fact that the boxes moved as such than where they actually landed – similar patterns as during the 180° conditions would be found. This was clearly not the case. Moreover, the visually rather striking effect of rotation speed did not show clear effects on either conflict adaptation or partial repetition costs.

GENERAL DISCUSSION

Trial-to-trial modulations in response-conflict inducing tasks are commonly taken to reflect adaptive control processes. According to this idea, conflict is registered by conflict-monitoring control process, which then signal the enhancement of the amount of control exerted (Botvinick et al., 2001; Botvinick, 2007). If so, control processes would be more efficient in trials following conflict-inducing trials, a result pattern that has been reported for various sorts of conflict tasks. In keeping with these predictions and previous observations, we were able to replicate the finding that the Simon effect is strongly reduced after incompatible trials (cf. Stürmer et al., 2002; Wühr and Ansorge, 2005). However, this sequential modulation was eliminated altogether by rather simple manipulations of the visual display in between two stimulus presentations. From a control-theoretic view this is unexpected and difficult to explain without additional assumptions, whereas an episodic approach provides a straightforward interpretation of the obtained pattern.

We have suggested that carrying out a response to a stimulus leads to the integration of stimulus and response features (shape, stimulus location, and response location in our case) into an event file that is retrieved if at least one element of the file is repeated (Hommel, 1998, 2004). Following Kahneman et al. (1992), we have assumed that visual conditions that suggest moving an object containing a stimulus to a new location induces the creation of a spatially updated file. The experiments provide evidence that this updated file also contains information about the response, so that the response in a sense travels with its object (Spapé and Hommel, 2010). The experiments also provide evidence that the updated file does not flush or overwrite the previous file, and that both files can affect performance concurrently. In the 180° conditions of all three Experiments, the impact of the two files apparently canceled each other out but when assessed separately, as in the 90° condition of Experiment 2, both could be shown to have an effect.

What do our findings imply for the relationship between adaptive control mechanisms and episodic integration and retrieval effects? We think that two different answers to this question are possible and that it would be premature to decide between them at this point. The radical response would be to consider that all effects that have been assumed to reflect adaptive control mechanisms are artifacts of priming and integration processes (cf. Schmidt, 2013). Indeed, there are more possible effects of that sort than proponents of control approaches have considered, ranging from simple repetition priming (Mayr et al., 2003) over feature integration and the partial-repetition costs they produce (Hommel et al., 2004) to contingency learning (Schmidt and De Houwer, 2011; Mordkoff, 2012) and effects of integrated competition (Duncan, 1996; Dutzi and Hommel, 2009). Even though the basic characteristics of these types of processes are reasonably well understood, it is entirely unclear how they affect performance in the standard conflict tasks and the often rather complicated task versions that have been designed to minimize episodic effects. With respect to the present study, it is fair to say that our event-coding analyses are much easier and straightforward to interpret than the conflict-control analyses, but, more importantly, that the latter are actually not needed to understand the data patterns we obtained. Thus, one might consider the reasoning underlying the conflict-control analyses as unnecessary theoretical overhead.

An alternative, less radical response could consider that adaptive control does take place and can indeed affect subsequent performance, but that the adaptations achieved by the respective control processes are entirely integrated and thus rely on episodic event files (for a somewhat similar suggestion, see Verguts and Notebaert, 2009). For instance, a given file may not only contain pointers to, or associations with codes of stimulus and response features but also information about association weights, that is, about how strongly each given stimulus feature is associated with, or predicts successful responses. There are several observations that are consistent with this scenario. For one, it has been observed that event files are relatively liberal with regard to the precise timing relation between the stimuli and the responses they integrate, as long as the stimuli appear close to response execution (Hommel, 2005). This might suggest that the integration takes place vis-à-vis an evaluation of the response's success and is informed by the outcome of this evaluation.

Consistent with that possibility is the observation that the partial-repetition costs that we attribute to event files are systematically affected by experimental manipulations impacting the current dopamine level: Partial-repetition costs are positively correlated with the spontaneous eye-blink rate, a marker of dopaminergic activity (Colzato et al., 2007a); they increase if stimulus-response pairings are followed by task-irrelevant pictures with positive affective content (Colzato et al., 2007b), stimuli that are suspected to induce a phasic increase of the individual dopamine level (Ashby and Isen, 1999; Cohen et al., 2002); and they decrease in the case of stress, a condition that is known to induce an overproduction of dopamine (Colzato et al., 2008). Given the evidence that phasic changes in the dopamine level are essential for success-controlled learning and stimulus-response integration (Schultz, 2002), these findings fit with the idea that the creation of event files is regulated by success. If we further assume that success triggers the integration of information about all processing aspects that were responsible for it and consider that the cognitive states underlying the efficient handling of response conflicts belong to those aspects, it makes sense to think that event files include control-relevant information. If so, some part of trial-to-trial modulations in conflict tasks may well reflect adaptive control processes and finding that these modulations are in a sense controlled by episodic retrieval does not necessarily imply a contradiction. This idea fits well with later revisions of the conflict-monitoring hypothesis (Botvinick, 2007) that consider conflict as aversive stimuli that operate as teaching signals to avoid using the same selection of associated tasks and strategies. Accordingly, after conflict trials that are accompanied by rewarding stimuli, conflict adaptation is reduced (Van Steenbergen et al., 2009). Indeed, a more adaptive form of cognitive control operation would apply control-relevant information in comparable situations only – that is, in situations that trigger the retrieval of episodic memories related to that situation.

Although this interpretation would be in line with the present results, current theorizing seems to restrict itself to the boundaries of either conflict-control or event-files while their possible interdependency is left to be accounted for. In contrast to Akçay and Hazeltine (2008) or Spapé and Hommel (2008), who found conflict-adaptation to be dependent on the context of the stimulus or the task, others (e.g., Freitas et al., 2007) still found conflict-adaptation even when task-relevant features changed between trials, making the present state of affairs heterogeneous. Rather than arguing that the effects of sequential conflict effects are a by-product of pure stimulus/response-repetition or feature-integration as such, we feel that a framework that focuses on the interplay of control and episodic retrieval could provide the more adequate solution to this puzzle. One of the greater challenges, then, becomes to be able to predict which contextual discontinuities reduce episodic retrieval, thereby disrupting or preventing cognitive control and adaptation. The presented experiments provide several examples of such episodic boundaries of control, and we hope they will inspire future research to focus on re-integrating the fields of executive control with episodic memory.

To conclude, our findings suggest that sequential modulations of conflict effects, the bread-and-butter of adaptive-control approaches, are strongly dependent on episodic retrieval and disappear under conditions that make episodic retrieval unlikely. At a minimum, the findings add to the evidence that demonstrate that sequential modulations cannot be taken to represent process-pure measures of adaptive control (c.f. Hommel et al., 2004; Risko et al., 2008). Possibly, all presented effects may be accounted for entirely in terms of episodic effects. Alternatively, an intriguing compromise could be that control-relevant information is integrated into event files and retrieved only if the current situation is sufficiently similar to the situation in which the event file was originally created.

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Is conflict adaptation triggered by feature repetitions? An unexpected finding

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For decades, cognitive adaptation to response conflict has been considered to be the hallmark of cognitive control. Notwithstanding a vast amount of evidence ruling out low-level interpretations of these findings, disbelief still exists with regard to the underlying cause of the observed effects. Especially when considering cognitive adaptation to unconscious conflict, it is still a matter of debate whether repetitions of features between trials might explain this intriguing finding rather than the involvement of unconscious control. To this purpose, we conducted two masked priming experiments in which four different responses to four different stimuli were required. This allowed us to completely eliminate repetitions of prime and target over consecutive trials. Independent of whether conflicting information was presented clearly visible or almost imperceptible, the results showed an unexpected pattern. Contrary to the regular congruency sequence effect (CSE; i.e., classic Gratton effect), in both experiments the congruency effect *increased* following incongruent trials. Interestingly, this reversed effect completely disappeared when we eliminated all trials with feature repetitions from the analysis. A third experiment, in which feature repetitions were excluded *a priori*, showed a small but regular CSE in the error rates only. Given that feature repetitions are theoretically thought to create a regular CSE, our results are not in line with an interpretation in terms of feature repetitions nor with an interpretation in terms of cognitive control. We conclude that examining cognitive adaptation with or without feature repetitions might be more difficult to conceive than is often suggested in the literature.

Keywords: conflict adaptation, Gratton effect, cognitive control, priming, subliminal, feature repetitions

INTRODUCTION

In the search for the limits and possibilities of unconscious processing, cognitive control processes have been studied extensively. These processes, which make it possible to behave appropriately in constantly changing environments (e.g., Botvinick et al., 2001), are traditionally thought to require consciousness (Dehaene and Naccache, 2001; Jack and Shallice, 2001). To experimentally test this theoretical assumption, conflict tasks have often been used. In these tasks, participants need to respond to relevant stimulus features while ignoring irrelevant features. For example, in the priming paradigm, participants need to identify a target while ignoring a preceding prime. The relation between the prime and the target is manipulated to create conflict and non-conflict trials. On conflict (also termed incongruent) trials, the prime and target trigger a different response while on non-conflict (also termed congruent) trials both prime and target trigger the same response. Responses are typically slower and error rates higher on incongruent compared to congruent trials (i.e., the congruency effect). Interestingly, participants adapt their behavior after encountering conflicting information, leading to a decrease in the congruency effect. In a seminal study, Gratton et al. (1992) observed that the congruency effect is sharply reduced when the previous trial is incongruent compared to congruent. This congruency sequence effect (CSE) is also called the Gratton effect (Gratton et al., 1992). Although the Gratton effect has been studied the most and is a robust finding in different kinds of conflict tasks, it is just one specific kind of CSE. In theory, also other modulations of the congruency effect can potentially occur, and hence be called CSE. Therefore, in the following, we will refer to the Gratton effect as a ‘regular CSE’ and to all other kinds of sequential modulation of the congruency effect as ‘irregular CSE.’ To explain the occurrence of a *regular* CSE, it is typically assumed that if participants experience conflicting response activations, they try to reduce the influence of the conflicting information on subsequent occasions, by focusing more on the relevant stimulus features and/or ignoring irrelevant features (Botvinick et al., 2001). This leads to conflict adaptation: enhanced performance on incongruent trials and reduced performance on congruent trials, leading overall to a reduced congruency effect.

The regular CSE is traditionally considered to be a result of cognitive control processes (Botvinick et al., 2001), which are assumed to require consciousness (Dehaene and Naccache, 2001; Jack and Shallice, 2001). To get a better grasp on the possibility of unconscious cognitive control, researchers have studied whether this regular CSE still occurs when the conflicting information remains unconscious. Results of this approach, however, are not unambiguous (for a review see Desender and Van den Bussche, 2012; Kunde et al., 2012). While some researchers argue that awareness of the irrelevant information (e.g., the prime) is a prerequisite for this effect to occur (Greenwald et al., 1996; Kunde, 2003; Frings and Wentura, 2008; Ansorge et al., 2011), others have found a reliable regular CSE even when the prime remained unconscious (van Gaal et al., 2010; Desender et al., 2013). These latter findings suggest that unconscious cognitive control is possible. However, recently it has been challenged whether performance on conflict tasks can be used as an index of cognitive control processes at all. Unlike the long-held assumption that the regular CSE reflects cognitive adaptation to conflicting information (i.e., *conflict monitoring account*; Botvinick et al., 2001), it has been argued that this effect can be explained by low-level feature repetitions without the need for control processes (i.e., *feature repetitions accounts*; Mayr et al., 2003; Hommel et al., 2004; Schmidt, 2013). Hitherto, several explanations of the regular CSE in terms of feature repetitions have been proposed. For example, according to Mayr et al. (2003), repetition priming effects (e.g., Pashler and Baylis, 1991) underlie the regular CSE. They pointed out that in a two-alternative forced choice task 50% of II (i.e., an incongruent trial followed by an incongruent trial) and CC (i.e., a congruent trial followed by a congruent trial) trials are complete repetitions. CI (i.e., a congruent trial followed by an incongruent trial) and IC (i.e., an incongruent trial followed by a congruent trial) trials, on the other hand, can never be complete repetitions. Thus, the regular CSE can be explained as a superior performance on II and CC trials due to repetition priming effects. In support of this idea, Mayr et al. (2003) observed a regular CSE when all trials were analyzed but this effect was no longer present after removing all target repetitions (i.e., consecutive trials in which the target is the same) from the analysis. In a similar vein, Hommel et al. (2004) claimed that feature repetitions underlie the regular CSE. They argued that stimulus and response features are combined in an event file (i.e., a common episodic memory representation, Hommel, 1998). Whenever one of these two features is the same as in the previous trial, this event file is reactivated, automatically activating both features on the current trial. This is beneficial when both features are indeed repeated on that trial (i.e., complete S-R repetition). On the other hand, when none of the features is repeated (i.e., complete S-R alternations) no event file will be reactivated, thus no wrong activation needs to be suppressed and reaction times (RTs) will also be fast in those cases. However, if only one of both features repeats (i.e., partial S-R repetition) wrong activation needs to be suppressed, which is detrimental for the performance. Given that II and CC trials are always complete repetitions or complete alternations in two-alternative forced choice tasks, responses will be faster and more accurate on these trials compared to CI and IC trials, which are always partial repetitions. According to Hommel et al. (2004), these differences in performance lead to the regular CSE. Although the gist of the arguments in terms of feature repetitions is identical, the specific details slightly differ between these theories. Hence it is important to rule out all possible sorts of repetitions (i.e., complete as well as partial repetitions).

The debate between both interpretations of the regular CSE (i.e., conflict monitoring account versus feature repetitions accounts) has far-reaching consequences for the broad field of cognitive control, given that the effect is often considered as one of the main expressions of cognitive control. Researchers have to bear the alternative interpretations of the regular CSE in mind when investigating cognitive control. Before any conclusions concerning cognitive control processes can be drawn, confounding influences of feature repetitions need to be ruled out. Therefore, it is also crucial to take these alternative explanations into account when studying the assumption that cognitive control processes require consciousness. In the two studies reporting a reliable regular CSE for unconscious primes (van Gaal et al., 2010; Desender et al., 2013) feature repetitions were not sufficiently controlled for. Given that a two-alternative forced choice task was used, repetition effects could not be fully ruled out (Egner, 2007; Mordkoff, 2012) and the observed effects might not reflect a *pure* conflict adaptation effect. In general, the influence of feature repetitions in conflict tasks is still a large matter of debate. Sometimes the effect vanishes after controlling for the confound of repetitions (e.g., Schmidt and De Houwer, 2011), and sometimes the effect remains present (e.g., Kim and Cho, 2014; Schmidt and Weissman, 2014). Still, in none of these studies the regular CSE was investigated in an unconscious condition. It becomes clear that more research is needed in which feature repetitions are controlled for, especially in the field of consciousness.

In this study, three conflict tasks were set-up to thoroughly test both interpretations of the regular CSE, while simultaneously studying the influence of visibility of the conflicting information. We used a priming paradigm with four stimuli and responses. Using a four-alternative instead of a two-alternative forced choice task enabled us to analyze the regular CSE before and after removing all feature repetitions in a masked and unmasked condition. If we would observe a regular CSE when the primes are masked and if that effect would remain present after controlling for feature repetitions, this would be support for the possibility of unconscious cognitive control. However, if the effect would no longer be present after controlling for this bias, low-level processes (i.e., feature repetitions) instead of cognitive control processes would seem to be the underlying cause of the regular CSE.

EXPERIMENT 1

Experiment 1 is an extension of previous work (Desender et al., 2013), with the modification that we used four different stimuli and responses instead of two. In Experiment 1A, participants completed a priming task using Arabic numbers as stimuli. In Experiment 1B, participants performed a Stroop priming task with color words as primes (e.g., "yellow") and colored symbols as targets (e.g., &&&&&; presented in yellow). In these four-alternative forced choice tasks, we could eliminate feature repetitions. This enables us to investigate the contribution of the monitoring of conflict and/or feature repetitions to the regular CSE.

MATERIALS AND METHODS

Participants

Twenty-eight students participated in Experiment 1A. One participant was eliminated because the mean error-rate was above 20% and the mean RT was more than two SDs below the average mean.

Another participant was eliminated because of a technical failure. Thus, the final sample of Experiment 1A consisted of 26 participants (23 females), with an age range of 18–27 years ($M = 19.4$, $SD = 2.0$). 27 students (16 females) participated in Experiment 1B. The participants were between 17 and 22 years old ($M = 18.9$, $SD = 1.3$).

All participants participated in exchange for course credit and had normal or corrected-to-normal vision. Each signed an informed consent before experimentation.

Apparatus and stimuli

Intel Pentium 4 computers with 17-inch LCD screens were used to run the experiment. The refresh rate was set to 60 Hz and stimulus presentation was synchronized with the vertical refresh rate (16.7 ms). For stimulus presentation and data collection E-prime version 1.1. was used. The data were analyzed using SPSS 19. All stimuli were presented on a black background in the center of the screen, using Arial, size 14.

In Experiment 1A, targets were the Arabic numbers "1," "2," "8," and "9." Primes were the Arabic numbers "1," "2," "8," "9," and the neutral prime "X." The forward mask was "#$#" and the backward mask was "$#$." All stimuli were presented in white. Each prime was combined once with a congruent target and once with an incongruent target[1]. The neutral prime was combined with each possible target. As such, four congruent (11, 99, 22, 88), four incongruent (19, 91, 28, 82) and four neutral prime–target combinations (X1, X9, X2, X8) were created.

In Experiment 1B, targets were strings of five colored ampersands. Four colors were used: yellow, blue, green, and red. The Dutch names of the colors [geel (yellow), blauw (blue), groen (green), and rood (red)] were printed in capital letters in gray and used as primes. In this experiment the neutral prime was "££££." The forward mask was "#$#$#" and the backward mask was "$#$#$." Experiment 1B comprised four congruent (YELLOW–yellow, BLUE–blue, GREEN–green, RED–red), four incongruent (YELLOW–blue, BLUE–red, GREEN–yellow, RED–green) and four neutral prime–target combinations (££££-yellow, ££££-blue, ££££-green, ££££-red).

Note that we had two categories of trials (i.e., combinations of 1 and 9 and combinations of 2 and 8) in Experiment 1A while in Experiment 1B the different stimuli were mixed to create incongruent trials.

To fully control for feature repetitions, we consider each trial where the identity of the prime and/or the target is the same as the identity of the prime and/or the target of the previous trial as a repetition (i.e., prime–prime, target–target, prime–target, target–prime).

Procedure

In both experiments, all participants completed a practice block, an experimental block and a posttest to assess prime visibility. All these parts were completed once in the masked condition and once in the unmasked condition.

Each trial started with a forward mask presented for 480 ms, followed by a prime for 33 ms. Afterward, a backward mask appeared for 67 ms in the masked condition, or a blank screen in the unmasked condition. Finally, the target was presented until a response was made. These specific timing parameters were chosen because they proved effective in reducing prime visibility in previous research (Desender et al., 2013). Participants needed to categorize the target as quickly and accurately as possible. In Experiment 1A, participants had to press the corresponding numerical key on the top of a standard qwerty keyboard ("1" with the left middle finger, "2" with the left index finger, "8" with the right index finger and "9" with the right middle finger). In Experiment 1B, participants had to respond by pressing the following keys on a qwerty keyboard: "d" with the left middle finger for yellow ampersands, "f" with the left index finger for blue ampersands, "j" with the right index finger for red ampersands and "k" with the right middle finger for green ampersands. Colored stickers were applied on each of these keys to avoid any confusion. The inter-stimulus interval was set to 1000 ms. In **Figure 1**, an example of an experimental trial is shown.

Participants started the masked condition with eight practice trials, during which no prime was shown. Afterward, they were presented with 360 randomly selected experimental trials with an equal amount of congruent, incongruent and neutral trials. After the experimental trials, participants were informed about the presence of the primes and they then completed a posttest where they had to categorize the prime instead of the target. The posttest comprised 120 trials, identical to the experimental trials with the exclusion of neutral trials. Participants were instructed to

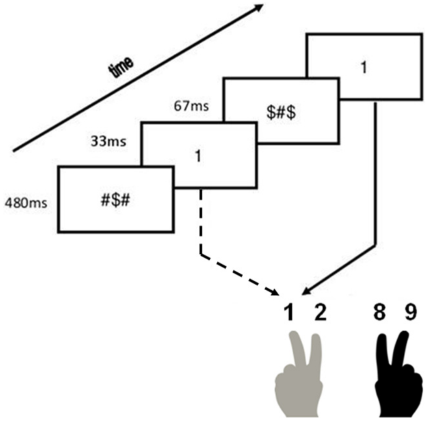

FIGURE 1 | Experimental procedure. Example of a congruent trial in the masked condition.

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[1] This was done to avoid contingency biases. If we would fully cross all possible stimulus combinations, each prime could be combined with three different incongruent targets and only one congruent target. When in addition the congruent and incongruent trials are balanced (i.e., each 50%) in the design, the primes are more often combined with a congruent target than can be expected by chance (i.e., 25% in our design), leading to higher contingency between this prime and target (Schmidt and De Houwer, 2011; Mordkoff, 2012). If participants learn these contingencies, this can lead to faster and more correct responses on high contingency trials relative to low contingency trials (i.e., contingency effect; Schmidt et al., 2007). Moreover, this contingency effect is smaller when the previous trial is a low contingency (or incongruent) trial compared to a high contingency (or congruent) trial (Schmidt et al., 2007). Thus, when contingency biases are not controlled for, the regular CSE could reflect a sequential modulation of the contingency effect.

perform this task at their own pace. Next, participants performed the same three parts in the unmasked condition.

RESULTS

Reaction times above 1000 ms and below 200 ms (2.1% of the data in Experiment 1A; 4.1% in Experiment 1B), trials on which an error was made (4.6% in Experiment 1A; 4.3% in Experiment 1B) and trials following an error (4.9% in Experiment 1A; 4.7% in Experiment 1B) were excluded from further analysis. Mean RTs of correct trials and mean error rates were submitted to a repeated measures ANOVA with current congruency (two levels: congruent or incongruent)[2], previous congruency (two levels: congruent or incongruent) and visibility (two levels: unmasked or masked) as within-subject factors.

Reaction times

In *Experiment 1A*, this analysis showed a main effect of current congruency [$F(1,25) = 196.99$, $p < 0.001$] with faster average RTs on congruent (511 ms) compared to incongruent trials (577 ms). There was an interaction between visibility and current congruency [$F(1,25) = 139.16$, $p < 0.001$], indicating larger congruency effects in the unmasked (111 ms) than the masked condition (22 ms). The interaction between visibility and previous congruency was also significant [$F(1,25) = 4.75$, $p = 0.039$]. The difference in RTs after previous congruent and previous incongruent trials was larger in the unmasked (537 ms versus 544 ms) compared with the masked condition (550 ms versus 547 ms). Crucially, there was an interaction between current congruency and previous congruency [$F(1,25) = 6.78$, $p = 0.015$] which was not modulated by visibility ($F < 1$; see **Figure 2**). Follow-up analyses showed that the congruency effect was always *larger* following an incongruent trial compared to a congruent trial [i.e., *irregular*

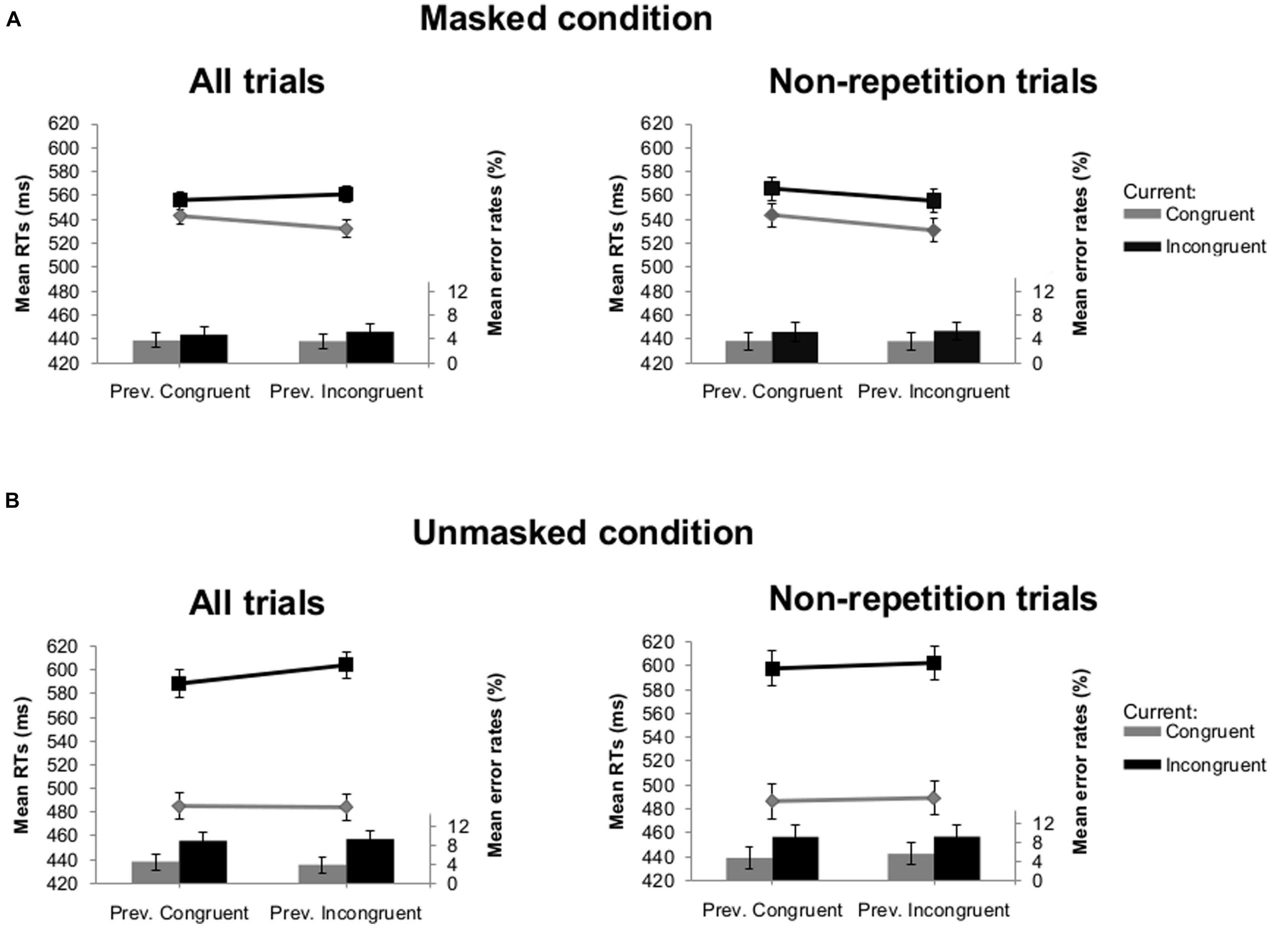

FIGURE 2 | Mean reaction times (RTs; lines) and error rates (bars) as a function of previous and current congruency in Experiment 1A. (A) Results of all trials (left) and non-repetition trials (right) in the masked condition. **(B)** Results of all trials (left) and non-repetition trials (right) in the unmasked condition. Error bars reflect 95% within-subject confidence intervals (see Masson and Loftus, 2003).

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[2] We initially included neutral trials in the experiments to be able to investigate whether the regular CSE was caused by previous incongruent trials, previous congruent trials or both (Desender et al., 2013). Because of the unexpected findings reported below, these analyses were no longer useful. Therefore, we report only the results of the analysis on congruent and incongruent trials. Note, however, that conceptually the same results were obtained when including neutral trials in the analysis.

CSE; 29 ms versus 13 ms, $t(25) = -2.49$, $p = 0.020$ in the masked condition; 120 ms versus 103 ms, $t(25) = -1.73$, $p = 0.097$ in the unmasked condition]. To examine the effects of feature repetitions on this unexpected sequential modulation of the congruency effect, we conducted the same analysis after eliminating all trials where the identity of the prime and/or the target of the current trial was the same as the identity of the prime and/or the target of the previous trial (i.e., 43.3% of all trials). We retained on average 90 (SD = 6.7) trials per participant in the masked condition and 88 (SD = 5.7) in the unmasked condition. Importantly, the crucial interaction between current congruency and previous congruency, indicating a CSE, was no longer significant ($F < 1$; see **Figure 2**).

The results of *Experiment 1B* were in line with Experiment 1A. A similar main effect of current congruency [$F(1,26) = 14.28$, $p = 0.001$] and interaction between visibility and previous congruency [$F(1,26) = 10.02$, $p = 0.004$] was observed. As in Experiment 1A, the crucial interaction between current congruency and previous congruency was significant [$F(1,26) = 91.05$, $p < 0.001$] and not modulated by visibility ($F < 1$). We again always observed that congruency effects were sharply *enhanced* following incongruent trials compared to congruent trials [34 ms versus −24 ms; $t(26) = -8.58$, $p < 0.001$ in the masked condition; 47 ms versus −22 ms; $t(26) = -6.20$, $p < 0.001$ in the unmasked condition; see **Figure 3**]. After removing all possible repetitions [we retained 85 (SD = 7.3) trials in the masked and 80 (SD = 8.6) trials unmasked condition], the interaction between current congruency and previous congruency again was no longer significant ($F < 1$; see **Figure 3**).

Error rates

In *Experiment 1B*, this analysis showed a main effect of visibility [$F(1,25) = 17.66$, $p < 0.001$], with participants making more errors in the unmasked (6.7%) compared to the masked condition (4.3%). We observed a main effect of current congruency [$F(1,25) = 28.52$, $p < 0.001$]: more errors were made on average on incongruent (7.0%) than on congruent (4.0%) trials. There was also an interaction between visibility and current congruency [$F(1,25) = 10.83$, $p = 0.003$], reflecting the fact that the congruency effect was more prominent for the unmasked (4.8%) than for the masked condition (1.3%). Crucially, there was no interaction between current and previous congruency ($F < 1$; see **Figure 2**). None of the other effects reached significance. After removing all possible repetitions, the interaction between current congruency and previous congruency was also not significant ($F < 1$, see **Figure 2**).

For *Experiment 1B* there was an interaction between visibility and current congruency [$F(1,26) = 14.36$, $p = 0.001$]. The congruency effect was larger in the unmasked (4.8%) compared to the masked condition (1.3%). There was no interaction between current and previous congruency [$F(1,26) = 1.87$, $p = 0.18$]. None of the other effects reached significance. After removing all possible repetitions, the analysis showed no interaction between current and previous congruency [$F(1,26) = 2.19$, $p = 0.15$; see **Figure 3**].

Prime visibility

Data of the *masked condition* showed that participants correctly categorized primes in 33% of the posttest trials in Experiment 1A and in 38% of the posttest trials in Experiment 1B. This is above chance level performance [i.e., 25%; $t(25) = 4.78$, $p < 0.001$

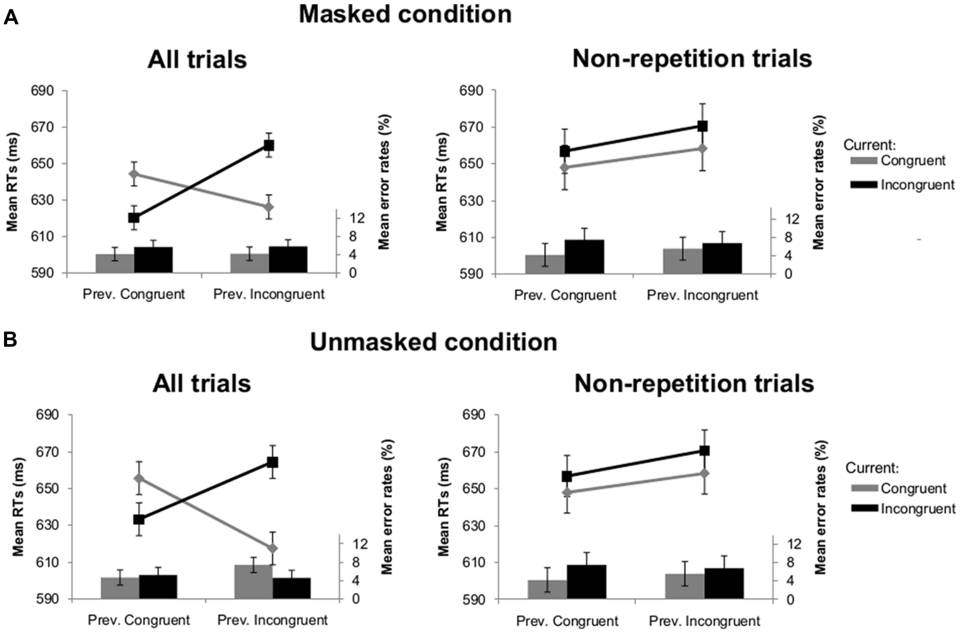

FIGURE 3 | Mean RTs (lines) and error rates (bars) as a function of previous and current congruency in Experiment 1B. (A) Results of all trials (left) and non-repetition trials (right) in the masked condition. **(B)** Results of all trials (left) and non-repetition trials (right) in the unmasked condition. Error bars reflect 95% within-subject confidence intervals (see Masson and Loftus, 2003).

in Experiment 1A; $t(26) = 5.99$, $p < 0.001$ in Experiment 1B]. Non-significant correlations were found between the individual visibility measure and our index of the CSE ($r = -0.12$, $p = 0.55$ in Experiment 1A; $r = -0.08$, $p = 0.70$ in Experiment 1B). In the *unmasked condition*, the average proportion of correctly categorized primes (77% in both experiments) was clearly above chance level [$t(25) = 17.34$, $p < 0.001$ in Experiment 1A; $t(26) = 11.84$, $p < 0.001$ in Experiment 1B]. Importantly, the visibility in the unmasked condition was significantly higher than in the masked condition [$t(25) = -14.00$, $p < 0.001$ in Experiment 1A; $t(26) = -8.58$, $p < 0.001$ in Experiment 1B].

DISCUSSION

In Experiment 1 we examined whether the regular CSE (i.e., the Gratton effect) is caused by the monitoring of conflict (Botvinick et al., 2001) or rather by the presence of feature repetitions (Mayr et al., 2003; Hommel et al., 2004). To be able to test both accounts, we used a masked priming paradigm with four stimuli and four responses. By increasing the amount of stimuli and responses, we were able to compare the regular CSE in all trials with the effect in non-repetition trials (i.e., the identity of the prime and/or the target of the previous trial was not repeated on the current trial). Although we expected a regular CSE (i.e., *reduced* congruency effect following an incongruent trial compared to a congruent trial) we observed an opposite pattern in the responses (i.e., *increased* congruency effect following an incongruent trial compared to a congruent trial) when analyzing all trials. This unexpected and irregular CSE cannot be accounted for by the conflict monitoring theory (Botvinick et al., 2001). According to this account, the sequential modulation of the congruency effect is a consequence of an increase in cognitive control following conflict. However, from this perspective it would be hard to explain the current increase of the congruency effect following conflict. Furthermore, this effect disappeared completely when analyzing non-repetition trials only. Thus, as predicted by the feature repetitions account and in line with the results of Mayr et al. (2003), we observed no regular CSE when all feature repetitions were removed. This supports the idea that feature repetitions influence the sequential modulation of the congruency effect. However, given that our full dataset showed an irregular CSE, which is also not predicted by this latter account, the absence of a regular CSE after removing all possible feature repetitions is no convincing evidence in support of the feature repetitions account either. We can only conclude with certainty that feature repetitions have an impact on this irregular CSE. Hence, examining cognitive adaptation with or without feature repetitions might be more difficult to conceive than is often suggested in the literature. Neither the interpretation of the regular CSE in terms of repetition priming effects (Mayr et al., 2003), nor the interpretation in terms of feature integration (Hommel et al., 2004) can explain the irregular CSE that we observed. According to both interpretations, specific feature repetitions lead to a faster reaction on II and CC trials, respectively, compared to CI and IC trials. This difference in RTs is considered to be the underlying source of the *regular* CSE. However, this selective benefit for II and CC trials was not present in our results. In contrast, we found the exact opposite pattern (i.e., slower reaction on II and CC trials, respectively, compared to CI and IC trials). As Mordkoff (2012) pointed out, there are different sorts of repetition trials (i.e., complete repetition, partial repetition in which the relevant or irrelevant information repeats, negative priming repetitions in which the relevant information of the previous trial becomes the relevant information on the current trial) and the removal of all these trials affect the different trial types (i.e., II, CC, IC, CI) at varying degrees. Hence, when repetition trials are included in the design, the proportion of repetition and non-repetition trials differs for each trial type. These varying influences of feature repetitions on each trial type can lead to complex interactions affecting the overall pattern of responses. In conclusion, the observed irregular CSE might be triggered by these complex influences of feature repetitions. Therefore it seems necessary for future studies to investigate the regular CSE in a more 'clean' design where feature repetitions are excluded beforehand. Such a straightforward approach seems even more indispensable when considering that studies differ in which specific feature repetitions are removed, making these studies hard to compare (Notebaert and Verguts, 2007). In some recent studies feature repetitions were already excluded *a priori* in the design (Kim and Cho, 2014; Schmidt and Weissman, 2014). We also used this approach in our second experiment in order to investigate whether the regular CSE can be observed when feature repetitions confounds are completely controlled for by excluding them by design.

EXPERIMENT 2

In our first experiment, we limited the analysis to non-repetition trials to investigate the effects of feature repetitions. Although this is a widely applied approach (e.g., Mayr et al., 2003; Kerns et al., 2004; Ullsperger et al., 2005; Fernandez-Duque and Knight, 2008), it has its limitations. As previously discussed, even though repetition trials are removed from the analysis, the presence of these trials during the experiment could still have an overall influence on the response tendencies of the participants. To preclude every plausible influence of feature repetitions, we set up a second experiment in which all possible repetitions (i.e., prime–prime, target–target, prime–target, target–prime) were excluded beforehand. In Experiment 2 we used the same design as in Experiment 1A, but in this case a trial from one category (e.g., 1–9) was always followed by a trial from the other category (e.g., 2–8). As such, the prime and/or the target were never repeated in two consecutive trials.

MATERIALS AND METHODS

Participants

Twenty-one students (eight females) participated in exchange for course credit. They provided written informed consent before experimentation. One participant was eliminated because the mean error-rate was above 20% and the mean RT was more than two SDs below the average mean. Another participant was eliminated because of a technical failure. Thus, the final sample consisted of 19 participants (seven females), with an age range of 18–25 years ($M = 20.0$, $SD = 2.1$). All participants had normal or corrected-to-normal vision.

Apparatus and stimuli

Apparatus and stimuli were the same as in Experiment 1. The possible prime–target combinations were the same as in Experiment 1A, thus for the congruent and incongruent trials we had two categories of trials (i.e., combinations of 1 and 9 and combinations of 2 and 8). To create neutral trials, the neutral prime "X" was combined with each possible target.

Procedure

In general, the same experimental procedure was used as in Experiment 1A (see **Figure 1**). We did change the amount of trials in the different parts of the experiment. In Experiment 2, the participants were presented with eight practice trials, 384 experimental trials and 80 trials in the posttest twice (i.e., once in the masked and once in the unmasked condition). To avoid feature repetitions *a priori*, we also changed the sequence of the trials. A trial of one category was always followed by a trial of the other category. Participants had to respond by pressing the corresponding button on a Cedrus response box (type RB-840).

RESULTS

RTs above 1000 ms and below 200 ms (2.0% of the data), trials on which an error was made (4.4%) and trials following an error (5.3%) were excluded from further analysis. Mean RTs of correct trials and mean error rates were submitted to a repeated measures ANOVA with the same within-subject factors as in Experiment 1.

Reaction times

This analysis showed a main effect of current congruency [$F(1,18) = 69.76$, $p < 0.001$], indicating that participants responded slower to incongruent (541 ms) compared to congruent trials (487 ms). The interaction between visibility and current congruency was significant [$F(1,18) = 67.26$, $p < 0.001$]; the congruency effect was smaller in the masked (20 ms) than the unmasked condition (89 ms). Crucially, the interaction between current and previous congruency did not reach significance ($F < 1$), indicating the absence of a regular CSE (see **Figure 4**). None of the other effects reached significance.

Error rates

As in the RT-analysis, there was a significant main effect of current congruency [$F(1,18) = 7.80$, $p = 0.012$] with more errors being made on incongruent (7.5%) compared to congruent trials (3.8%). There was also a significant main effect of previous congruency [$F(1,18) = 4.44$, $p = 0.049$]: the error rates on the current trial were higher when the previous trial was congruent (6.2%) compared to incongruent (5.0%). There was a significant interaction between visibility and current congruency [$F(1,18) = 5.59$, $p = 0.030$], reflecting that congruency effects were larger in the unmasked condition (5.1%) than in the masked condition (2.2%). The crucial interaction between current congruency and previous congruency was not significant [$F(1,18) = 1.80$, $p = 0.197$]. However, the three-way interaction was significant [$F(1,18) = 5.66$, $p = 0.029$]. Separate ANOVAs indicated that the interaction between current and previous congruency was not significant in the masked condition ($F < 1$; see **Figure 4**), but was significant in the unmasked condition [$F(1,18) = 4.83$, $p = 0.041$; see **Figure 4**].

Prime visibility

To assess prime visibility, we analyzed the average proportion of correctly categorized primes in the posttest. In the *masked condition*, participants correctly categorized primes on 34% of the trials, which is above chance level performance [i.e., 25%; $t(18) = 2.83$, $p = 0.011$]. In the *unmasked condition*, the average proportion of correctly categorized primes (88%) was clearly above chance level [$t(18) = 19.84$, $p < 0.001$]. Importantly, the visibility in the masked condition differed significantly from the visibility in the unmasked condition [$t(18) = -19.47$, $p < 0.001$].

GENERAL DISCUSSION

In spite of the large amount of research, it is still unclear whether and how cognitive control and consciousness are linked. Many researchers used conflict tasks and analyzed the presence of the regular CSE (i.e., the Gratton effect) in conditions differing in stimulus visibility. However, the results of these studies are not unequivocal (for a review see Desender and Van den Bussche, 2012; Kunde et al., 2012), making it hard to draw strong conclusions on the consciousness-control link. In addition, some researchers argue that the regular CSE does not truly reflect cognitive control, making such conclusions even harder. These researchers suggested that the regular CSE is a consequence of feature repetitions (Mayr et al., 2003; Hommel et al., 2004) rather than cognitive control

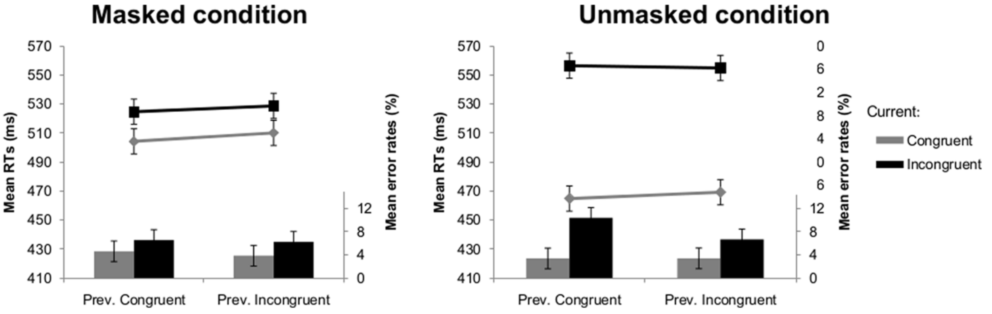

FIGURE 4 | Mean RTs (lines) and error rates (bars) as a function of previous and current congruency in Experiment 2 in the masked (left) and unmasked (right) condition. Error bars reflect 95% within-subject confidence intervals (see Masson and Loftus, 2003).

(Botvinick et al., 2001). We designed three priming experiments to investigate whether the regular CSE is a consequence of cognitive control, and if so, whether this effect also occurs when the conflicting information is masked.

DOES THE GRATTON EFFECT REFLECT COGNITIVE CONTROL?

When restricting the analysis to non-repetition trials, we observed no regular CSE in Experiment 1. As such, our results seem to corroborate previous findings (Schmidt and De Houwer, 2011; Mordkoff, 2012) in support of the feature repetitions account. However, when all trials were analyzed, we did not observe a regular CSE either but a CSE characterized by an opposite pattern of responses. This makes an interpretation of the regular CSE in terms of feature repetitions not appropriate based on our data. We can only conclude that feature repetitions seem to influence the sequential modulation of the congruency effect in various ways. As discussed before, the *post hoc* removal of feature repetitions is not without problems. To overcome these limitations, feature repetitions were precluded beforehand in some recent studies (Kim and Cho, 2014; Schmidt and Weissman, 2014). In both studies, a reliable regular CSE was found. Like in our study, Schmidt and Weissman (2014) used a priming paradigm, however, with clearly visible primes only. To the best of our knowledge, we are the first to study the regular CSE in both a masked and unmasked condition while controlling for feature repetition confounds. Our analysis of the error rates of Experiment 2 revealed the presence of a regular CSE in the unmasked condition. Although we have to be cautious in interpreting this small effect, it suggests that cognitive control can trigger a regular CSE when the conflicting information is consciously perceived. In contrast to some previous studies (van Gaal et al., 2010; Desender et al., 2013), our study failed to provide evidence for conflict adaptation when the conflicting information is masked. However, it is possible that the difference between both conditions may be caused by a difference in the size of the congruency effect rather than a difference in awareness. In the masked condition the congruency effect was smaller, therefore the amount of conflict might be too small to induce adaptation processes. Furthermore, given that we only observed a small effect, more research is needed to investigate whether our results can be replicated.

Right now, we conclude that the effects of feature repetitions might be more complex than previously suggested and that a 'clean' design is needed to examine both competing accounts concerning the regular CSE. Additionally, based on our second experiment we cautiously conclude that there might be some evidence for a regular CSE triggered by cognitive control if the conflicting information is presented clearly visible. This questions the idea that the regular CSE can solely be explained by feature repetitions. Egner (2007) already suggested that neither account can fully explain all the results that are found by different researchers. He points to the possibility of a combination of different processes underlying the regular CSE.

AN UNEXPECTED FINDING: AN INCREASED CONGRUENCY EFFECT FOLLOWING CONFLICT

In Experiments 1A and 1B, we found an increased congruency effect following an incongruent trial compared to a congruent trial when including all trials in the analysis. As discussed before, we did not anticipate this irregular CSE and neither the conflict monitoring account (Botvinick et al., 2001) nor the feature repetitions accounts (Mayr et al., 2003; Hommel et al., 2004) can fully explain this reversed effect.

Although this irregular CSE seems highly remarkable, a closer look at the literature shows that at least in some studies this effect was also observed (Fischer et al., 2008; Jiang et al., 2013). Fischer et al. (2008) observed a reversed adaptation pattern when comparing adaptation to different kinds of conflict. Furthermore, in a recent study using an affective priming paradigm with masked and unmasked primes, Jiang et al. (2013) compared the regular CSE on response alternation versus response repetition trials. When the response of two consecutive trials was repeated, they observed the same irregular CSE as we did. When the response alternated, they observed a regular CSE (in the conscious condition only). They proposed that the irregular CSE was a consequence of response priming rather than emotional conflict, given that this effect only occurred when analyzing the trials in which the response was repeated. However, as suggested by Mayr et al. (2003), response priming should result in a *regular* CSE, and not a reversed pattern as Jiang et al. (2013) observed. In contrast to the results of Jiang et al. (2013), we observed no regular CSE when analyzing non-repetition trials in the current study. However, we observed an irregular CSE in all trials. Given that response repetition as well as response alternation trials are included in these analysis, this irregular CSE cannot be exclusively attributed to response priming, challenging the conclusion of Jiang et al. (2013). Further research, where stimulus and response repetitions are properly separated, is needed to understand these conflicting results.

Apart from these isolated studies, the irregular CSE that we observed has also been observed in studies in which feature repetitions are avoided by using two different tasks and/or responses between which participants have to switch. For example, Verguts and Notebaert (2008) found evidence for this reversed pattern of responses when the task switches between two trials. In another study participants had to switch between a vertical and horizontal Simon task (Braem et al., 2011). Braem et al. (2011) observed a regular CSE when participants had to respond to both the vertical as well as the horizontal Simon task by pressing buttons with their two hands. However, when they had to respond to one task with their hands and to the other with their feet (i.e., two different response modalities), they observed the same irregular CSE that we did when the response modality switched. The results of both studies are explained by an interpretation of the regular CSE in terms of associative learning (Verguts and Notebaert, 2008, 2009). When participants encounter conflict (i.e., incongruent trial), this is assumed to result in a strengthening of the association between the task-relevant units, which leads to reduced influence of irrelevant information on the following trial (Verguts and Notebaert, 2008, 2009). When stimulus-response associations of task 1 are enhanced as a consequence of conflict, the other stimulus-response associations (including those of task 2) are weakened. Thus, there is less attention for the relevant information of task 2 following an incongruent trial of task 1, leading to the reversed pattern of responses. In Experiment 1A of the current study, a prime from one category (e.g., 1–9) was only combined with a target from the same category. Therefore, the irregular CSE could have been a consequence of a switch between these two categories. We found a reversed pattern of responses when all trials were analyzed and no CSE after removing all feature repetitions. This suggests that task switches only lead to the irregular CSE when feature repetitions are included and that the effect vanishes when repetitions are controlled for. This corroborates the finding of Kim and Cho (2014), who did not observe a regular CSE when the response mode switched (e.g., from right to left hand) in non-repetition trials. However, it may be argued that an interpretation in terms of task switches does not hold for the irregular CSE observed in the current study. First, in Experiment 1B, the different stimulus-options were mixed. In spite of not having two categories we still observed a reversed pattern of responses. Second, it seems unlikely that the participants would categorize the stimuli in these two categories (i.e., 1–9 and 2–8) in the masked condition given that the primes are almost imperceptible. Nevertheless, we did find a reversed effect in this condition. Further research in which the switches between such categories are manipulated is needed to evaluate whether task switches could have had an influence in our studies.

CONCLUSION

Based on the current results, we conclude that feature repetitions have an influence on the sequential modulation of the congruency effect. However, this influence seems more complex than previously suggested and might not be directly comparable to conflict adaptation effects. Further research is needed to come to a better understanding of the irregular CSE that we observed in our first experiment. Furthermore, to avoid all confounding influences of feature repetitions, it is important to preclude all sorts of feature repetitions in the design to circumvent the shortcomings of *post hoc* removal of repetition trials. To be able to further unravel the influence of cognitive control on the one hand and feature repetitions on the other such designs will be crucial. In our second experiment we used such a design and found limited support for the role of cognitive control in the regular CSE. This was only the case when the irrelevant information was presented clearly visible. However, more research in which feature repetitions are precluded in both an unmasked as well as masked condition is needed to verify whether our results can be replicated.

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Sequential modulation of distractor-interference produced by semantic generalization of stimulus features

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Sequential modulations of distractor-related interference (i.e., reduced congruency effect after incongruent as compared to congruent predecessor trials, a.k.a. Gratton effect) have been taken to reflect conflict-induced attentional focusing. To dismiss an alternative interpretation based on integration and retrieval of low-level features, it is important to exert experimental control of stimulus and response feature sequences. This has been achieved by considering only trials associated with complete feature changes. Furthermore, distractors from two different perceptual dimensions, such as stimulus location and shape, have been combined in the same experiment to investigate the question of specificity vs. generality of conflict adaptation. With this method feature sequence control can be exerted, in principle, without disregarding data from feature repetition trials. However, such control may be insufficient when the distractor dimensions overlap semantically. In two experiments we found evidence consistent with the assumption that semantic generalization of stimulus features, such as between a stimulus presented at a left-sided location and a stimulus shape pointing to the left, may yield a between-dimension Gratton effect. These findings raise doubts about inferring generalized attentional conflict adaptation when semantically related distractor dimensions are used.

Keywords: conflict adaptation, Gratton effect, feature integration, semantic generalization, episodic memory

INTRODUCTION

When people have to base the selection of a response on a specific stimulus object or feature, processing selectivity is often incomplete in the sense that other stimulus features, which are formally not needed for successful task performance, receive cognitive processing up to a level of behavioral relevance. This can be seen in slower and/or less accurate performance on trials in which an irrelevant stimulus feature (henceforth *distractor*) is associated with an incorrect response (henceforth *incongruent* condition) compared to with the same response as the target feature (henceforth *congruent* condition). This congruency effect has been found to be reduced after incongruent as compared to after congruent predecessor trials, a sequential modulation often referred to as the *Gratton effect*. The Gratton effect has been observed in a variety of different tasks, such as the Stroop task (e.g., Kerns et al., 2004), the Eriksen flanker task (e.g., Gratton et al., 1992), the Simon task (e.g., Notebaert et al., 2001; Wühr and Ansorge, 2005), and different versions of priming tasks (e.g., Kunde and Wühr, 2006; Hazeltine et al., 2011). The dominant interpretation of the Gratton effect implies the assumption of increased focusing of attention on the target stimulus dimension after processing a high-interference stimulus event (Botvinick et al., 2001; Blais et al., 2007; Verguts and Notebaert, 2008, 2009; see Gratton et al., 1992, for a related account).

An alternative view to this attentional adaptation account was put forward by Hommel et al. (2004), see Mayr et al. (2003), for a related idea. Based on Hommel's (Hommel, 1998; Hommel and Colzato, 2004) feature integration theory, the Gratton effect is assumed to result from the retrieval of stimulus and response features previously bound together in episodic memory. According to feature integration theory, stimulus and response features that co-occur close in time are integrated in transient memory episodes, referred to as *event files*. Activation of an item of an event file due to a match with current perceptual input or response demands is assumed to co-activate the other feature(s) of the event file, and mismatches between co-activated representations and current perceptual input or response demands are assumed to interfere with response selection. Therefore, feature integration theory predicts performance impairments on *partial repetition trials* (i.e., on trials associated with repetition of one stimulus or response feature and alternation of another one from the preceding trial) compared to complete feature repetition or alternation trials.

The possible role of stimulus and response feature sequences for the Gratton effect becomes apparent if one looks at the sequences of congruency levels in standard interference tasks. Consider a typical Simon task, in which participants perform a binary classification of a non-spatial target feature, such as judging whether a given stimulus is black or white, by pressing one of two response keys. On each trial, the stimulus is presented in one of two possible locations, each of which spatially corresponds to the location of one of the responses, a left one and a right one, say. A given trial is congruent if stimulus and response locations fall on the same side and incongruent if stimulus and response locations fall on opposite sides. As can be seen in **Table 1**, with such an arrangement there is a confound between the sequence of

Table 1 | Stimulus displays in trials $N-1$ and N, sequence of the response, and sequence of the stimulus location as a function of congruency on trial $N-1$ and trial N.

Congruent ($N-1$) → Congruent (N)				**Incongruent ($N-1$) → Congruent (N)**				**Congruent ($N-1$) → Incongruent (N)**				**Incongruent ($N-1$) → Incongruent (N)**			
$N-1$	**N**	**Resp**	**Loc**	**$N-1$**	**N**	**Resp**	**Loc**	**$N-1$**	**N**	**Resp**	**Loc**	**$N-1$**	**N**	**Resp**	**Loc**
○ +	○ +	Rep	Rep	● +	○ +	Alt	Rep	○ +	+ ○	Rep	Alt	● +	+ ○	Alt	Alt
+ ●	○ +	Alt	Alt	+ ○	○ +	Rep	Alt	+ ●	+ ○	Alt	Rep	+ ○	+ ○	Rep	Rep
○ +	+ ●	Alt	Alt	● +	+ ●	Rep	Alt	○ +	● +	Alt	Rep	● +	● +	Rep	Rep
+ ●	+ ●	Rep	Rep	+ ○	+ ●	Alt	Rep	+ ●	● +	Rep	Alt	+ ○	● +	Alt	Alt

White circles indicate left-sided responses, black circles indicate right-sided responses. + indicates the center of the display. Resp, Response; Loc, (Stimulus) Location, Rep, Repetition, Alt, Alternation.

congruency levels on the one hand and the sequence of stimulus and response locations on the other. Specifically, the congruency level repeats if and only if either both the target/response and the stimulus location repeat or if all these features alternate. Conversely, alternations of the congruency level are bound to trial transitions with repetition of either the stimulus location or the target/response and alternation of the other feature(s). Given this confound, feature integration theory predicts facilitated performance on congruency level repetition trials (i.e., congruent → congruent or incongruent → incongruent) compared to congruency level alternation trials (i.e., congruent → incongruent or incongruent → congruent), and thus a Gratton effect.

Previous research has tried to deconfound the sequence of congruency levels and the sequences of stimulus and response features. One approach is characterized by using a larger number of stimuli and responses and confining the sequential congruency analysis to trials in which all discriminative stimulus and response features differ from the preceding trial. Applying this approach bears the risk of associating congruent and incongruent stimuli with different degrees of distractor-target or distractor-response contingencies, thereby making it possible to account for a Gratton effect in terms of contingency level switch costs rather than conflict adaptation (Schmidt and De Houwer, 2011; Mordkoff, 2012; Schmidt, 2013). Indeed, previous studies that controlled contingencies by using a four-choice task and choosing both the target and the distractor completely randomly on each trial have failed to replicate the Gratton effect (Schmidt and De Houwer, 2011; Mordkoff, 2012). Some recent studies, however, successfully observed Gratton effects under conditions of controlled feature sequences and contingencies. This was achieved by dividing a four-choice task into a pair of two-choice tasks involving distinct sets of targets and distractors. With this arrangement, contingencies are unbiased when congruent and incongruent trials are administered with a probability of 50% each. Trial-to-trial feature repetitions were prevented by alternating between the two tasks on every trial (Kim and Cho, 2014, Experiment 1; Schmidt and Weissman, 2014; Weissman et al., 2014) or by eliminating all data from trials associated with a feature repetition from the analysis (Freitas et al., 2007, Experiment 1; Freitas and Clark, 2014, Experiment 1). Although Gratton effects have thus been obtained in the absence of feature repetitions and biased contingencies, the evidence so far is confined to a particular procedure of grouping targets and distractors into distinct two-choice tasks, devoid of any featural overlap. Future research is needed to clarify the factors underlying the superiority of this "task-splitting procedure."

A different methodological approach that has been applied in research on sequential conflict adaptation includes the presentation of distractors that belong to two different perceptual dimensions. Examining the congruency effect regarding one dimension as a function of the preceding congruency level regarding the other dimension (henceforth, if a sequential modulation is obtained, *between-dimension Gratton effect*) speaks to the question of specificity vs. generality of conflict adaptation mechanisms (for an overview, see Egner, 2008). As will be shown in detail below, the standard experimental set-ups used for this purpose nicely control for the feature sequence confound laid out above. Moreover, with a standard experimental design involving binary target and distractor sets and random choice of both the target and the distractor(s) on each trial, distractor-target/response contingencies are constantly unbiased, irrespective of the congruency level sequence.

Empirically, most studies which combined distractors from different perceptual dimensions failed to yield between-dimension Gratton effects (e.g., Egner et al., 2007; Fernandez-Duque and Knight, 2008; Notebaert and Verguts, 2008, condition 2; Funes et al., 2010a,b; Akçay and Hazeltine, 2011; Lee and Cho, 2013; Stürmer et al., 2005; Verbruggen et al., 2005; Wendt et al., 2006; Schlaghecken et al., 2011; Torres-Quesada et al., 2013, 2014)[1].

In some studies, however, between-dimension Gratton effects were successfully obtained (e.g., Kunde and Wühr, 2006; Freitas et al., 2007; Notebaert and Verguts, 2008, condition 1; Freitas and Clark, 2014). For illustration, consider Experiment 2 of Kunde and Wühr (2006). Participants responded to the left or right direction of a stimulus arrow with spatially corresponding key presses. The arrow occurred randomly on the left or on the right side of the screen and was preceded

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[1] Contrasting with the between-dimension sequential congruency manipulation, none of the above studies controlled for feature sequence and contingencies regarding within-dimension sequential congruency effects. Inferring dimension-specificity of conflict adaptation from these studies (e.g., Egner et al., 2007; Schlaghecken et al., 2011; Lee and Cho, 2013) may thus be premature because the within-dimension Gratton effects found might reflect feature integration or contingency level switching.

by a prime stimulus (a smaller arrow) in the same location, which could also point to the left or right. We shall refer to (mis)match between arrow direction and response location as *direction-(in)congruency* and to (mis)match between stimulus and response location as *location-(in)congruency*. In addition to within-dimension Gratton effects (i.e., a reduced direction-based congruency effect after direction-incongruent trials and a reduced location-based congruency effect after location-incongruent trials), Kunde and Wühr observed—albeit smaller—reductions of location- and direction-based congruency effects after incongruent trials regarding the other distractor dimension.

Table 2 shows the congruency level sequences and the sequences of distractor and response features under such circumstances[2]. As can be seen in **Table 2**, unlike the sequence of congruency levels regarding the same distractor dimension, the sequence of congruency levels regarding different distractor dimensions is not confounded with the sequences of the distractor stimulus features (i.e., arrow direction and stimulus location), the response, or the combination of these features. That is, unlike within-dimension congruency level repetitions and alternations, between-dimension congruency level repetitions and alternations are associated with the same amount of conjoined and partial stimulus and response feature repetitions and alternations.

Notwithstanding this independence, the sequence of congruency levels may be confounded with more abstract stimulus features at least for certain combinations of distractor dimensions. In the current study, we focus on one particular kind of abstract features inherent in manipulations involving two distractor dimensions that are *semantically related*. Consider again the example depicted in **Table 2**. In this example both stimulus location and arrow direction overlap semantically in that each can take one of two values, that is, left or right. Such semantic overlap offers the possibility to account for a between-dimension Gratton effect in terms of feature integration theory, if one assumes semantic generalization of stimulus features in the sense that a stimulus presented on one side, the left say, tends to activate a left-pointing arrow integrated in a previously formed event file and vice versa. This interpretation becomes apparent if one looks at the sequences of left/right features occurring in different formats (i.e., stimulus location and arrow direction) on consecutive trials. As can be seen in the "Location → Direction" and "Direction → Location" columns of **Table 2**, between-dimension congruency level alternations are associated with more partial repetitions regarding such abstract left/right feature-response sequences than between-dimension congruency level repetitions. Consider the case that a direction-congruent trial follows a location-incongruent trial (i.e., between-dimension congruency level alternation). If the response repeats a left-sided stimulus location is followed by a prime arrow pointing to the right or a right-sided stimulus location is followed by a prime arrow pointing to the left (i.e., abstract feature alternation, see column "Location → Direction"). If the response alternates a left-sided stimulus location is followed by a prime arrow pointing to the left or a right-sided stimulus location is followed by a prime arrow pointing to the right (i.e., abstract feature repetition). Assuming semantic generalization, this contingency could impair performance, possibly leading to a reduced direction-based congruency effect after a location-incongruent trial, hence a between-dimension Gratton effect.

This *semantic generalization hypothesis* could also explain why in Kunde and Wühr's (2006) experiment within-dimension Gratton effects were more pronounced than between-dimension Gratton effects. Such a difference in effect strength can be expected for two reasons. First, activation of a feature in an event file should be triggered more reliably or more strongly by perception of an identical rather than a semantically related feature. Second, the confound of between-dimension congruency level sequences and abstract feature sequences is less complete than the confound of within-dimension congruency level sequences and concrete feature sequences, as will be elaborated in the following paragraph.

To gain evidence about semantic generalization of stimulus features that belong to different distractor dimensions it is instructive to look at the relationship of congruency level sequences and abstract feature sequences in more detail. Inspection of **Table 2** shows that, for half of the trials, the location-direction sequence matches the direction-location sequence in the sense that when the abstract left/right feature repeats regarding the location-direction transition, it also repeats regarding the direction-location transition. Also, when the abstract left/right feature alternates regarding the location-direction transition, it also alternates regarding the direction-location transition (see top and bottom quarters of **Table 2**). For example, in line 3 of **Table 2** a left-sided stimulus location in trial $N-1$ is followed by an arrow prime pointing to the left in trial N while at the same time an arrow prime pointing to the right in trial $N-1$ is followed by a right-sided stimulus location in trial N. These sequences mismatch on the other half of trials (see (shaded) middle quarters of **Table 2**). We denote the former trials as *abstract (feature sequence) match* trials and the latter trials as *abstract (feature sequence) mismatch* trials.

It is important to note that whereas on abstract match trials the sequence of the response either matches or mismatches both abstract feature sequences (i.e., location-to-direction and direction-to-location), on abstract mismatch trials the sequence of the response matches one of the abstract feature sequences and mismatches the other one. Thus, on abstract match trials, a conjoined repetition or alternation of the response and the abstract left/right feature regarding one between-dimension transition (e.g., location-direction) is always associated with a conjoined repetition or alternation of the response and the abstract left/right feature regarding the reversed transition (i.e., direction-location). And a partial repetition regarding the response and the abstract left/right feature regarding one between-dimension transition is always associated with a partial repetition regarding the response and the abstract left/right feature regarding the reversed

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[2] Whereas in the experiment of Kunde and Wühr (2006) participants were instructed to identify the direction of a target arrow stimulus, **Table 2** two depicts a situation in which stimulus color acts as the target feature, thereby avoiding overlap with the distractor stimulus dimensions. This difference is not relevant regarding the relationship of consecutive congruency levels and distractor/response feature sequences displayed in the table.

Table 2 | Stimulus displays in trials *N* − 1 and *N*, sequences of the response, and sequences of concrete (location-to-location and direction-to-direction) and abstract (direction-to-location and location-to-direction) distractor features as a function of direction-congruency on trial *N* and location-congruency on trial *N* − 1.

Location-congruent (*N* − 1) → Direction-congruent (*N*)							**Location-incongruent (*N* − 1) → Direction-congruent (*N*)**						
***N* − 1**	***N***	**Resp**	**Loc→Loc**	**Dir→Dir**	**Dir→Loc**	**Loc→Dir**	***N* − 1**	***N***	**Resp**	**Loc→Loc**	**Dir→Dir**	**Dir→Loc**	**Loc→Dir**
⇦ +	⇦ +	Rep	Rep	Rep	Rep	Rep	⬅ +	⇦ +	Alt	Rep	Rep	Rep	Rep
+ ➡	⇦ +	Alt	Alt	Alt	Alt	Alt	+ ⇨	⇦ +	Rep	Alt	Alt	Alt	Alt
⇨ +	+ ⇦	Rep	Alt	Alt	Rep	Rep	➡ +	+ ⇦	Alt	Alt	Alt	Rep	Rep
+ ⬅	+ ⇦	Alt	Rep	Rep	Alt	Alt	+ ⇦	+ ⇦	Rep	Rep	Rep	Alt	Alt
⇨ +	➡ +	Alt	Rep	Rep	Alt	Alt	➡ +	➡ +	Rep	Rep	Rep	Alt	Alt
+ ⬅	➡ +	Rep	Alt	Alt	Rep	Rep	+ ⇦	➡ +	Alt	Alt	Alt	Rep	Rep
⇦ +	+ ➡	Alt	Alt	Alt	Alt	Alt	⬅ +	+ ➡	Rep	Alt	Alt	Alt	Alt
+ ➡	+ ➡	Rep	Rep	Rep	Rep	Rep	+ ⇨	+ ➡	Alt	Rep	Rep	Rep	Rep
⇨ +	⇦ +	Rep	Rep	Alt	Alt	Rep	➡ +	⇦ +	Alt	Rep	Alt	Alt	Rep
+ ⬅	⇦ +	Alt	Alt	Rep	Rep	Alt	+ ⇦	⇦ +	Rep	Alt	Rep	Rep	Alt
⇦ +	+ ⇦	Rep	Alt	Rep	Alt	Rep	⬅ +	+ ⇦	Alt	Alt	Rep	Alt	Rep
+ ➡	+ ⇦	Alt	Rep	Alt	Rep	Alt	+ ⇨	+ ⇦	Rep	Rep	Alt	Rep	Alt
⇦ +	➡ +	Alt	Rep	Alt	Rep	Alt	⬅ +	➡ +	Rep	Rep	Alt	Rep	Alt
+ ➡	➡ +	Rep	Alt	Rep	Alt	Rep	+ ⇨	➡ +	Alt	Alt	Rep	Alt	Rep
⇨ +	+ ➡	Alt	Alt	Rep	Rep	Alt	➡ +	+ ➡	Rep	Alt	Rep	Rep	Alt
+ ⬅	+ ➡	Rep	Rep	Alt	Alt	Rep	+ ⇦	+ ➡	Alt	Rep	Alt	Alt	Rep

Location-congruent (*N* − 1) → Direction-incongruent (*N*)							**Location-incongruent (*N* − 1) → Direction-incongruent (*N*)**						
***N* − 1**	***N***	**Resp**	**Loc→Loc**	**Dir→Dir**	**Dir→Loc**	**Loc→Dir**	***N* − 1**	***N***	**Resp**	**Loc→Loc**	**Dir→Dir**	**Dir→Loc**	**Loc→Dir**
⇦ +	⇨ +	Rep	Rep	Alt	Rep	Alt	⬅ +	⇨ +	Alt	Rep	Alt	Rep	Alt
+ ➡	⇨ +	Alt	Alt	Rep	Alt	Rep	+ ⇨	⇨ +	Rep	Alt	Rep	Alt	Rep
⇨ +	+ ⇨	Rep	Alt	Rep	Rep	Alt	➡ +	+ ⇨	Alt	Alt	Rep	Rep	Alt
+ ⬅	+ ⇨	Alt	Rep	Alt	Alt	Rep	+ ⇦	+ ⇨	Rep	Rep	Alt	Alt	Rep
⇨ +	⬅ +	Alt	Rep	Alt	Alt	Rep	➡ +	⬅ +	Rep	Rep	Alt	Alt	Rep
+ ⬅	⬅ +	Rep	Alt	Rep	Rep	Alt	+ ⇦	⬅ +	Alt	Alt	Rep	Rep	Alt
⇦ +	+ ⬅	Alt	Alt	Rep	Alt	Rep	⬅ +	+ ⬅	Rep	Alt	Rep	Alt	Rep
+ ➡	+ ⬅	Rep	Rep	Alt	Rep	Alt	+ ⇨	+ ⬅	Alt	Rep	Alt	Rep	Alt
⇨ +	⇨ +	Rep	Rep	Rep	Alt	Alt	➡ +	⇨ +	Alt	Rep	Rep	Alt	Alt
+ ⬅	⇨ +	Alt	Alt	Alt	Rep	Rep	+ ⇦	⇨ +	Rep	Alt	Alt	Rep	Rep
⇦ +	+ ⇨	Rep	Alt	Alt	Alt	Alt	⬅ +	+ ⇨	Alt	Alt	Alt	Alt	Alt
+ ➡	+ ⇨	Alt	Rep	Rep	Rep	Rep	+ ⇨	+ ⇨	Rep	Rep	Rep	Rep	Rep
⇦ +	⬅ +	Alt	Rep	Rep	Rep	Rep	⬅ +	⬅ +	Rep	Rep	Rep	Rep	Rep
+ ➡	⬅ +	Rep	Alt	Alt	Alt	Alt	+ ⇨	⬅ +	Alt	Alt	Alt	Alt	Alt
⇨ +	+ ⬅	Alt	Alt	Alt	Rep	Rep	➡ +	+ ⬅	Rep	Alt	Alt	Rep	Rep
+ ⬅	+ ⬅	Rep	Rep	Rep	Alt	Alt	+ ⇦	+ ⬅	Alt	Rep	Rep	Alt	Alt

Arrows represent directional information in the distractor stimulus feature. The target stimulus feature is indicated by color: White indicates a left-sided response, black indicates a right-sided response. + indicates the center of the display. Unshaded and shaded areas indicate abstract feature sequence match and mismatch trials, respectively (see text for explanation). Resp, Response; Dir, (Arrow) Direction; Loc, Stimulus Location; Rep, Repetition; Alt, Alternation.

between-dimension transition. In contrast, on abstract mismatch trials, whenever there is a conjoined repetition or alternation of the response and the abstract left/right feature regarding one between-dimension transition, there is a partial repetition regarding the response and the abstract left/right feature regarding the reversed between-dimension transition.

As a consequence of this contingency, on abstract match trials both semantic sequence effects should work in the same direction, whereas on abstract mismatch trials they should work in opposite directions. Assuming that semantic location-direction transitions and direction-location transitions yield effects of comparable strength—that is, a stimulus location integrated in an event file is activated by perceiving a corresponding arrow direction to roughly the same amount as an arrow direction integrated in an event file is activated by perceiving a corresponding stimulus location—semantic sequence effects should add up to zero on abstract feature sequence mismatch trials. On the corollary assumption of comparable effect strength, semantic generalization should therefore yield a between-dimension Gratton effect selectively on abstract match trials and an absence thereof on abstract mismatch trials. The semantic generalization hypothesis thus predicts between-dimension Gratton effects on abstract match trials and, assuming comparable efficacy of semantic generalization for both dimension transition directions, the absence of a between-dimension Gratton effect on abstract mismatch trials.

EXPERIMENT 1

In the first experiment of the current study, we explored the role of semantic generalization for between-dimension Gratton effects by combining stimulus location and pointing direction of the stimulus shape, each of which could vary between the values left and right, as distractor dimensions, thereby producing the contingencies displayed in **Table 2**. Participants classified the color of stimulus arrows, which pointed either to the left or to the right and occurred either to the left or to the right of the screen center. A pilot experiment yielded location-based interference (i.e., a Simon effect) but no main effect of direction-based congruency. A possible explanation for this result is that participants did not sufficiently code the stimulus arrows as pointing to the left or to the right because arrow direction was never relevant throughout the experiment. To increase the likelihood of left/right coding of the arrow direction, we inserted blocks of trials in which participants responded to the direction of the arrows rather than to their colors in Experiment 1.

METHOD

Participants

Two female and 13 male students of the Helmut-Schmidt-University/University of the Federal Armed Forces Hamburg participated in exchange for partial course requirements. They ranged in age from 19 to 28 years. The experiments of the current study were conducted in accordance with the ethical guidelines of the German Psychological Society (Deutsche Gesellschaft für Psychologie) and the Declaration of Helsinki of the World Medical Association. Formal ethics approvals for the described kind of research are not required by the guidelines of the German Psychological Association or the World Medical Association.

Apparatus and stimuli

The stimuli were presented on a 17-in. monitor with a refresh rate of 60 Hz. A dark gray background was used. Stimuli were arrows pointing either to the left or to the right, which extended 3.0 cm horizontally and 2.5 cm vertically. As mentioned above, we inserted blocks of trials, in which participants responded to arrow direction, amongst the experimental blocks, in which stimulus color had to be judged. We refer to the former blocks as intermediate blocks, and to the latter blocks as critical blocks. In the critical blocks, arrows were presented in either blue or yellow color and occurred either 2.4 cm to the left or to the right of the screen center (nearest edge) on the horizontal midline. In the intermediate blocks the arrows were white stimuli and occurred in the screen center. Two response keys were used. They were located on an external keyboard and extended 1.0 × 1.0 cm. One key was located 4.0 cm to the left and the other 4.0 cm to the right of the keyboard's saggital midline, which was placed perpendicular to the screen and aligned with the screen center. The left response key was pressed with the index or middle finger of the left hand; the right response key was pressed with the index or middle finger of the right hand. During an experimental block the fingers remained on the keys. Regarding the critical blocks, odd-numbered participants were instructed to press the left key for yellow and the right key for blue. This assignment was reversed for even-numbered participants. In the intermediate blocks, participants were to respond to the direction of the arrow with the spatially corresponding key press. **Figure 1**, left panel, depicts schematic examples of stimulus displays used in different conditions of the critical blocks.

Procedure

Participants sat approximately 50 cm from the computer screen. Each trial began with the presentation of a fixation cross in the center of the screen (0.3 × 0.3 cm). After a period of 500 ms, an arrow stimulus was presented. In the critical blocks, the color (blue vs. yellow), the location (left vs. right), and the direction (left vs. right) of the arrow were chosen randomly on each trial. In the intermediate blocks, arrow direction was again chosen randomly on each trial. Participants were instructed to classify the stimulus by pressing the assigned response key as quickly as possible while avoiding errors. Immediately after a response key was pressed, the stimulus and the fixation cross disappeared from the screen. In case of a correct response the next trial started 500 ms after the response with the presentation of the fixation cross. In

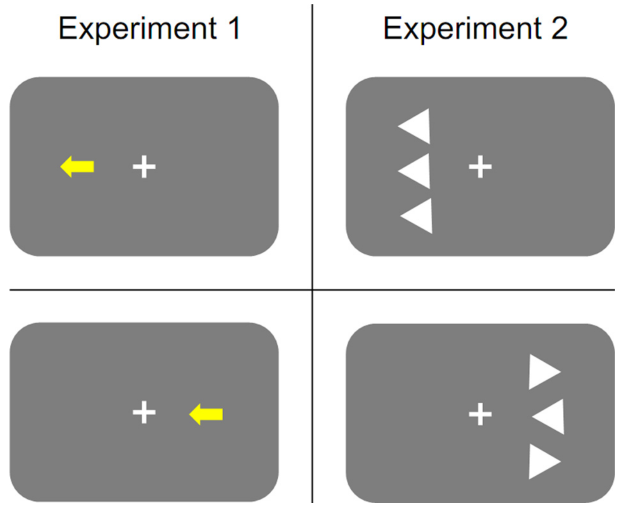

FIGURE 1 | Schematic examples of stimulus displays used in Experiment 1 (left panel) and Experiment 2 (right panel). Regarding Experiment 1, for participants that responded to yellow arrows with the left-sided response and to blue arrows with the right-sided response the top row depicts a stimulus of a location-congruent/direction-congruent trial and the bottom row depicts a stimulus of a location-incongruent/direction-congruent trial. For participants that responded to yellow arrows with the right-sided response and to blue arrows with the left-sided response the top row depicts a stimulus of a location-incongruent/direction-incongruent trial and the bottom row depicts a stimulus of a location-congruent/direction-incongruent trial. Regarding Experiment 2, the top row depicts a stimulus of a location-congruent/direction-congruent trial, and the bottom row depicts a stimulus of a location-incongruent/direction-congruent trial.

case of an incorrect response, the German word "falsch" ("incorrect") occurred for 800 ms slightly below the screen center. Then the trial was repeated with an identical stimulus. Repetitions of incorrect trials were not counted as trials (and not subjected to the statistical analyses).

At the beginning of the experiment participants received written instructions. After a practice block of 30 trials, which was structurally identical to the critical blocks, participants were presented with 24 critical blocks of 35 trials each. In advance of each critical block, an intermediate block of 16 trials was administered. Only critical blocks were subjected to the statistical analysis. Between blocks, participants were allowed to rest for some time. A complete session took between 40 and 45 min.

RESULTS

The first three trials of each critical block were considered "warm-up" trials and not analyzed. Furthermore, we excluded data from the first two trials following an error as well as RTs associated with an incorrect response or smaller than 200 ms or larger than 1200 ms. 0.2% of the data were eliminated by excluding RT outliers.

Two sets of analysis, an overall analysis of within-dimension and between-dimension Gratton effects, and a semantic feature sequence analysis, were conducted. In the overall analysis, trials were classified depending on location-congruency and direction-congruency on the current (N) and the preceding ($N - 1$) trial. To investigate the semantic generalization hypothesis, we analyzed between-dimension sequential congruency effects depending on match vs. mismatch of the abstract feature sequences.

Overall analysis

Analyses of Variance (ANOVAs) with repeated measures on the factors direction-congruency on the current trial (congruent, incongruent), location-congruency on the current trial (congruent, incongruent), direction-congruency on the preceding trial (congruent, incongruent), and location-congruency on the preceding trial (congruent, incongruent) were conducted on the mean RTs and error proportions.

Responding took longer when the current trial was associated with an incongruent as compared to a congruent arrow direction (378 vs. 372 ms), $F_{(1,\ 14)} = 5.8$; $p < 0.04$; $MSE = 310.1$, and when the current trial was associated with an incongruent as compared to a congruent stimulus location (388 vs. 362 ms), $F_{(1,\ 14)} = 39.1$; $p < 0.01$; $MSE = 1034.2$. Within-dimension Gratton effects reliably occurred for both distractor dimensions. The direction-based congruency effect was reduced from 17 to -6 ms after a direction-incongruent trial as compared to a direction-congruent trial, $F_{(1,\ 14)} = 7.9$; $p < 0.02$; $MSE = 858.7$, and the location-based congruency effect was reduced from 57 to -4 ms after a location-incongruent trial as compared to a location-congruent trial, $F_{(1,\ 14)} = 125.6$; $p < 0.01$; $MSE = 443.5$. Regarding between-dimension sequential modulations, the direction-based congruency effect was reduced after a location-incongruent trials as compared to a location-congruent trials (0 vs. 11 ms), $F_{(1,\ 14)} = 6.9$; $p < 0.02$; $MSE = 257.9$. Likewise, the location-based congruency effect was reduced after direction-incongruent as compared to after direction-congruent trials (24 vs. 29 ms), $F_{(1,\ 14)} = 4.7$; $p < 0.05$; $MSE = 87.2$.

In the error analysis, the main effect of direction-congruency failed to reach significance (4.1 vs. 4.9%, for congruent and incongruent trials, respectively), $F_{(1,\ 14)} = 3.2$; $p = 0.10$; $MSE = 0.00116$. Errors were more frequent, however, when the current trial was associated with an incongruent as compared to a congruent stimulus location (5.6 vs. 3.4%), $F_{(1,\ 14)} = 6.9$; $p < 0.03$; $MSE = 0.00434$. In addition, an incongruent arrow direction on the preceding trial reduced errors from 4.9 to 4.1%, $F_{(1,\ 14)} = 5.2$; $p < 0.04$; $MSE = 0.00061$. Again, within-dimension Gratton effects occurred for both dimensions: The direction-based congruency effect was reduced from 1.9 to -0.3% after a direction-incongruent trial compared to a direction-congruent trial, $F_{(1,\ 14)} = 8.4$; $p < 0.02$; $MSE = 0.00098$, and the location-based congruency effect was reduced from 7.4 to -3.0% after a location-incongruent trial compared to a location-congruent trial, $F_{(1,\ 14)} = 25.4$; $p < 0.01$; $MSE = 0.00630$. By contrast, no between-dimension sequential modulation occurred, both $Fs_{(1,\ 14)} < 1$.

Semantic generalization effects

Location-congruency ($N - 1$) → direction-congruency (N). Trials were classified as a function of direction-congruency in trial N (congruent, incongruent), location-congruency in trial $N - 1$ (congruent, incongruent), and abstract correspondence (match, mismatch). **Figure 2** displays mean RTs and error percentages for these data. Regarding RTs, the main effect of direction-congruency as well as the interaction with the location-congruency level of the preceding trial were replicated from the overall analysis, $F_{(1,\ 14)} = 4.9$; $p < 0.05$; $MSE = 207.4$, and $F_{(1,\ 14)} = 5.7$; $p < 0.04$; $MSE = 139.6$, respectively. As predicted by the semantic generalization hypothesis, this was modulated by the abstract correspondence, $F_{(1,\ 14)} = 10.3$;

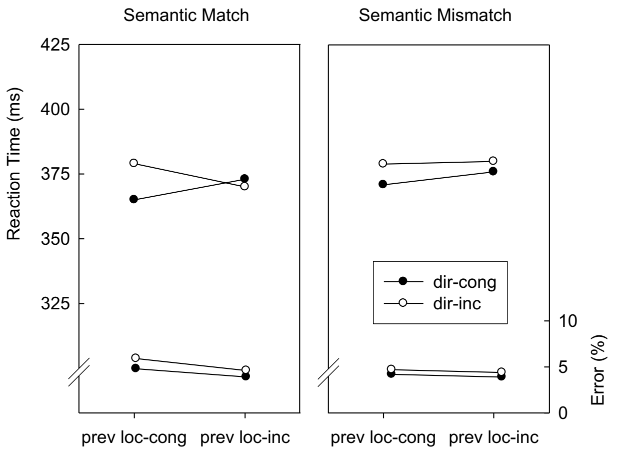

FIGURE 2 | Mean reaction times and error percentages in Experiment 1 as a function of direction-congruency on the current trial (dir-cong, dir-inc), location-congruency on the preceding trial (prev loc-cong, prev loc-inc), and correspondence of the abstract feature sequences (match, mismatch).

$p < 0.01$; $MSE = 31.1$. Whereas on abstract match trials, the direction-based congruency effect amounted to 14 ms after location-congruent trials and −3 ms after location-incongruent trials, abstract mismatch trials were associated with 8 and 4 ms of direction-based interference after location-congruent and location-incongruent trials, respectively. The error data yielded no significant effects.

Direction-congruency (N − 1) → Location-congruency (N). Trials were classified as a function of location-congruency in trial *N* (congruent, incongruent), direction-congruency in trial $N - 1$ (congruent, incongruent), and abstract correspondence (match, mismatch). **Figure 3** displays mean RTs and error percentages for these data. Regarding RTs, the main effect of location-congruency as well as the interaction with direction-congruency of the preceding trial were replicated from the overall analysis, $F_{(1,\,14)} = 35.8$; $p < 0.01$; $MSE = 564.0$, and, $F_{(1,\,14)} = 4.9$; $p < 0.05$; $MSE = 29.6$, respectively. In line with the semantic generalization hypothesis, reduction of the location-based congruency effect after direction-incongruent trials occurred selectively on abstract match trials, $F_{(1,\,14)} = 6.2$; $p < 0.03$; $MSE = 131.4$. Specifically, on abstract match trials, the location-based congruency effect amounted to 35 ms after direction-congruent trials and 20 ms after direction-incongruent trials, whereas on abstract mismatch trials the location-based congruency effect was 21 ms after direction-congruent trials and 28 ms after direction-incongruent trials. The error data yielded only a significant main effect of location-congruency, $F_{(1,\,14)} = 7.4$; $p < 0.02$; $MSE = 0.00221$.

DISCUSSION

Experiment 1 replicated previously found sequential modulations of location-based interference and of direction-based interference. Although these within-dimension Gratton effects are in line with the conflict adaptation hypothesis, they can also be accounted for in terms of distractor-target or distractor-response feature integration because of the complete confound of the sequences of congruency levels and the sequences of specific distractor and target/response conjunctions. In addition, replicating the finding of Kunde and Wühr (2006), both direction- and location-based interference were reduced after an incongruent compared to after a congruent distractor of the other stimulus dimension. In light of evidence suggesting different time courses of interference effects elicited by location-based and symbolic spatial distractors (Pellicano et al., 2009), these findings are remarkable from a conflict adaptation perspective that assumes that conflict adaptation generalizes preferentially between types of conflict with overlapping characteristics. However, although these between-dimension Graton effects occurred in the absence of confounds with the sequences of discriminative stimulus and response features, they were confined to abstract match trials. Thus, the results of Experiment 1 conform to the predictions of the semantic generalization hypothesis. By contrast, the conflict adaptation hypothesis does not seem to offer an explanation for this pattern of findings.

Although we intermixed blocks of trials in which participants responded to the direction of the stimulus arrow, direction-based interference (in the critical blocks) was overall small–considerably smaller than location-based interference and smaller than in the study of Kunde and Wühr (2006). Given the possibility that attentional adaptation correlates with conflict strength (e.g., Wendt et al., 2014), it may not have occurred in a detectable way under these conditions. To investigate the modulation of the congruency effects under conditions in which incongruent arrow distractors were likely to yield substantial conflict, we conducted a second experiment in which we used a task that involved the identification of the pointing direction of a target stimulus as left or right.

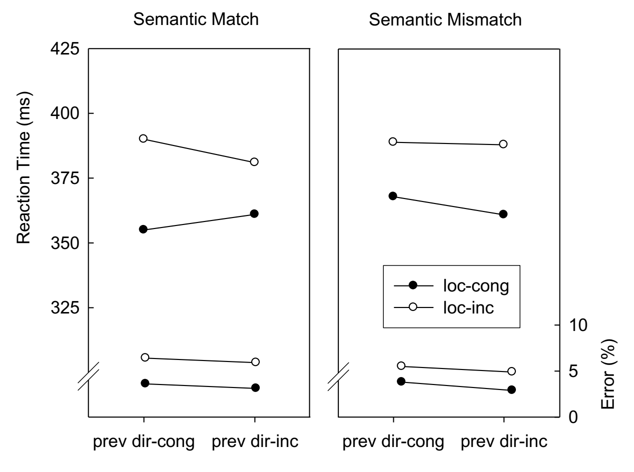

FIGURE 3 | Mean reaction times and error percentages in Experiment 1 as a function of location-congruency on the current trial (loc-cong, loc-inc), direction-congruency on the preceding trial (prev dir-cong, prev dir-inc), and correspondence of the abstract feature sequences (match, mismatch).

EXPERIMENT 2

In Experiment 2 participants responded to a triangle that pointed either to the left or to the right by pressing the spatially corresponding key. Directional distractor information was presented in the form of a pair of different triangles, in which the target stimulus was embedded, forming a vertical target-flanker configuration. Like in Experiment 1, the stimuli could be presented in a left-sided or in a right-sided screen location. Experiment 2 thus combined location-based interference with interference evoked by spatially adjacent flanker stimuli. Noteworthily, previous studies that combined location-based interference and flanker interference, however in the context of non-spatial tasks, failed to obtain between-dimension Gratton effects (Stürmer et al., 2005; Wendt et al., 2006).

Although using left- and right-pointing triangles as target stimuli introduced additional overlap of left/right features, this did not change any contingencies of relevance for our analyses. This can be illustrated by replacing, in **Table 2**, white and black arrow color with left-and right-pointing target triangles, and left- and right-pointing arrows with left-and right-pointing flanker triangles, respectively. This would evidently not affect the contingencies of the sequences of congruency levels and the sequences of concrete as well as abstract distractor features and responses.

Therefore, Experiment 2 could be analyzed along the same lines as Experiment 1.

METHOD

Participants

Five female and 10 male students of the Helmut-Schmidt-University/University of the Federal Armed Forces Hamburg participated in exchange for partial course requirements. They ranged in age from 21 to 24 years.

Apparatus and stimuli

Apparatus and stimuli were the same as in the preceding experiments with the following exceptions. Arrow stimuli were replaced by a row of three, vertically aligned equilateral triangles, presented either 2.8 cm (medial edge) to the left or to the right of the screen center, and extending 5.5 cm vertically and 1.7 cm horizontally. The central (target) triangle was presented on the horizontal midline. Triangles had a side length of 1.8 cm and could point to the left or to the right. The same response keys were used as in Experiment 1.

Procedure

The procedure of Experiment 2 was the same as the procedure of Experiment 1 with the following exceptions. First, participants responded to the pointing direction of the central triangle by pressing the spatially corresponding response key. Second, on each trial, the pointing direction (left vs. right) of the central triangle as well as the pointing direction of the flanker triangles (left vs. right) were chosen randomly with the only constraint that the two distractor triangles pointed in the same direction. **Figure 1** depicts schematic examples of stimulus displays used in different conditions. Third, after working through a practice block of 30 trials participants were administered 15 experimental (i.e., critical) blocks of 67 trials each. A complete session took between 45 and 60 min.

RESULTS

The same analyses and exclusion criteria were applied as in Experiment 1. RT outlier exclusion eliminated 0.04% of the data.

Overall analysis

Responses were slower when the current trial was associated with incongruent as compared to congruent flankers (486 vs. 415 ms), $F_{(1,\ 14)} = 296.7$; $p < 0.01$; $MSE = 996.4$. In contrast, there was no overall location-based congruency effect, $F_{(1,\ 14)} < 1$. Direction- and location-based interference interacted however, yielding a location-based congruency effect of 11 ms on direction-congruent trials and a slightly reversed location-based congruency effect of −5 ms on direction-incongruent trials, $F_{(1,\ 14)} = 19.4$; $p < 0.01$; $MSE = 199.5$. Within-dimension Gratton effects occurred for both dimensions. The direction-based congruency effect was reduced from 82 to 59 ms after a direction-incongruent compared to after a direction-congruent trial, $F_{(1,\ 14)} = 32.9$; $p < 0.01$; $MSE = 252.6$, and the location-based congruency effect was reversed from 37 ms after location-congruent to −31 ms after location-incongruent trials, $F_{(1,\ 14)} = 98.0$; $p < 0.01$; $MSE = 715$. The latter effect was further modulated by an interaction with direction-congruency of the preceding trial, $F_{(1,\ 14)} = 6.6$; $p < 0.03$; $MSE = 212.4$, indicating that the sequential modulation was somewhat larger when the preceding trial involved incongruent flankers. The direction-based congruency effect was also reduced after location-incongruent trials compared to after location-congruent trials (65 vs. 76 ms), $F_{(1,\ 14)} = 16.0$; $p < 0.01$; $MSE = 113.4$. This between-dimension Gratton effect was further modulated by an interaction with location-congruency of the current trial, $F_{(1,\ 14)} = 39.2$; $p < 0.01$; $MSE = 148.1$, indicating that the effect was confined to location-congruent trials. The location-based congruency effect was not affected by the direction-congruency level of the preceding trial (1 vs. 4 ms after direction-congruent and direction-incongruent trials, respectively), $F_{(1,\ 14)} = 1.4$; $p = 0.25$; $MSE = 65.6$.

In the error analysis, all main effects were significant, indicating that errors were more frequent with incongruent than with congruent flankers (4.1 vs. 1.3%), $F_{(1,\ 14)} = 28.5$; $p < 0.01$; $MSE = 0.00163$, and with incongruent than with congruent stimulus locations (3.5 vs. 1.9%), $F_{(1,\ 14)} = 12.9$; $p < 0.01$; $MSE = 0.00116$, and less frequent after a trial with incongruent than congruent flankers (2.4 vs. 3.0%), $F_{(1,\ 14)} = 5.5$; $p < 0.04$; $MSE = 0.00042$, as well as after a trial with an incongruent than with a congruent stimulus location (2.0 vs. 3.5%), $F_{(1,\ 14)} = 14.9$; $p < 0.01$; $MSE = 0.00087$. Direction- and location-based interference interacted overadditively, $F_{(1,\ 14)} = 5.3$; $p < 0.04$; $MSE = 0.00068$. Both the direction-based and the location-based congruency effect were reduced after a location-incongruent compared to a location-congruent predecessor trial, 1.9 vs. 3.6%, $F_{(1,\ 14)} = 6.2$; $p < 0.03$; $MSE = 0.00075$, and −1.4 vs. 4.5%, $F_{(1,\ 14)} = 30.6$; $p < 0.01$; $MSE = 0.00172$, respectively. A three-way interaction involving location-congruency of the current and of the preceding trial and direction-congruency of the current trial, $F_{(1,\ 14)} = 13.3$; $p < 0.01$; $MSE = 0.00062$, indicated that the location-based Gratton effect was more pronounced on trials with incongruent than with congruent flankers. Neither direction- nor location-based interference was affected by the direction-congruency level of the preceding trial, both Fs < 1.

Semantic generalization effects

Location-congruency ($N - 1$) → direction-congruency (N). Trials were classified as a function of direction-congruency in trial N (congruent, incongruent), location-congruency in trial $N - 1$ (congruent, incongruent), and abstract correspondence (match, mismatch). **Figure 4** displays mean RTs and error percentages for these data. In RTs, the main effect of direction-congruency and the interaction with location-congruency on the preceding trial were replicated from the overall analysis, $F_{(1,\ 14)} = 294.2$; $p < 0.01$; $MSE = 504.2$, and $F_{(1,\ 14)} = 14.7$; $p < 0.01$; $MSE = 85.6$, respectively. Abstract mismatch trials were associated with an increase in overall RTs of 8 ms, $F_{(1,\ 14)} = 22.1$; $p < 0.01$; $MSE = 95.5$, and with an increase in direction-based interference of 8 ms, $F_{(1,\ 14)} = 5.4$; $p < 0.04$; $MSE = 108.5$. There was no sign of a three-way interaction involving direction-congruency of the current trial, location-congruency of the preceding trial, and abstract correspondence, $F_{(1,\ 14)} = 1.2$; $p = 0.30$; $MSE = 32.1$. The error analysis replicated the main effects of direction-congruency and preceding location-congruency as well as the

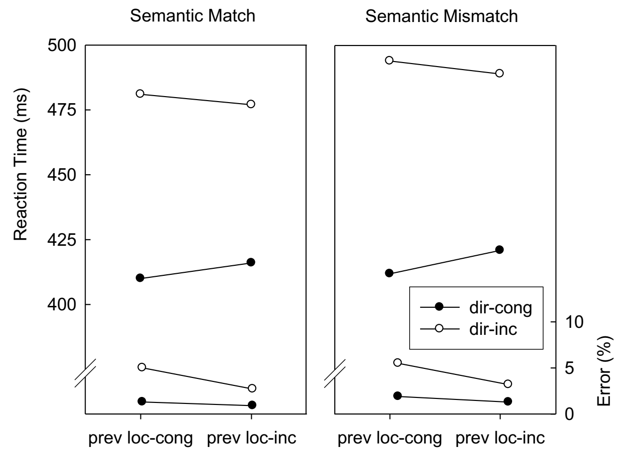

FIGURE 4 | Mean reaction times and error percentages in Experiment 2 as a function of direction-congruency on the current trial (dir-cong, dir-inc), location-congruency on the preceding trial (prev loc-cong, prev loc-inc), and correspondence of the abstract feature sequences (match, mismatch).

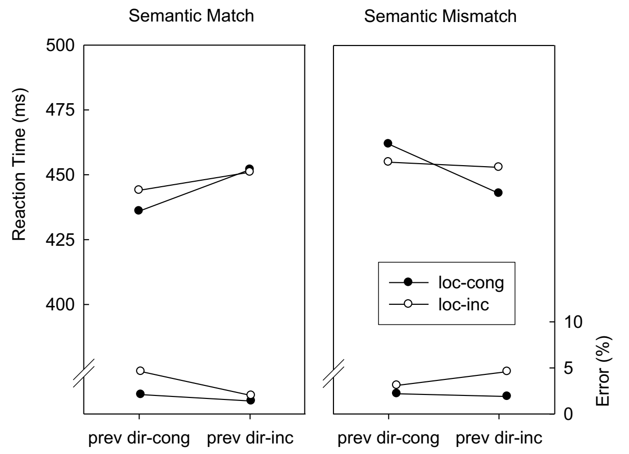

FIGURE 5 | Mean reaction times and error percentages in Experiment 2 as a function of location-congruency on the current trial (loc-cong, loc-inc), direction-congruency on the preceding trial (prev dir-cong, prev dir-inc), and correspondence of the abstract feature sequences (match, mismatch).

interaction between these two factors from the overall analysis, $F_{(1,\,14)} = 30.0$; $p < 0.01$; $MSE = 0.00077$, $F_{(1,\,14)} = 13.4$; $p < 0.01$; $MSE = 0.00045$, and $F_{(1,\,14)} = 7.2$; $p < 0.02$; $MSE = 0.00033$. Mirroring the RT results, the reduction of the direction-based congruency effect after location-incongruent trials was not modulated by abstract correspondence, $F_{(1,\,14)} < 1$.

Direction-congruency (N − 1) → Location-congruency (N). Trials were classified as a function of location-congruency in trial *N* (congruent, incongruent), direction-congruency in trial *N* − 1 (congruent, incongruent), and abstract correspondence (match, mismatch). **Figure 5** displays mean RTs and error percentages for these data. In the RT analysis, the only significant main effect was abstract correspondence, $F_{(1,\,14)} = 11.9$ $p < 0.01$; $MSE = 141.2$, indicating that abstract mismatch trials were associated with longer RTs than abstract match trials (453 vs. 446 ms). In addition, incongruent flankers in the preceding trial slowed responding on abstract match trials by 12 ms and speeded up responding on abstract mismatch trials by 10 ms, $F_{(1,\,14)} = 31.7$; $p < 0.01$; $MSE = 110.0$. As in the overall analysis, the location-based congruency effect was not significantly affected by the direction-congruency level of the preceding trial, $F_{(1,\,14)} = 3.1$; $p = 0.10$; $MSE = 44.4$. However, there was a significant three-way interaction, $F_{(1,\,14)} = 14.7$; $p < 0.01$; $MSE = 83.7$. As can be seen in **Figure 5**, this was because on abstract match trials the location-based congruency effect was larger after direction-congruent than direction-incongruent trials, whereas on abstract mismatch trials the location-based congruency effect was larger after direction-incongruent than after direction-congruent trials. The error analysis replicated the main effect of location-based congruency from the overall analysis, $F_{(1,\,14)} = 13.6$; $p < 0.02$; $MSE = 0.00060$. Also, the main effect of direction-congruency on the preceding trial approached significance, $F_{(1,\,14)} = 3.8$; $p = 0.07$; $MSE = 0.00024$. Contrary to the RT analysis, incongruent flankers in the preceding trial decreased the error rate on abstract match trials by 1.7% and increased the error rate on abstract mismatch trials by 0.6%, $F_{(1,\,14)} = 15.8$; $p < 0.01$; $MSE = 0.00025$. Although there was no overall reduction of location-based interference after direction-incongruent trials, $F_{(1,\,14)} < 1$, a significant three-way interaction indicated that the location-based congruency effect was larger after direction-congruent than direction-incongruent trials on abstract match trials, whereas it was larger after direction-incongruent than after direction-congruent trials on abstract mismatch trials, $F_{(1,\,14)} = 8.2$; $p < 0.02$; $MSE = 0.00033$.

DISCUSSION

Barring minor procedural differences Experiment 2 differed from Experiment 1 in that the target stimulus features were perceptually similar to the direction distractor features. As expected, the direction-based congruency effect was considerably larger under these conditions than in Experiment 1. In contrast to this enhancement of distractor interference, the main effect of location-based interference failed to reach significance in RTs.

Despite the absence of a significant overall effect of location-based interference, however, a clear-cut pattern of sequential modulations of congruency effects occurred. Again, we found within-dimension Gratton effects for both distractor dimensions, which cannot unequivocally be attributed to conflict adaptation because of the confound with distractor-target or distractor-response sequences. Replicating the findings of Experiment 1, a between-dimension Gratton effect was found for direction-based interference, and—deviating from the results of Experiment 1—displayed comparable strength for abstract match and mismatch trials. As such, these findings seem to provide novel evidence for the conflict adaptation account. However, the modulations of location-based interference pose a problem for this interpretation. Specifically, although there was no overall modulation of the location-based congruency effect by the direction-based congruency level of the preceding trial, this absence resulted from a between-dimension Gratton effect on abstract match trials and a reversed between-dimension Gratton effect (i.e., a larger congruency effect after an incongruent than after a congruent predecessor trial) on abstract mismatch trials. Viewed from the conflict adaptation perspective, the latter effect would suggest that location-congruent rather than location-incongruent trials increased attentional focusing. There seems to be no straightforward reason for such an assumption.

By contrast, the semantic generalization hypothesis offers a plausible explanation for the co-occurrence of a between-dimension Gratton effect for one of the distractor dimensions and a reversed between-dimension Gratton effect for the other distractor dimension, on abstract mismatch trials. As noted in the Introduction, on abstract mismatch trials conjoined repetitions/alternations regarding the response and the location-to-direction sequence are associated with partial repetitions regarding the response and the direction-to-location sequence and vice versa. We argued that, in the case that both abstract feature sequences yield equally strong effects, this arrangement should result in a null effect on abstract mismatch trials, as found in Experiment 1. On the other hand, there is no a priori reason to assume that the activation of a stored location code by perceiving a corresponding arrow direction is about as strong as the activation of a stored direction code by perceiving a stimulus in a corresponding location. Therefore, it is also conceivable that one of the assumed mechanisms, activation of a location code by a perceived direction or activation of a direction code by a perceived location, is substantially stronger than the other one, depending on yet unidentified stimulus and task characteristics.

Consider the case that the activation of a location code elicited by perceiving a corresponding arrow direction is stronger than the activation of a direction code elicited by perceiving a corresponding stimulus location. For abstract match trials, in which effects of the location-direction sequence and the direction-location sequence work in the same direction, this would lead to qualitatively the same expectation as an equal strength assumption. The prediction would be different, however, for abstract mismatch trials. Performance on these trials should be more affected by the location-direction sequence than by the direction-location-sequence. Looking at the abstract mismatch trials in **Table 2** shows that between-dimension congruency level repetition (i.e., location-congruent → direction-congruent and location-incongruent → direction-incongruent) are associated with advantageous conjunctions (i.e., conjoined repetitions or alternations) of the abstract location-to-direction feature and the response. By contrast, between-dimension congruency level alternations (i.e., location-incongruent → direction-congruent and location-congruent → direction-incongruent) are associated with disadvantageous conjunctions (i.e., partial repetitions) of the abstract location-to-direction feature and the response. This pattern is perfectly reversed for sequences regarding the abstract direction-to-location feature and the response. Assuming that the direction-location sequences are less influential than the location-direction sequences, we would expect facilitation of direction-congruent trials after location-congruent trials and facilitation of direction-incongruent trials after location-incongruent trials. Hence we would expect direction-based interference to be reduced after location-incongruent trials also on abstract mismatch trials.

Regarding location-based interference after direction-congruent and direction-incongruent trials, however, the assumption that direction-location sequences are less influential than location-direction sequences would lead to the opposite prediction for abstract mismatch trials, that is increased location-based interference after direction-incongruency, as compared to after direction-congruency. This is because on such trials between-dimension congruency level repetitions are associated with partial repetitions regarding the abstract location-direction feature and the response, whereas between-dimension congruency level alternations are associated with partial repetitions regarding the abstract location-direction feature and the response. (This assertion can be verified by identifying the respective trial transitions in the shaded area of **Table 2**).

An analogous reasoning shows that if direction-to-location sequences had a larger impact than location-to-direction sequences, the semantic generalization hypothesis would predict location-based interference to be reduced after direction-incongruent trials on both abstract match and mismatch trials. Direction-based interference, on the other hand, should be reduced after location-incongruent trials on abstract match trials but increased on abstract mismatch trials.

In more general terms, the semantic generalization hypothesis predicts one of three different patterns regarding between-dimension sequential congruency modulations on abstract mismatch trials: no between-dimension Gratton effects at all (as found in Experiment 1), reduced direction-based interference after location-incongruent trials and increased location-based interference after direction-incongruent trials (as found in Experiment 2), or reduced location-based interference after direction-incongruent trials and increased direction-based interference after location-incongruent trials, depending on the relative strengths of "location by direction" and "direction by location" activation. The results of Experiment 2 are consistent with the semantic generalization hypothesis if it is assumed that activation of a location code by perceiving a corresponding arrow outweighs activation of a direction code by perceiving a corresponding stimulus location under the conditions set up in the experiment.

GENERAL DISCUSSION

The present article deals with trial-to-trial modulations of interference evoked by processing a distractor stimulus feature, focusing on reductions of interference evoked by one distractor dimension after conflict evoked by a perceptually different distractor dimension. The presence or absence of such between-dimension Gratton effects bears theoretical importance for two reasons. First, the specific pattern of distractor dimensions for which between-dimension Gratton effects are found may provide insights regarding the specific processes of conflict detection and adaptation. Second, findings of between-dimension Gratton effects with an experimental set-up like the one realized in the current study might provide valuable evidence for the notion of attentional conflict adaptation as such. This is because so far within-dimension Gratton effects could not be replicated in conditions of rigorous control of feature sequence and distractor-target/response contingencies unless the task used was divided into a pair of non-overlapping two-choice tasks (Freitas and Clark, 2014; Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014). Investigations of between-dimension Gratton effects offer a different method of feature sequence control by including, with equal probability, data from trials with all kinds of feature repetitions and alternations in all congruency level sequences. Although the use of a single two-choice task precludes considering only complete feature change trials, observing Gratton effects under these conditions would broaden the empirical basis for the idea of attentional conflict adaptation.

In the current study, we used lateralized responses and combined interference evoked by left- vs. right-sided presentation of the stimulus and by stimulus shapes pointing to the left or right. Distractor dimensions involving left/right variations have been combined in at least three previous studies in which between-dimension Gratton effects were observed. First, as described in detail in the Introduction, Kunde and Wühr (2006, Experiment 2) used arrow stimuli, pointing to the left or right, presented at a left- or right-sided location. Similarly, Freitas and Clark (2014) combined different versions of a left-right spatial Stroop task with a flanker task comprising left-pointing vs. right-pointing arrows or pictures of left-pointing vs. right-pointing hands (Experiment 2), as well as a "Trajectory Stroop" task with a flanker task which again both comprised distractor stimuli pointing in one of two possible directions (Experiments 3A and B). Finally, Notebaert and Verguts (2008) used left- and right-sided stimulus presentation and SNARC (Spatial Numerical Association of Response Codes) correspondence (Dehaene et al., 1993). In that study, participants made lateralized key presses to lateralized Xs as well as to centrally presented digits. The SNARC effect is characterized by facilitation of left-sided responses when the value of a to-be-classified digit is small and of right-sided responses when the value of a to-be-classified digit is large. Thus, the two distractor dimensions overlapped semantically on the left/right dimension. Between-dimension Gratton effects were found, albeit this was confined to a situation in which both digits and Xs required the same type of judgment (i.e., presentation format normal vs. italics, condition 1) and did not occur when the Xs required a different judgment (i.e., color, condition 2).

In light of the fact that a considerable number of previous studies have failed to obtain between-dimension Gratton effects, the results of the experiments of the current study (i.e., between-dimension Gratton effects for three of four comparisons) accords with the assumption that semantic overlap between distractor dimensions plays a facilitative role in generating such effects. It is conceivable that semantic overlap of distractor dimensions promotes representing both dimensions in a linked structure, thereby possibly enhancing the likelihood of generalized conflict adaptation. Although we cannot dismiss this possibility, an alternative explanation seems better suited to account for the overall pattern of results we obtained. Specifically, we hypothesized that overlap between abstract stimulus features may result in activation of a stimulus feature code in episodic memory by perception of a semantically related feature of the other dimension. With this assumption it is possible, in principle, to account for between-dimension Gratton effects in terms of processing disadvantages on between-dimension congruency level alternations due to partial repetitions regarding the responses and abstract stimulus features.

Consistent with this hypothesis, the between-dimension Gratton effects in Experiment 1 were confined to abstract match trials, whereas there was no trial-to-trial modulation of the congruency effect on abstract mismatch trials, a result difficult to explain by the assumption of (dimension-unspecific) conflict adaptation. In Experiment 2, the task required responding to the direction of a target stimulus. Although this manipulation did not alter the sequences of concrete and abstract distractor and response features, it added perceptual overlap of the target stimulus dimension with the direction distractor dimension (and also semantic overlap of the target stimulus dimension with both distractor dimensions). This arrangement resulted not only in an overall larger congruency effect evoked by the direction distractor but also yielded a more complicated pattern of between-dimension Gratton effects. Precisely, whereas a between-dimension Gratton effect, unaffected by the abstract feature sequence, occurred for the direction dimension, the location dimension was associated with a between-dimension Gratton effect for abstract match trials but a reversed Gratton effect for abstract mismatch trials. Viewed from a conflict adaptation perspective, this pattern of results would suggest that attentional focusing was stronger after (direction-) congruent than after (direction-) incongruent trials on abstract mismatch trials. There seems to be no straightforward reason for this assumption. By contrast, the semantic generalization hypothesis offers a plausible explanation for this pattern of findings.

Specifically, considering the possibility of different strengths of abstract feature sequence effects on location-direction and direction-location transitions, three different patterns of a three-way interaction of congruency of the current trial, congruency regarding the other distractor dimension on the preceding trial, and the abstract feature sequence (match vs. mismatch) are conceivable. The results obtained in Experiments 1 and 2 constitute one of them each. Given the absence of an independent measure of relative strength of abstract feature sequence effects, this interpretation can only be applied with caution. On the other hand, because all of the three possible result patterns involve a modulation of the sequential congruency effect by the sequence of abstract stimulus and response features, they seem difficult to account for in terms of conflict adaptation. Further research regarding semantic generalization of feature integration effects could be undertaken by means of manipulating semantic overlap between prime and probe stimuli in Hommel's feature integration paradigm (e.g., Hommel, 1998; Hommel and Colzato, 2004). Such manipulations may be useful, in particular, to explore possible strength asymmetries.

As noted in the Introduction, recent studies have provided strong evidence for conflict adaptation, uncontaminated by feature repetitions and biased distractor-target/response contingencies. Investigating sequential congruency effects evoked by different perceptual dimensions complement this approach and seem particularly useful to examine the question of specificity vs. generality of conflict adaptation. Providing initial evidence for semantic generalization effects of stimulus and response feature integration, however, our current study demonstrates a limitation in attributing between-dimension Gratton effects to generalized conflict adaptation if the two distractor dimensions overlap semantically. Such inference seems justified only if between-dimension Gratton effects are consistently found not only for abstract match trials but also for abstract mismatch trials, for both distractor dimensions that are combined in the experiment.

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Even with time, conflict adaptation is not made of expectancies

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In conflict tasks, congruency effects are modulated by the sequence of preceding trials. This modulation has been interpreted as a strategic reconfiguration of cognitive control, depending on the amount of conflict encountered on the very last trial, and occurring unconditionally whenever there is time to produce it (Notebaert et al., 2006). Jiménez and Méndez (2013) arranged a 4-choice Stroop task with a response-to-stimulus interval (RSI) of 0 ms, and they found that, under these conditions, congruency effects may become dissociated from the explicit expectancies assessed over analogous, but independent, trials. The present study generalizes this phenomenon to a condition with larger RSI, and it shows that participants' performance does not rely on expectancies unless the task includes a specific requirement to generate and report on these expectancies. The results are interpreted as providing new insights with respect to the status of conflict adaptation effects.

Keywords: conflict adaptation, stroop task, expectancies, congruency sequence effect, cognitive control, reactive control

INTRODUCTION

Is *conflict adaptation* an illusion? The response to this question may depend on the meaning of the italicized expression. If it is taken as a descriptive label, referring to the fact that congruency effects are adaptively modulated by the congruency of the previous trials, then we will argue that this is not an illusion, but rather it is a pervasive phenomenon resulting from the highly dynamic and adaptive nature of the cognitive system, which always takes advantage of its previous experience to process and respond to the upcoming events in ways similar to those practiced in the past. If, on the other hand, conflict adaptation is strictly defined as the result of a top–down expectancy, or as a strategic reconfiguration of the cognitive control system depending on the conflict encountered on the very last trial, and occurring unconditionally whenever there is time to produce it, i.e., as a result of a control process which takes about 200 ms to complete, as suggested by Notebaert et al. (2006), then we will contend that this is more an exception than the rule, and that this kind of process is only activated under very specific conditions.

Cognitive conflict arises whenever there are two features in a display which are potentially incongruent with each other. In the traditional Stroop task, for instance, participants are told to respond to the color in which a word is written, and congruency effects refer to the fact that people respond faster when the color is congruent with the meaning of that word (e.g., "GREEN" written in green), than when they are incongruent with each other (e.g., "GREEN" written in red). The most popular way to analyze conflict adaptation in this context has been to look at the "congruency sequence effect" (CSE), which arises as a difference in the effect of congruency depending on the congruency of the previous trial. As a rule, the effect of congruency tends to increase after a congruent trial, and it tends to decrease after an incongruent trial. However, in those experiments using fewer than four different stimuli and responses, it becomes problematic to distinguish between the alleged effects of conflict adaptation and those potentially caused by episodic memory factors, such as the immediate repetition of a trial (Mayr et al., 2003), or the repetition of a feature that reappears immediately in a different role (i.e., a target feature reappearing as a distractor, or vice versa, Hommel et al., 2004).

To avoid both total and partial repetitions, Jiménez and Méndez (2013) arranged a 4-choice Stroop task, and they grouped the four possible colors in two alternating pairs, so that trials displaying the word "RED" or "GREEN" printed in red or green, alternated with trials showing the word "BLUE" or "YELLOW" printed in blue or yellow. These conditions, which are structurally analogous to those used in some variants of the flanker task (Mayr et al., 2003) or of the prime-probe arrow task (Kim and Cho, 2014; Schmidt and Weissman, 2014), served not only to avoid immediate feature repetitions, but also to maintain a relatively high proportion of congruent trials (50%), without associating the distractors more often with the congruent response than with any of the possible incongruent responses (see Schmidt and De Houwer, 2011 or Mordkoff, 2012, for discussions about how contingency learning may get confounded with the CSE in congruency tasks). In order to reduce the potential effects of explicit expectancies, Jiménez and Méndez (2013) set the response-to-stimulus interval (RSI) to 0 ms, and they found that the CSE disappeared under these conditions, at least when it was measured in the standard way, as the impact of the congruency of the last trial on the congruency effect measured on the following trial. However, when the congruency of a larger set of previous trials was taken into account, they obtained a significant linear trend, showing that the effect of congruency was inversely proportional to the amount of conflict accumulated over the last few trials. Thus, the effect became maximal after a run of three consecutive congruent trials (C,C,C,-), but it decreased progressively after runs of two (I,C,C,-) or just one (x,I,C,-) previous congruent trial, and it decreased further after runs of one (x,C,I,-), two (C,I,I,-), or three (I,I,I,-) consecutive incongruent trials[1].

Interestingly, the linear pattern observed in the measures of reaction time (RT) from Jiménez and Méndez (2013) could not be explained in terms of the explicit expectancies developed over those larger contexts, since those expectancies were measured independently over different blocks, and they revealed the development of a bias opposite to the effects observed in the measures of RT. According to the gambler's fallacy (Jarvik, 1951), participants' expectancies were biased to predict an incongruent successor after a series of two or more consecutive congruent trials, whereas the RT measured in those low-conflict contexts showed the largest advantage in favor of responding to these supposedly "unexpected" congruent trials. Reciprocally, after a series of two or more incongruent trials, participants reported to be expecting a change to a congruent successor, but these high-conflict contexts resulted in the minimal difference in RT between responding to a congruent and to an incongruent successor.

The dissociation found in Jiménez and Méndez (2013) between explicit expectancies and long-range conflict adaptation effects was interpreted by the authors as indicating that explicit predictions would not be affecting performance in speeded conditions, but that the observed adaptation effects would reflect an *inertial* adaptation to the amount of conflict (or lack of conflict) experienced over the last few trials, which would improve responding to those trials which make analogous control demands to those made by the series of previous trials (see also Lamers and Roelofs, 2011; Schlaghecken and Martini, 2012, for similar conclusions)[2]. However, given that this pattern of results had been obtained in conditions which minimized the chances of developing and exploiting any explicit prediction, and in which those expectancies were measured over independent blocks, differing widely in their temporal arrangement with respect to the regular Stroop blocks, we set to conceptually replicate these results under temporal conditions which may leave enough room for strategic processes to operate. According to Notebaert et al. (2006), an RSI of 200 ms might be enough to produce a top-down reconfiguration of the control system. However, because recent parametric studies have documented that the CSEs are usually larger with RSI between 500 and 1000 ms (Egner et al., 2010; Duthoo et al., 2014) we decided to set a fixed RSI of 750 ms, that could be long enough to allow for the development of strategic operations, but not so long as to dilute the effects of the series of previous trials.

MATERIALS AND METHODS

The experiment was conducted in accordance with the Spanish regulations on behavioral research. Eighteen students of psychology from the University of Santiago de Compostela participated in the experiment in exchange for a monetary fee. The procedure closely followed that of Experiment 1 from Jiménez and Méndez (2013), with the exception that the RSI was fixed at 750 ms. The task required participants to respond to the color of a word that might be written in red, blue, yellow, and green, by using, respectively, the keys corresponding to the letters "z," "x," "n," and "m," which were covered by congruently colored stickers. Responses were emitted using the index and middle fingers of both hands. After a short practice block in which participants got familiar with the mapping between colors and keys, using words unrelated to the colors, participants completed five experimental blocks with Stroop stimuli, consisting of the Spanish words for red (*rojo*), blue (*azul*), yellow (*amarillo*), and green (*verde*). Participants were instructed to ignore the meaning of those words, and to respond exclusively on their color. They were also informed that the color could be congruent with the word meaning in approximately a half of the trials, but that color and word meaning would be incongruent with each other in the other half of trials. Errors were explicitly marked by a tone, and the stimulus remained on the screen until the correct key was pressed. The next trial arose after an RSI of 750 ms, composed of a fixation point appearing at the center of the screen for 500 ms, and a blank interval of 250 ms which preceded the next word. At the end of each block, participants were informed about the percentage of correct responses produced over the last block, and they were asked to keep responding as fast as possible, while maintaining the level of errors below 10%.

To avoid color repetitions over successive trials, the four colors were grouped into alternating pairs, producing an alternation between trials showing the word "RED" or "GREEN" printed in red or in green, and trials showing the word "BLUE" or "YELLOW" printed in blue or in yellow. The colors grouped into a target/distractor pair were selected so that their responses were assigned to different hands, thus avoiding that the alternating color pattern would amount to a pattern of alternating hands (cf. Kim and Cho, 2014, Experiment 2). Each block contained 176 trials, including exactly 88 congruent and 88 incongruent trials. The runs of trials were also controlled so as to conform to those expected by chance (see Perruchet et al., 2006). Thus, we included as many runs of a single congruent trial (16) or of a single incongruent trial (16), as there were runs of two consecutively congruent trials (16) or of two consecutively incongruent trials (16). From here on, because chance probabilities of producing larger runs should be multiplicatively smaller than those of their smaller components, we included 8 runs of three consecutively congruent trials, 8 runs of three consecutively incongruent trials, four runs of four consecutively congruent trials, and 4 runs of four consecutively

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[1] A run of trials is defined backward, as the maximum number of consecutive trials from a previous context which belong to a given congruency class. For instance, a run of two congruent trials (I,C,C,-) is defined as the context in which trials n-1 and n-2 are both congruent, but trial n-3 is incongruent. A shorter run of just one congruent trial (x,I,C,-) is defined as a context in which trial n-1 is congruent (C) but trial n-2 is incongruent (I). In that case, the nature of trial n-3 becomes irrelevant (x).

[2] In this context, we will refer to the pattern obtained by Jiménez and Méndez (2013) as a conceptual "dissociation" between expectancies and conflict adaptation effects, even though in statistical terms their results showed a negative association, rather than a statistical dissociation. Given that it is conceptually very implausible that expecting a congruent successor could directly result in a slower RT to these expected trials, the conclusion inferred from that negative association was that expectancies and CSE should be driven by independent factors: whereas the CSE could reflect an inertial adaptation to the amount of conflict experienced over the last few trials, expectancies would be built exclusively when they are explicitly required, and they would be modulated by some sort of anti-inertial, or compensating bias, resembling the gambler's fallacy.

incongruent trials. The probability of producing still larger runs by chance was too low to permit a reliable measure of their effects, and therefore we set length-four as the maximum length of trials of the same type. All these runs were randomly intermixed for each block and participant, with the constraint that congruent and incongruent runs should alternate with each other, so as to avoid producing larger runs out of the concatenation of shorter ones. As a consequence of this specific design, not only the proportion of congruent and incongruent trials, but also the conditional probabilities of finding repetitions or alternations of congruency were balanced for each individual block.

Blocks 1, 3, and 4 were arranged as standard Stroop blocks. In blocks 2 and 5 participants also responded to regular Stroop trials, but after each of these trials they were presented with an explicit measure of their expectancies. At the beginning of each of these Expectancy blocks, participants were awarded with 100 points, and they were asked to bet 3, 2, or 1 of their points depending on the certainty with which they could predict that the next trial was going to be congruent or incongruent: (3) "sure," (2) "fairly sure," or (1) "guessing." Participants reported their bets verbally, and the corresponding score was entered by the experimenter manually using a second keyboard. The next trial appeared 750 ms after the bet was entered, including an update of their remaining points, together with the next Stroop trial, which also served as an indirect feedback on the accuracy of the previous prediction.

RESULTS

Jiménez and Méndez (2013) analyzed RT exclusively over the regular Stroop blocks, mainly because in their Expectancy blocks the predictions were entered by the participants with the aid of a computer mouse, which forced them to continually shift their right hand from the keyboard to the mouse, and then back to the keyboard. In the Expectancy blocks from the present experiment we replicated the original procedure with two exceptions: participants reported their bets verbally, while maintaining their fingers on the response keys, and the encoding of each prediction was entered in the computer by the experimenter, and was followed by the next Stroop trial after an RSI of 750 ms, just as during the standard Stroop task. In this way, even though our main interest was still focused on the analysis of the CSE observed during the standard Stroop blocks, we were also able to explore the effects of congruency that might be observed on the Expectancy blocks. Duthoo et al. (2013) reported that a close association between expectancies and congruency effects was obtained when both measures were taken on the very same trials. Thus, an additional objective of this study was to ascertain whether this association between expectancies and congruency effects could be extended to the regular Stroop trials when participants have enough time to develop a prediction, or whether such association could be rather restricted to conditions in which expectancies were generated in response to explicit task demands.

Alpha level was set at 0.05 for all reported analyses. Wherever there was a risk of violation of the sphericity assumption, we relied on the Greenhouse–Geisser $\hat{\varepsilon}$-corrected p values, but we reported the nominal degrees of freedom for simplicity. We will restrict the report to the analyses of RT, but we confirmed in each case that the reported effects could not be explained in terms of a trade-off between speed and accuracy. In the Stroop blocks, we analyzed the effect of congruency, the first-order (i.e., standard) CSE, and the progressive CSE, defined as the progressive changes in the effect of congruency which depended on the type and length of the last run of trials[3]. As for the Expectancy blocks, we assessed participants' expectancies in the contexts defined by the same runs of trials, and we also analyzed whether these expectancies were consistent with their speeded performance, either during the regular Stroop blocks, or during the same blocks in which the expectancies were measured.

STROOP BLOCKS

As in Jiménez and Méndez (2013), we excluded the first trial from each block, those trials containing an error, and the trial immediately following an error, as well as outliers, defined as those trials with RT straying more than 3 standard deviations from each block and individual mean. In total, 6.9% of the trials from the three Stroop blocks were excluded by applying these criteria. Data from these three blocks were collapsed together, in order to produce a sufficient number of observations even for larger contexts. The measure of RT was submitted to an analysis of variance (ANOVA) with Congruency (2) and Previous Congruency (2) as repeated factors. This analysis showed a strong effect of Congruency, $F(1,17) = 36.30$, $p < 0.001$; $\eta_p^2 = 0.68$, but not a main effect of Previous Congruency ($F < 1$). A significant Congruency × Previous Congruency interaction, $F(1,17) = 5.17$, $p = 0.036$; $\eta_p^2 = 0.23$, indicated that the Stroop effect was significantly larger after a congruent trial (52 ms) than after an incongruent trial (36 ms, see **Figure 1A**). Thus, the standard CSE, which had not been obtained in the original experiment by Jiménez and Méndez with an RSI of 0 ms, was observed in this case by using a larger RSI, and in conditions in which these sequential effects were properly distinguished from the potential influence of feature repetitions (cf. Mayr et al., 2003; Hommel et al., 2004), as well as from contingency learning confounds (Schmidt and De Houwer, 2011; Schmidt and Weissman, 2014).

The analysis of the progressive variation of these congruency effects depending on the type and length of the previous run of trials was conducted using context (6: runs of 3, 2, or 1 congruent trials, and runs of 1, 2, and 3 incongruent trials) and Congruency (2) as repeated factors. Again, this analysis showed a robust effect of Congruency, $F(1,17) = 28.10$, $p < 0.001$; $\eta_p^2 = 0.62$, but not an effect of context ($F < 1$). The Congruency × Context interaction was also significant in this analysis, $F(5,85) = 3.35$, $p = 0.02$; $\eta_p^2 = 0.17$, and it was qualified by a significant linear contrast, $F(1,17) = 10.40$, $p = 0.005$; $\eta_p^2 = 0.38$, which confirmed that the effect of congruency was inversely proportional to the amount of conflict accumulated over the last few trials. Thus, it reached larger values in the context of previous congruent trials (62, 63, and 41 ms, respectively, after runs of 3, 2, or 1 previous

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[3] Regardless of the nominal "runs" of trials introduced by design to generate a balanced sequence, these analyses were conducted considering all the trials satisfying the criteria for a given type and length of context. Thus, for instance, the second congruent trial from a nominal run of three consecutive congruent trials (C,**C**,C,-) was not only the context for a forthcoming successor, but also the successor of a shorter context.

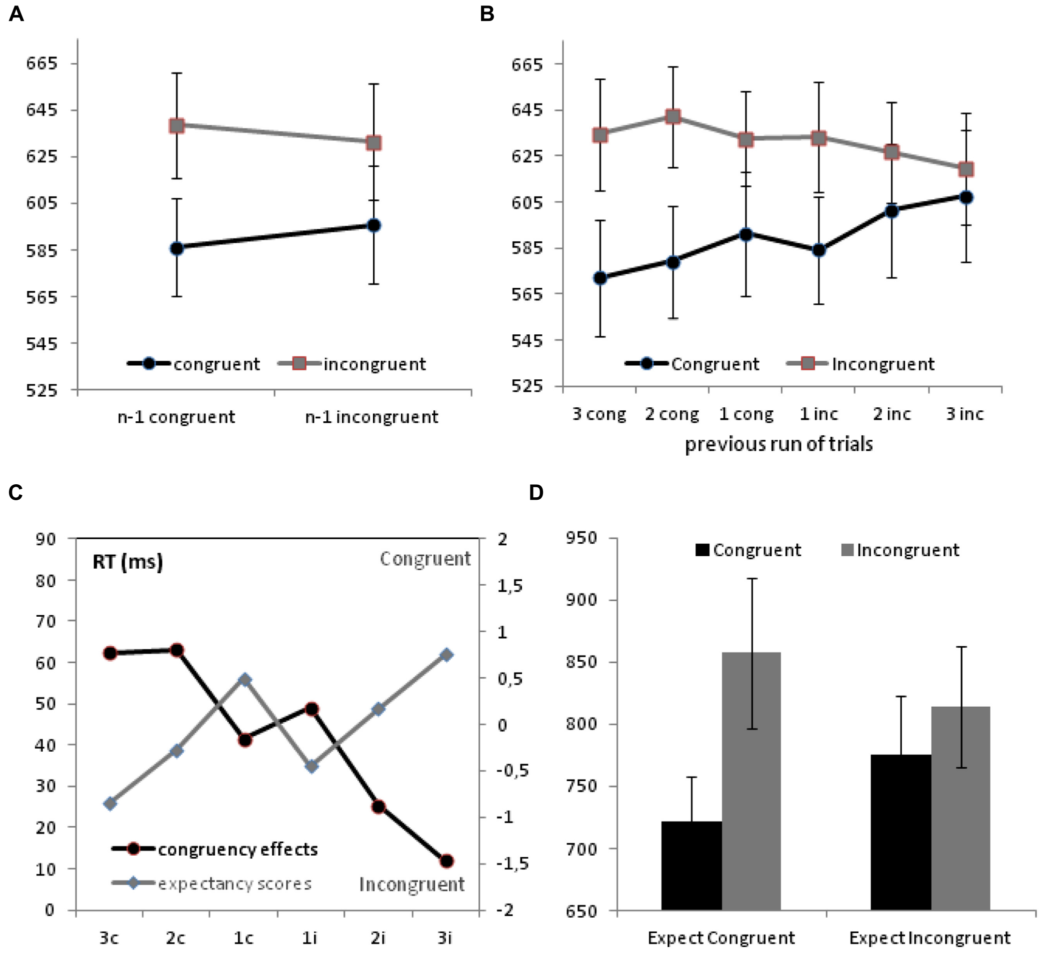

FIGURE 1 | (A) Congruency sequence effects (CSE). **(B)** Progressive variation of the congruency effects depending on the type and length of the previous run of trials. **(C)** Dissociation between the Congruency effects as taken from the standard Stroop blocks (left axis) and the expectancy scores taken from the Expectancy blocks (right axis). **(D)** Congruency effects as measured on the Expectancy blocks, represented separately for trials in which participants expected a congruent or an incongruent successor.

congruent trial), and decreased to values of 49, 25, and 12 ms in the contexts defined by 1, 2, or 3 consecutive incongruent trials (see **Figure 1B**). This pattern of results was largely consistent with that found in Jiménez and Méndez (2013), and it showed that the conflict adaptation effect did not arise immediately after a single trial of each type. Thus, even though we obtained a significant CSE when we assessed the effect in the standard way, it is important to notice that this sequential effect depended on the accumulation of several previous trials of the same type, and it was not observed if we remove the impact of larger runs and compare specifically the congruency effect provoked by a single congruent trial (x,I,C,-), with that obtained after a single incongruent trial (x,C,I,-). In this case, the effect of congruency was numerically smaller after a congruent trial (41 ms) than after an incongruent trial (49 ms).

EXPECTANCY BLOCKS

Participants' expectancies were coded for each trial as in Jiménez and Méndez (2013), by scoring 1, 2, or 3 points for the respective bets in favor of a congruent successor, and by changing the sign to −1, −2, or −3 for the corresponding bets made in favor of an incongruent successor. We collapsed those values over the two expectancy blocks, and conducted an ANOVA on these scores with context (6) as a single repeated factor. The analysis showed that the expectancy scores were significantly affected by the Context, $F(5,85) = 6.35$, $p < 0.01$; $\eta_p^2 = 0.27$. As predicted by a repetition expectancy account, participants predicted a congruent successor after a single congruent trial (0.50), and an incongruent successor after a single incongruent trial (−0.44). These two predictions were significantly different from each other, $t(17) = 3.68$, $p = 0.002$. In contrast, after a longer series composed of two or three trials of the same type, participants predicted an alternation pattern, as if they fell into the gambler's fallacy. Specifically, after a row of three congruent trials participants predicted an incongruent successor (−0.85), whereas after a similar row of three incongruent trials they reported to expect a congruent successor (0.76). These two predictions were also significantly different from each other, $t(17) = 3.15$, $p = 0.006$.

The compensation bias arising in the expectancy scores replicated the results reported in Jiménez and Méndez (2013), and it stood in sharp contrast with the RT distribution obtained over the regular Stroop blocks. **Figure 1C** represents the congruency effects observed on the relevant contexts from the standard Stroop blocks, together with the expectancy measures taken over analogous contexts from the Expectancy blocks. As it can be observed, the figure shows a striking dissociation between both measures, showing larger increases of the effect of congruency precisely in those contexts in which participants declare to be expecting an incongruent successor (i.e., after runs of two or three consecutive congruent trials), and showing larger decreases of this effect in those contexts in which participants reported to be expecting a congruent successor. This pattern is not compatible with the claim that explicit expectancies directly modulate the effect of congruency in the standard Stroop task, and therefore indicates that having enough time to elicit a prediction is not enough for participants to generate those predictions, and to rely on them in the context of a standard Stroop task.

In the face of the dissociation observed between congruency effects and expectancy scores when they are gathered from analogous contexts, but out of different blocks of trials, one may wonder whether a similar dissociation could also be obtained when one looks at both effects strictly at the same moment. The results from Duthoo et al. (2013) indicated that expectancies and congruency effects were closely associated when both measures were taken on the very same trials. Thus, to assess whether a similar association could arise over the Expectancy blocks from the present experiment, we classified each trial from the Expectancy blocks in two different categories, depending on whether the participants declared to be expecting a congruent or an incongruent successor, and we looked at the effects of congruency observed in those trials depending on these explicit expectancies. An ANOVA conducted on the RT from the Expectancy blocks with declared Expectancy (2) and Congruency (2) as two repeated factors, showed no significant effect of Expectancy, $F(1,17) = 1.22$, $p = 0.29$, but it showed a significant effect of Congruency, $F(1,17) = 22.95$, $p < 0.001$; $\eta_p^2 = 0.58$, and a significant Expectancy × Congruency interaction, $F(1,17) = 11.38$, $p = 0.004$; $\eta_p^2 = 0.40$. As shown in **Figure 1D**, these results indicate that, in the Expectancy blocks in which the participants were required to generate a prediction, the effect of congruency was indeed associated with those explicit predictions: even though the effect of congruency was significant even in those trials in which the participants declared to be expecting an incongruent successor (775 vs. 814 ms), $t(17) = 3.48$, $p = 0.003$, the effect was much larger when they reported to be expecting a congruent successor (722 vs. 857 ms).

DISCUSSION

This study replicated a previous dissociation reported in Jiménez and Méndez (2013) between explicit expectancies and conflict adaptation effects, in conditions in which the effects of total and partial repetitions were controlled (cf. Mayr et al., 2003; Hommel et al., 2004), and in which the measures of expectancies and the effects of conflict adaptation were assessed independently over different sets of Stroop blocks. The results indicate that the dissociation originally obtained using an RSI of 0 ms can also be extended to less restrictive temporal conditions, and particularly that it can be generalized to an RSI of 750 ms, an interval which should leave plenty of time for any potential strategic adjustment to take place (cf. Notebaert et al., 2006). Under these conditions, the explicit predictions elicited in the context of the Expectancy blocks reflect a compensation bias, which tends to predict a change after a series of two or more trials of the same congruency type. Thus, participants report to be expecting an incongruent successor after a run of two or more congruent trials, and a congruent successor after a series of two or more incongruent trials. In contrast, the effect of congruency, as measured over the standard Stroop blocks, shows an opposite pattern that decreases progressively with the accumulation of conflict, thus growing to their maximal scores after a series of consecutive congruent trials, and decreasing to their minimal values after a series of consecutive incongruent trials. Thus, in sharp contrast to any expectancy account of the CSE, these results indicate that the effect of congruency decreases precisely in those contexts in which the participants report to be expecting a congruent successor, and reaches its maximal levels in those contexts in which participants report to be expecting an incongruent successor.

Interestingly, the dissociation observed between congruency and expectancy scores when both measures were taken from independent blocks was no longer maintained when they were taken from the very same trials, that is, when the congruency effects were computed in a context that explicitly required participants to elicit an explicit expectancy on each trial. In those conditions, which resembled those arranged by Duthoo et al. (2013), we found an association between expectancies and congruency effects, indicating that the reported expectancies were indeed efficacious to modulate RT when they were actually elicited in response to an expectancy test. Thus, the failure to obtain a direct effect of the expectancies over the standard Stroop blocks, together with the observation that this effect exists in the expectancy blocks, could be taken to indicate that such explicit expectancies are not built by default, even if there is enough time available to do that, but that they may be generated on request, and of course in that case they affect performance.

The most important discrepancy between the results reported by Duthoo et al. (2013) and those obtained in the present experiment, and in the previous experiments by Jiménez and Méndez (2013), refers to the specific pattern of expectancies observed in each paradigm. Thus, whereas Duthoo et al. (2013) reported that their participants kept predicting repetitions even after a series of two or three trials of the same type, according to what they dubbed as "the hot-hand fallacy," our participants showed a less extended propensity to predict repetitions, and they felt quickly into the opposite "gambler's fallacy." We can only speculate about the possible source of this empirical difference, but we surmise that the use of just two colors in the case of the study by Duthoo et al. (2013) instead of the four different colors arranged in our paradigm, may be partially responsible of producing a difference in the perceived likelihood of repetitions, and may ultimately affect their expectancy scores. In any case, our results did closely replicate the dissociation pattern first reported by Jiménez and Méndez (2013), and they confirmed that such dissociation is not exclusive of those speeded conditions which leave no room for top–down preparation, and that the effect of explicit predictions may be absent unless they are explicitly requested.

Thus, coming back to the original question underlying this research topic, about the status of conflict adaptation effects, we would like to conclude by suggesting that many of the so-called "reactive" conflict adaptation effects, rather than being exerted by some "shadow" set of control mechanisms, which exert the same functions as those fulfilled by the proactive mechanisms, but only in a faster, more automatic, and less conscious mode, could be better explained as the integrated outcome of a mixture of mechanisms which may have not been designed specifically for such control purposes, but which take advantage of the system's past experience to respond to new events in ways similar to those which were proven effective in the past. From this point view, finally, these mechanisms, which may comprise those processes underlying phenomena such as perceptual priming, process priming, episodic memory, implicit contingency learning, temporal learning, and the like, perhaps should not be taken as potential confounds, or as alternatives to the genuine "mechanism" of reactive control. Rather, they could just be considered as components of the cognitive toolbox which, together, implement this delicately adaptive, automatic, complex, and highly dynamic, function of control.

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Transferring control demands across incidental learning tasks – stronger sequence usage in serial reaction task after shortcut option in letter string checking

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After incidentally learning about a hidden regularity, participants can either continue to solve the task as instructed or, alternatively, apply a shortcut. Past research suggests that the amount of conflict implied by adopting a shortcut seems to bias the decision for vs. against continuing instruction-coherent task processing. We explored whether this decision might transfer from one incidental learning task to the next. Theories that conceptualize strategy change in incidental learning as a learning-plus-decision phenomenon suggest that high demands to adhere to instruction-coherent task processing in Task 1 will impede shortcut usage in Task 2, whereas low control demands will foster it. We sequentially applied two established incidental learning tasks differing in stimuli, responses and hidden regularity (the alphabet verification task followed by the serial reaction task, SRT). While some participants experienced a complete redundancy in the task material of the alphabet verification task (low demands to adhere to instructions), for others the redundancy was only partial. Thus, shortcut application would have led to errors (high demands to follow instructions). The low control demand condition showed the strongest usage of the fixed and repeating sequence of responses in the SRT. The transfer results are in line with the learning-plus-decision view of strategy change in incidental learning, rather than with resource theories of self-control.

Keywords: incidental learning, information reduction, serial reaction task, transfer, cognitive conflict, instruction following, pliance

INTRODUCTION

The human factors literature counts many cases where, with experience, people change from processing a task as instructed to applying a shortcut (Reason, 1990; Niessen et al., 1999; Underwood et al., 2002). This has triggered experimental work on incidental learning to explore the role of cognitive control in strategy change (e.g., Strayer and Kramer, 1994; Haider and Frensch, 1999; Touron and Hertzog, 2004a,b; Haider et al., 2005; Hoyndorf and Haider, 2009). In some experimental setups participants who had discovered a shortcut were faced with high vs. low demands to adhere to instruction-coherent task processing instead of applying the shortcut. For instance, Gaschler and Frensch (2009) instructed participants to check strings for alphabet errors (see **Figure 1A** for an example). With practice, participants could learn that some string positions rarely contained alphabet errors so that time could be saved by skipping these positions when checking the strings. Experimental conditions differed in the amount of alphabet errors in these less relevant string positions. Disregarding the instruction to exhaustively check the strings led to few errors for one group of participants (low demand to secure adherence to instructions). On average this group showed a higher rate of shortcut usage than the group for which more errors would have resulted from disregarding the instructions (high control demand).

Importantly, the number of errors that one would commit using the shortcut seemed to affect performance by influencing the probability that a participant fully used the shortcut vs. refrained from using it. Thus, an all-or-non adjustment of control was observed. While some participants started to use the shortcut on all following trials after some practice, others completely refrained from using it. Conflict level (i.e., level of errors implied by shortcut usage) was influencing how many of the participants used the shortcut, rather than to what extent they used it. The link between conflict level and shortcut-based errors seems plausible, given that response errors have been tied to similar control processes and neural substrates driving behavioral adjustment as the ones involved in case of competing response tendencies, decision uncertainty and unfavorable outcomes (e.g., Ridderinkhof et al., 2004). The adjustment of shortcut usage to control demands is in line with work suggesting that strategy change in incidental learning is based on a general decision to apply or not apply an incidentally discovered shortcut (e.g., Haider and Frensch, 1996, 2002). When people apply the shortcut, they do so for practiced and novel stimuli alike (cf. Gaschler et al., 2014a). For instance, Touron

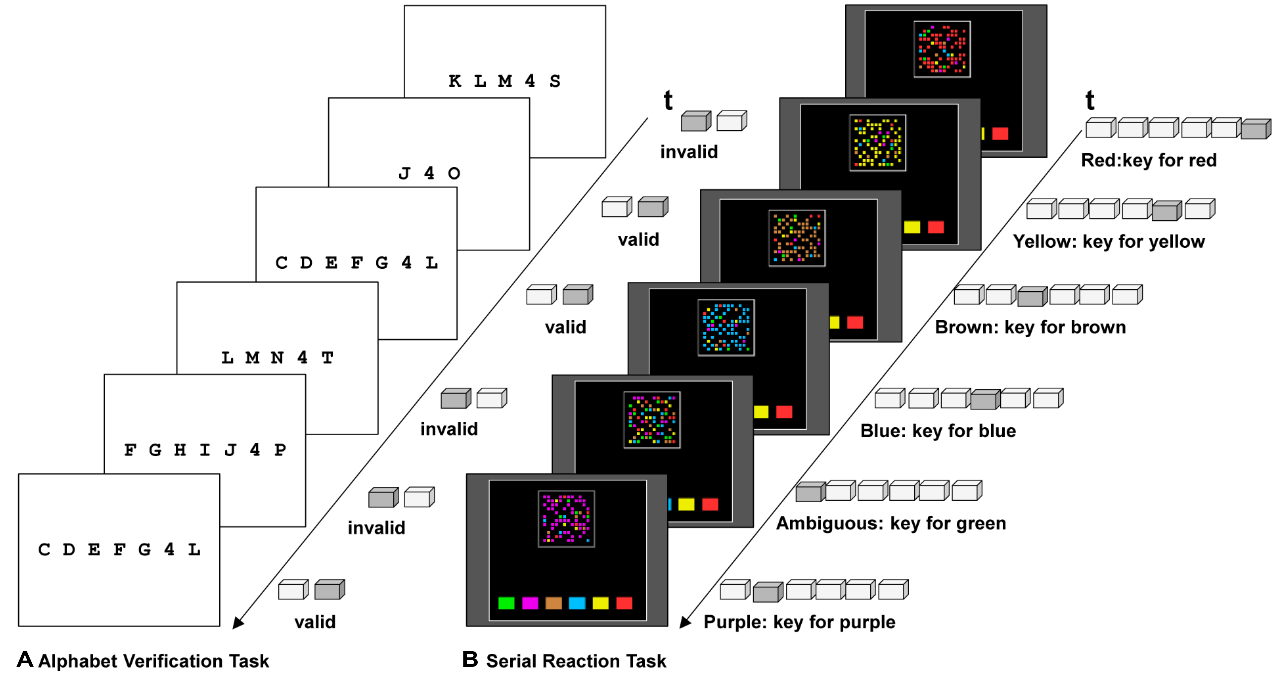

FIGURE 1 | Task material in the alphabet verification task (A) and the serial reaction task (SRT; B).

and Hertzog (2004a,b) reported that most older (as compared to younger) research participants in incidental learning experiments were reluctant to apply a shortcut they had learned. While they had sufficiently memorized the set of search items in a match-to-sample visual search task to avoid visual search in favor of faster memory search, they continued to solve the task as instructed. As the shortcut option is not mentioned in the instructions of incidental learning tasks, participants cannot be sure that the shortcut option they eventually discovered will hold throughout the experiment. In addition to the insecurity regarding the reliability of the shortcut, some participants reported reluctance to apply a shortcut because they placed more emphasis on accuracy than speed (see also Haider and Frensch, 1999).

Following or disregarding instructions is not a one-shot game. Learning theorists have suggested that people follow instructions, because they have generalized episodes in which instruction following was reinforced (e.g., Hayes et al., 1986, 2001; Törneke et al., 2008). On the one hand, participants might thus learn about contexts in which it generally pays off to follow instructions. On the other hand, they should also be able to learn under which conditions it is more advantageous to apply a shortcut instead of sticking to instruction-coherent task processing. As suggested above, people might decide to apply a shortcut, based on the experience that it rarely or never leads to errors. However, adaptation to the conflict level that a shortcut implies might not only affect processing of the current task, but also transfers to other tasks. Having experienced an incidental learning task in which a potential shortcut leads to few vs. many errors might influence the likelihood to adopt a shortcut discovered in a later task.

Theories that view strategy change as a phenomenon involving both, the learning of a shortcut option, as well as the decision to apply it or to refrain from applying it (e.g., Touron and Hertzog, 2004a,b; Gaschler et al., 2014a) can predict that experience with one incidental learning task offering a shortcut option, transfers to a second incidental learning task (see discussion for competing theories). Prior experience with low demands to refrain from shortcut usage will foster shortcut usage in the next incidental learning task. This is because the experience that shortcut application did not lead to errors in the first task, could bias the expectation that this would not be the case either in the next task (for expectation effects on conflict processing within task cf. Duthoo et al., 2013; or review by Gaschler et al., 2014b). Thus, after working on a task in which a shortcut could be discovered and adopted, participants should be more likely to use a shortcut on a task presented later on. Conversely, prior experience with a setup where the demands to refrain from applying a shortcut are high, could lead to the expectation of high control demands for the next task. In this case, participants would be more likely to refrain from using a shortcut in Task 2. A baseline condition not working on Task 1 should show intermediate levels of shortcut usage.

In the current experiment, we combined two established incidental learning tasks in order to study transfer of control demands. We used control demands in the task applied first as an independent variable and performance in the second task as a dependent variable. In two conditions participants first worked on the *alphabet verification task* (e.g., Haider and Frensch, 1996; Green and Wright, 2003; **Figure 1A**) and then on a variant of the *serial reaction task* (SRT; e.g., Nissen and Bullemer, 1987; Abrahamse et al., 2010; **Figure 1B**). In the alphabet verification task participants are instructed to tediously check alphanumeric strings. Yet they learn that these strings contain a redundant section that could be skipped. In the SRT participants receive choice reaction instructions for a consistent stimulus-response mapping. Instead of choosing reactions based on the stimulus of the current trial as instructed, they can substantially simplify task processing by learning and applying the fixed repeating sequence of stimuli and responses. While in typical variants of the SRT the sequence is long and learning remains implicit (e.g., Abrahamse et al., 2010), we used a variant with a short and simple sequence – similar to experiments in which participants have become aware of the sequence and became able to produce reactions without paying attention to the stimuli (cf. Haider and Rose, 2007; Rünger and Frensch, 2008; Schwager et al., 2012). Our variant of the SRT was constructed such that large gains in performance based on sequence knowledge were possible. Tubau et al. (2007, see also Verwey and Wright, 2014) showed that sequence knowledge allows participants to change from stimulus-based responding to memory-based responding. We used a rather simple repeating sequence. The six stimuli and keys were each presented once. The rationale behind this setup, established in Rünger and Frensch (2008), is that people would neither find it difficult to represent nor implement the shortcut option, once they have learned it – allowing us to focus on control demands (minimizing strategy performance problems).

As a novel approach to continuously assess sequence knowledge throughout practice, we included randomly interspersed ambiguous stimuli. If participants know the repeating sequence, they can give the response that would have been due according to the fixed repeating sequence if an ambiguous stimulus is presented. Otherwise they have to guess a response as the stimulus cannot be discriminated. In addition, we adopted a more traditional measure of sequence knowledge. Studies using the SRT usually measure sequence knowledge after practice with the sequential regularity by assessing the reaction time slowing in off-sequence blocks or randomly interspersed off-sequence deviant trials in comparison to trials following the sequence (e.g., Schvaneveldt and Gomez, 1998; Shanks et al., 2003; Abrahamse et al., 2010; Gaschler et al., 2012). We used this measure by randomly inserting deviant trials. We did so only at the end of practice, as reports of participants starting to rely on memory-based instead of stimulus-based response selection in the SRT come from setups using sequences without deviants (e.g., Tubau et al., 2007; Rünger and Frensch, 2008; Schwager et al., 2012). Providing a further reason for saving this measure for the end of practice, Verwey and Wright (2014) reported RT data suggesting that deviants might suppress the expression of sequence knowledge.

In summary, the present study set out to examine whether shortcut usage in one task transfers to a subsequent task. We hypothesized that prior experience with a setup where a shortcut can be safely applied should lead to increased shortcut usage in a second incidental learning task. Conversely, prior exposure to a shortcut which would lead to errors should reduce shortcut usage in the second task.

MATERIALS AND METHODS

PARTICIPANTS

One hundred and four students from different Berlin-based universities took part in the experiment and were paid € 10 (69 female; mean age 24.8 years, SD = 5.2). When entering the lab, participants were randomly assigned to the low or high control demand condition without knowledge of the experimenter. Conditions differed in the variant of the alphabet verification task that they were presented before working on the SRT. The participants of the baseline condition worked only on the SRT. Therefore, they were in the lab for a shorter time and were treated separately by the experimenter. Exclusion of four participants (see results) led to 32 participants in both, the high and the low control demand condition and 36 participants in the baseline condition. The experiment took place in the laboratories of the Psychology Department of Humboldt-Universität Berlin. We obtained informed consent from the participants and approval by the ethical review board.

MATERIALS AND APPARATUS OF TASK 1: ALPHABET VERIFICATION TASK

The stimuli in the alphabet verification task consisted of 48 alphanumeric strings (e.g., C D E F G 4 L; see **Figure 1A**), presented two times in each of the four blocks of practice. Half of the strings were *valid*, following the order of the alphabet; the other half were *invalid*, deviating from it. The digit 4 in the letter-digit-letter triplet indicated that the next four consecutive letters of the alphabet needed to be skipped at this string location, and that the string would continue with the fifth letter. Thus, "M 4 R," for instance, was to be interpreted as "M, skip N, O, P, Q, continue with R." There were either no, two or four letters forming a prefix before the letter-digit-letter triplet.

In the low control demand condition, violations of the alphabetical order only occurred in the letter-digit-letter triplet (five instead of the indicated four letters fitting the void). The prefix (i.e., the letter in front of the letter-digit-letter triplet) was always correct. In the high control demand condition, however, the prefix was free of errors only in 75% rather than 100% of the trials. The letters outside the triplet could therefore not be safely ignored. As in other work on strategy change with the alphabet verification task (e.g., Gaschler and Frensch, 2007), the length of the prefix was varied in order to obtain a reaction time measure of the extent of prefix processing. As long as participants adhere to the instructions and check the strings exhaustively, longer strings should lead to higher reaction times as compared to shorter strings. The impact of string length on RT should diminish with practice to the extent that participants stop to check the prefixes.

Each trial started with a fixation cross presented centrally for 200 ms that was followed by an alphanumeric string. Strings were centrally presented in bold Courier New font, size 26, at the center of a 17-inch CRT screen in black color on a light yellow background, controlled by a PC. The font ensured constant spacing between letters. The letters were ~1.1° × 0.9° in size. Consecutive letters appeared ~0.9° apart on the screen. After the manual response was registered, the string was erased from the screen and there was a blank interval of 200 ms before the fixation cross of the next trial appeared. Incorrect responses were immediately followed by a high tone as error signal. Participants responded by pressing either the "y" or the "," key on the second row from the bottom on a standard German PC keyboard. Half of the participants were instructed to use the "y" key to indicate that a string was valid and the "," key to indicate that the string was invalid; for the other half, the key assignment was reversed.

In the computerized instructions, the characteristics of the alphanumeric strings were described, and participants were shown how to evaluate the strings. Participants were instructed to pay attention to the entire string because errors could occur anywhere in the string. Furthermore, they were told to respond as quickly as possible while keeping the rate of errors below 10%. The alphanumeric strings used as examples in the instructions and in the 10 practice trials (triplets starting with E and F) contained violations of the alphabetical order outside the letter-digit-letter triplet and were not from the pool of material used for the rest of the task. The task was completed within ∼45 min.

MATERIALS AND APPARATUS OF TASK 2: SRT

In each trial, participants saw a random cloud of 72 dots, each colored in one of six colors for 250 ms in a centrally presented frame (**Figure 1B**). The frame was drawn in gray lines on a black background. Afterward, the cloud disappeared and the program awaited a response. A new stimulus was displayed after a response stimulus interval (RSI) of 100 ms. In order to allow for execution of fast response sequences (e.g., based on sequence knowledge), the stimulus presentation ended early in case participants responded during the stimulus presentation. Except on some irregular trials, (see below) one color was much more frequent (52 dots) as compared to the other five (four dots each) in each trial. At a distance of 3.6° beneath the 4.4° × 4.4° frame with the dots, a row of colored squares indicated the mapping between the dominant color and the response. Each color box was 1.2° high and 1° wide and the spacing was 0.5°. Participants responded with the keys on the lower row of the keyboard (X to M) using index, middle, and ring fingers which they should keep resting on the keys. The keys were numbered 1 to 6 with stickers and the mapping of colors to keys was constant throughout the task.

Unbeknownst to the participants, stimuli (dominant stimulus colors) and the required responses followed a simple fixed repeating sequence. Sequences were drawn from a pool of 24 first-order sequences of length six. Each of the six stimuli and responses occurred once and this sequence was constantly repeated. To avoid salient spatial patterns in the response positions, the sequences did not contain "runs" of three or more adjacent response locations (e.g., 1-2-3, 6-5-4 with responses numbered from left to right; cf. Rünger and Frensch, 2008). The selection of the sequences was matched between the conditions of the experiment.

Each of the seven blocks of the SRT consisted of 108 regular and 12 irregular trials. An auditory error feedback was presented during the RSI on regular trials, while any response in the irregular trials was regarded as correct. With (1) ambiguous and (2) deviant trials, we used two different kinds of irregular trials in different blocks in order to assess usage of sequence knowledge – the dependent variable of the experiment. Stimuli in irregular trials in Blocks 1 to 6 were maximally ambiguous. The cloud of dots contained dots of all colors with equal frequency. Thus, the stimulus did *not* suggest any of the six responses more strongly than the other five. Therefore, sequence knowledge should be measurable in a response bias. For instance, the ambiguous stimulus in **Figure 1B** elicits the response for green. As this response was due according to the fixed repeating sequence, such a response suggests sequence knowledge. No sequence knowledge would be evident if the participant pressed the key according to the sequence only at chance level (match in 1/6th of the ambiguous trials). If participants acquire knowledge about the fixed repeating sequence and decide to exploit it for the simplification of task processing, they should not only pass chance level in choosing responses according to the repeating sequence. Rather they should start to consistently respond according to the fixed and repeating sequence in the ambiguous trials.

In Block 7, the stimuli in deviant trials had a dominant color that did not follow the sequence. For instance, instead of a cloud of predominantly yellow dots that should appear based on the repeating sequence, a predominantly blue stimulus might be randomly inserted instead. Random deviants were drawn such that immediate repetitions of responses were avoided. Sequence knowledge was assessed as the reaction time difference between, on the one hand, the irregular trials and their immediate successors, and, on the other hand, the remainder of the trials with correct responses. We included the immediate successor of the deviant as a potentially slowed trial in order to increase the number of trials available for the RT estimate.

PROCEDURE

Except for the baseline condition, participants started the experiment with the alphabet verification task. No references were made as to whether a part of the stimuli could be safely ignored or not. After completing the alphabet verification task, the experimenter started the automatized instructions of the serial reaction time task. Participants were told that this task is a speeded forced choice stimulus discrimination task. In doing so, no underlying regularities in the task material were mentioned. The experimenter then watched the first five trials to make sure that participants had properly understood the instructions. Only after completing the SRT participants were asked whether or not (forced choice) it would have been possible for them to skip checking a part of the string positions of the alphabet verification task (see results on manipulation check). Also the experimenter inquired about verbalizable sequence knowledge (SRT). Participants were asked to recall the fixed repeating sequence or otherwise guess a sequence of six elements. For each participant, the pattern of the correctly verbalized portion(s) of the trained sequence was compared to a simulation in order to estimate the likelihood that it was based on guessing (see Rünger and Frensch, 2008). The simulation determined how often the specific pattern of correct verbalizations observed for a participant (e.g., a triplet correct) would be obtained by matching the training sequence with a randomly generated sequence 10 million times. If the specific pattern of correct verbalizations occurred with low relative frequency in random matching, it was likely not the result of guessing.

RESULTS

SCREENING OF THE DATA

Screening of the data suggested that there was no speed–accuracy trade-off. In both tasks error trials tended to be slower rather than faster as compared to correct trials. In the low control demand condition, one participant did not fully complete the alphabet verification task and three participants were excluded because of error rates higher than 30%. The mean error rate of the remaining participants of the high control demand condition ($N = 32$) and those of the low control demand condition ($N = 32$) was 7.5% for either group. See below for SRT error rates of these conditions and the baseline condition ($N = 36$).

MANIPULATION CHECKS

In the main analysis below we employed presence and variant of the alphabet verification task (high control demand condition, low control demand condition, baseline condition) as an independent variable for performance in the SRT. Beforehand, we checked whether the manipulation of the feasibility of information reduction actually led to performance effects in the alphabet verification task itself. As participants in the low control demand condition could safely skip to check some of the string positions, it was to be expected that they should be generally faster than participants of the high control demand condition. Furthermore, RTs in the low control demand condition should be less strongly influenced by string length, because the string prefixes (letters before the letter-digit-letter triplet) of varying length did not contain to-be-spotted alphabet errors and thus could be skipped. The data presented in **Figure 2** are in line with these predictions. A mixed ANOVA on RTs, including block and string length as within-subjects factor and control demand as a between-subjects factor showed a main effect of control demand condition, $F(1,62) = 7.53$, $\text{MSE} = 16480000$, $p = 0.008$, $\eta_p^2 = 0.11$, of block of practice, $F(3,186) = 76.93$, $\text{MSE} = 1601747$, $p < 0.001$, $\eta_p^2 = 0.55$, and of string length on RT, $F(2,124) = 72.43$, $\text{MSE} = 447654$, $p < 0.001$, $\eta_p^2 = 0.54$. Furthermore, there was an interaction between control demand condition and string length, $F(2,124) = 4.53$, $\text{MSE} = 447654$, $p = 0.013$, $\eta_p^2 = 0.07$, as string length was of less influence for participants of the low control demand condition than for participants who could not safely skip to check the string prefixes. With practice there was a decrease of processing of the string positions containing alphabet errors rarely or never, $F(6,372) = 2.55$, $\text{MSE} = 204073$, $p = 0.02$, $\eta_p^2 = 0.04$, for the interaction between block of practice and string length (other Fs < 1.1). Note that we applied Greenhouse–Geisser correction in the ANOVAs when necessary.

Participants proved knowledgeable about whether or not information reduction had been possible in the version of the alphabet verification task they had been practicing. When asked to guess whether or not the prefix letters in their version of the task had or had not always been in correct alphabetical order, all participants of the high control demand condition correctly stated that errors in the alphabetical order had occurred in the letters placed in front of the letter-digit-letter triplet. Four of the participants in the low control demand condition incorrectly stated that this was the case in their version of the task as well, while the others correctly reported that the prefixes had always been correct.

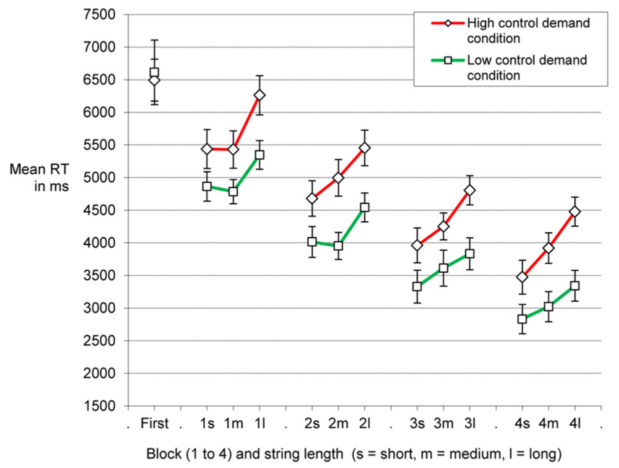

FIGURE 2 | Means of individual median reaction times. On the x-axis, reaction times for short, medium, and long strings (s, m, l) are grouped together by block in order to display the amount of processing of irrelevant information. The impact of string length on RT is stronger in the high control demand condition as compared to the low control demand condition, indicating more processing of the additional letters when information reduction is not possible. Note that error trials as well as RT of trials with violations of the alphabetical order (high control demand condition) were excluded. Note that performance for the two conditions was very similar during the very first trials of Block 1 and quickly diverged afterward. This was evident when we compared (a) the trials of Block 1 that occurred before participants in the high control demand condition encountered the first strings with violation of the alphabetical order outside the letter-digit-letter triplet with (b) the yoked trials of the low control demand condition. As soon as participants in the high control demand condition were confronted with the strings including incorrect prefixes (RT from the latter trials being excluded from main analysis) RT differences between the conditions quickly developed. Error bars: between-subjects standard error of the mean.

SLOWING BY OFF-SEQUENCE STIMULI

In line with other implicit and incidental sequence learning studies we assessed sequence knowledge indirectly by comparing trials that follow the fixed repeating sequence with off-sequence trials at the end of practice. The RT difference between regular and deviant (plus following) trials in Block 7 is displayed in **Figure 3**. The ANOVA with control demand condition as between subjects factor showed a main effect of control demand, $F(2,97) = 3.33$, $\text{MSE} = 11539.79$, $p = 0.04$, $\eta_p^2 = 0.064$. Slowing was strongest for participants in the low control demand condition and weakest for those of the high control demand condition (these conditions yielded the only significant pair comparison according to Tukey-HSD, $p = 0.04$). The baseline condition lay in between.

SEQUENCE FOLLOWED IN AMBIGUOUS TRIALS

Figure 4A suggests a practice-related increase in this dependent measure – the rate of ambiguous stimuli eliciting a response according to the repeating sequence. The mixed ANOVA with the factors block of practice and control demand condition showed a main effect of block of practice, $F(3.48,337.48) = 15.78$,

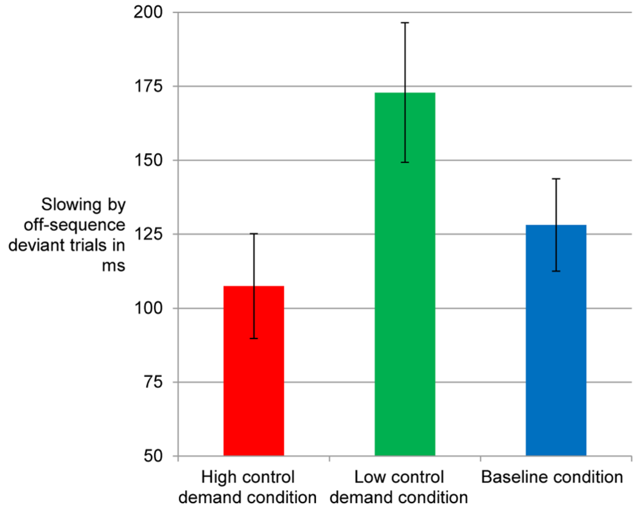

FIGURE 3 | Reaction time slowing in trials with off-sequence deviants in the SRT. Error bars: between-subjects standard error of the mean.

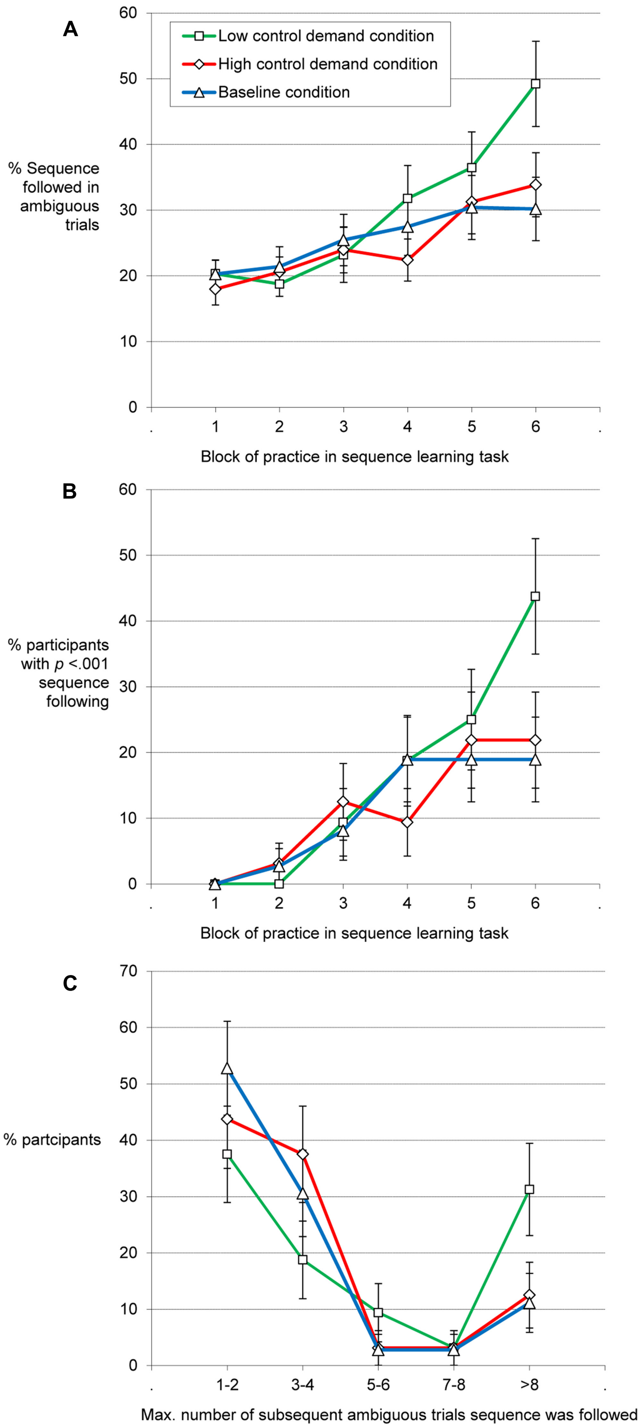

FIGURE 4 | The average rate of % ambiguous trials the sequence was followed in the SRT increases over blocks of practice (chance level = 16.7%; A). (B) Shows the proportion of participants using the sequence in at least 50% of the ambiguous trials of the respective block (i.e., $p < 0.001$ for that participant in that block). **(C)** Shows the distribution of sequence usage in the SRT. A larger proportion of participants of the low control demand condition as compared to the other conditions showed long chains of consistent sequence memory-based responses on randomly interspersed ambiguous trials. Error bars: between-subjects standard error of the mean or the proportion.

$\text{MSE} = 444.96$, $p < 0.001$, $\eta_p^2 = 0.14$, and an interaction of practice and control demand condition, $F(6.96{,}337.48) = 2.25$, $\text{MSE} = 444.96$, $p = 0.038$, $\eta_p^2 = 0.04$, but no main effect of control demand condition, $F(2{,}97) = 1.1$. The increase in sequence following across blocks was strongest in the low control demand condition. As detailed below, between-participant variability in sequence-following in ambiguous trials was substantial. Therefore, we secured that the abovementioned pattern of results also holds with a more robust statistic. For this we determined the percentage of participants per condition and block of practice who showed above chance sequence following. We determined (based on the binomial distribution) how many sequence following responses within the 12 ambiguous trials per block of practice a participant should accumulate to be classified as an above-chance sequence follower for that block. Seven of 12 responses (i.e., $>50\%$ sequence following) are sufficient for $p < 0.001$. Supporting the above analysis, the percentage of sequence followers (**Figure 4B**) showed a similar pattern as the average rate of sequence following (**Figure 4A**). It increased the most in the low control demand condition, X^2 $(2) = 6.93$, $p = 0.031$, for the across-condition comparison of the rate in the last block of practice. Note that the Block 6 rate also mirrors the overall increase with practice, as all conditions started from 0 in Block 1.

Several participants eventually started to consistently respond to the randomly interspersed ambiguous trials according to what the fixed sequence would have suggested. Run analyses were employed to explore the consistency of sequence following. Guessing should lead to sequence-followed responses on individual ambiguous trials, but not on whole runs of them. Consistent replacement of random key presses to ambiguous stimuli by sequence memory-based responses was captured by determining the maximum run length of sequence-following responses in ambiguous trials. We used the ambiguous trials as probes of sequence following that were randomly inserted into the repeating sequence of regular trials. Thus, runs span over many regular trials. For instance, a participant with a maximum run length of 30 has responded according to the fixed sequence without interruption for more than two blocks of practice (i.e., 12 ambiguous trials per block).

Cases where participants started to consistently respond according to the repeating sequence were especially pronounced in the low control demand condition. The maximum run length of sequence-consistent responses on subsequent ambiguous trials determined per participant was on average $M = 8.9$. It was $M = 4.6$ in the high control demand and the baseline condition. As depicted in **Figure 4C**, the distribution was heavily skewed in all conditions, as many participants did not show consistent usage of sequence knowledge in ambiguous trials. Yet, the low control demand condition yielded a high proportion of participants with especially long runs as compared to the other conditions. While 14 of the participants of the low control demand condition showed runs longer than four (four being the median of this condition; $p < 0.001$ for four consecutive hits; *Maximum* = 54 ambiguous trials), only six of the participants in both the high control demand condition and the baseline condition (*Maximum* = 30 and 29) showed sequence-consistent responses of the same run-length, $X^2(2) = 7.74$, $p = 0.025$. In summary, different indicators converge in suggesting stronger usage of incidentally acquired sequence knowledge following the low control demand condition compared to the high control demand condition (and intermediate performance for the baseline condition).

FOLLOW-UP ANALYSES ON ERROR RATES AND REACTION TIMES

Unexpectedly, the mean error rate for the regular trials of Blocks 1 to 6 of the SRT (**Figure 5A**) was higher for the baseline condition ($M = 5\%$) compared to the low control demand condition ($M = 2.8\%$) and the high control demand condition ($M = 3.4\%$). The baseline condition differed from the other two conditions according to Tukey-HSD (*ps* < 0.05). An ANOVA including block of practice and control demand condition showed a main effect of practice, as error rates decreased, $F(3.27,316.744) = 5.08$, MSE = 9.19, $p = 0.001$, $\eta^2_p = 0.05$, and a main effect of control demand condition, $F(2,97) = 8.16$, MSE = 31.93, $p < 0.001$, $\eta^2_p = 0.14$. There was no interaction of block and condition ($F = 1.08$).

An analysis of the error rates in Block 7 (**Figure 5B**) showed that participants produced more errors in deviant trials (compared to regular trials). Error rate increased when exclusively taking into account errors in line with the disrupted sequence, but also when only considering errors in which participants neither followed the sequence nor the current off-sequence stimulus. An ANOVA including the error rates in regular vs. in deviant trials resulted in a main effect of trial type, $F(1,97) = 32.31$, MSE = 112.2, $p < 0.001$, $\eta^2_p = 0.25$. There was neither a main effect of nor an interaction involving control demand condition (*Fs* < 1). A large proportion of errors in deviant trials were responses in line with what the repeating sequence would have suggested. A main effect of trial type (but no effects involving control demand condition) was also obtained, when comparing error rate on regular trials with the rate of sequence following in deviant trials, $F(1,97) = 5.23$, MSE = 73.82, $p = 0.024$, $\eta^2_p = 0.05$. The pattern of a higher error rate in deviant as compared to regular trials also held when only considering errors that were not in line with the response suggested by the repeating sequence, $F(1,97) = 7.18$, MSE = 29.98, $p = 0.009$, $\eta^2_p = 0.07$.

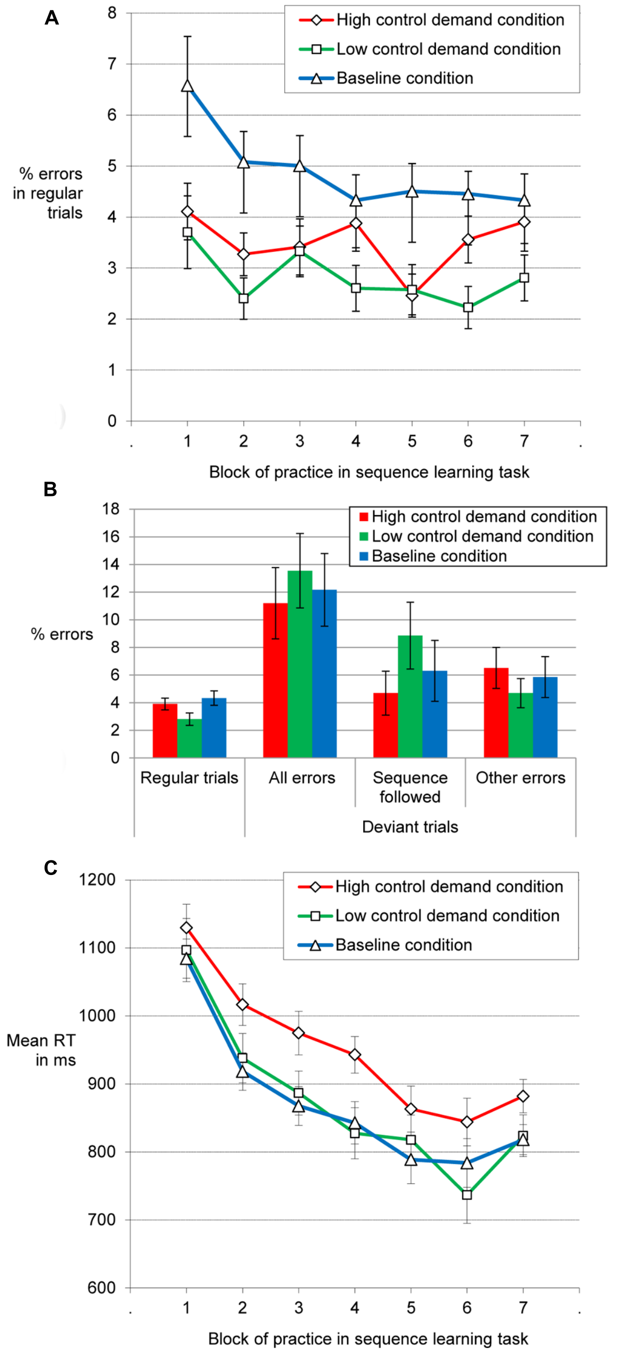

FIGURE 5 | Error rates in regular trials over blocks of practice (A), error rates and error type in deviant and regular trials of Block 7 (B), and reaction times in correct regular trials over the course of practice (C). Error bars: between-subjects standard error of the mean.

Last we analyzed how RT developed across blocks of practice in regular trials. While **Figure 5C** suggests that performance on regular trials was slowest in the high control demand condition, this was not confirmed by an ANOVA with block of practice and control demand condition as factors. We obtained a main effect of block of practice, reflecting that participants became faster over the six blocks of practice, $F(2.38,230.94) = 132.56$, MSE = 19705.33, $p < 0.001$, $\eta_p^2 = 0.58$. However, there was neither a main effect of control demand condition, $F(2,97) = 2.06$, nor an interaction of block and control demand condition, $F(4.76,230.94) = 1.59$. Note that an ANOVA involving trial type (regular trials vs. ambiguous trials) and block of practice did not show a main effect or interaction involving control demand condition either (Fs < 1).

VERBALIZABLE SEQUENCE KNOWLEDGE

The three experimental conditions did not differ with respect to the frequency with which the matches between verbalized sequence parts and practiced sequence were obtained by random matching in the simulation. The average relative frequencies were 13.39, 14.27, and 15.01%, for the low and high control demand condition and the baseline condition respectively ($F < 1$). As we administered the interview after the test block containing deviant trials, one could suspect that the measure of verbalizable sequence knowledge is too noisy to be useful. However, we obtained significant Spearman rank correlations of the measure with RT slowing on deviant trials ($r = -0.395$) and with the proportion of ambiguous trials responded to according to the fixed sequence in the last block of practice ($r = -0.501$; ps < 0.001). Thus, participants showing stronger behavioral signs of sequence knowledge also verbalized sequence patterns that were less frequently obtain in a random matching simulation (i.e., their verbalization was less likely based on guessing).

DISCUSSION

We observed transfer between two incidental learning tasks, the alphabet verification task and the SRT. Participants who had the opportunity to discover *and* apply (low control demand condition) a shortcut in the first task, were more likely to apply a different shortcut in the second incidental learning task compared to participants in the high control demand condition. Low demands to adhere to instruction-based task processing in the alphabet verification task (i.e., option to skip to check some string positions without that this would lead to errors) apparently were transferred to the SRT (i.e., respond based on sequence memory rather than based on stimuli). Less usage of sequence knowledge was observed in the high control demand condition. These participants had experienced that instruction-coherent task processing has to be maintained as a shortcut would lead to errors in the alphabet verification task. The participants of the baseline condition showed intermediate application of sequence knowledge.

The two incidental learning tasks employed were highly dissimilar in terms of stimuli, responses, and hidden regularity that could be exploited for task processing. Thus, the transfer across tasks rules out that stimulus-specific processing episodes rather than learning of control demands can account for the results. Rather, the experiment illustrates general demand effects – an issue important and hard to control in research with human participants. Hertwig and Ortmann (2001) have for instance suggested that research participants in psychological experiments often search for hidden regularities in the task material, because they suspect that task instructions convey a misleading or incomplete picture of what the experiment is really about (see also Harlow, 1949; Gaissmaier and Schooler, 2008). After taking part in an incidental learning experiment, research participants might (often falsely) assume that hidden task regularities might be waiting to be found and safe to exploit in other experiments of the same or maybe even other research labs. This might distract them from performing tasks as instructed, threatening the validity of studies not interested in incidental learning and instruction following.

As the task material of the low control demand condition was set up to support the belief that exploitable task regularities might exist, participants might have been inclined to also search and apply shortcuts in the SRT afterward. Crucially, participants in the low control demand condition experienced no costs (i.e., errors) in applying the shortcut (rather than processing the alphanumeric strings as instructed). The baseline condition tended to be more similar to the high control demand condition than to the low control demand condition. This would suggest a larger impact of experiencing the *lack* of the demand to control shortcut usage on performance in a subsequent incidental learning task (rather than experiencing the demand to continue instruction-coherent task processing). This might seem plausible if the demand to follow instructions is default and rewarded in everyday life (cf. Hayes et al., 1986, 2001; Törneke et al., 2008). Currently we cannot distinguish these variants as only the difference between the low and the high control demand condition was statistically robust.

The current study at least provides tentative evidence for distinguishing influences of control demands on applying shortcut options from influences on learning about these shortcut options in the first place (cf. ErEl and Meiran, 2011). In principle, participants in the low control demand condition might either have been better at learning about the fixed repeating sequence, better at applying it, once they have learned about it, or both. Our measure of verbalizable sequence knowledge did not differ between the control demand conditions (though it correlated with performance indicators, suggesting that it was sensitive). This suggests that the control demand conditions differed primarily in applying rather than in knowing the fixed repeating sequence in the SRT.

The finding of transfer between incidental learning tasks is remarkable given that researchers have struggled to obtain transfer between structurally equivalent thought problems (cf. Helfenstein and Saariluoma, 2006; Frensch and Haider, 2008; but see Green et al., 2010). In the current study participants seemed to transfer the knowledge that shortcut options might exist and can be safely exploited to a different incidental learning task presented subsequently. Verbal reports suggest that this knowledge was explicit. Currently we can only speculate on the role of verbal knowledge in transfer between incidental learning tasks as data for direct comparisons of transfer in incidental (i.e., with verbal knowledge) vs. implicit learning tasks (i.e., without verbal knowledge) are lacking. Note however that according to implicit learning studies at least some transfer seems to be possible even without verbal knowledge of the task regularity. For instance, Leber et al. (2009) reported that participants who have adopted one attentional set (feature search mode vs. singleton detection mode) in training transferred it to another session despite changes in the coloring of the search targets. Turk-Browne and Scholl (2009) reported that visual statistical regularities, with respect to temporal sequence, transferred to spatial sequencing and *vice versa*. Stadler et al. (2000; see also Newell and Bright, 2002) reported the transfer of implicit knowledge about defining features of number strings across formats (digits vs. words).

The finding of transfer of control demands is theoretically relevant as such task-general influences on performance imply the item-general operation of control processes. The current work might contribute to alter the perspective on cognitive control in strategy change. Some models of skill acquisition focus on the aspect that strategy change can help to overcome attention-demanding task processing by applying a (memory-based) shortcut (e.g., Logan, 1988, 1992). However, in his instance theory of automatization, strategy change (e.g., from calculating simple arithmetic problems to retrieving the answer from memory), is a mandatory consequence of task practice. As soon as the memory strength is sufficient, the shortcut is automatically applied. Importantly, transfer across tasks with different types of shortcuts operating on different types of task material is not to be expected according to the instance theory and related models of strategy change in skill acquisition (e.g., Cousineau and Larochelle, 2004), because shortcuts are based on knowledge that has to be acquired individually for each stimulus (e.g., the correct solution to an arithmetic problem). According to this perspective, strategy change relying on *automatic* memory retrieval of answers to formerly presented problems can free attentional resources. There is neither room for transfer across incidental learning tasks, nor for control processes that might modulate whether or not shortcut knowledge is applied. This changes, if participants can decide to apply or not apply a shortcut option which they have incidentally learned (e.g., Touron and Hertzog, 2004a,b; Gaschler and Frensch, 2009). According to the learning-plus-decision perspective on strategy change in incidental learning, incidentally learning about a shortcut option could lead to a demand of cognitive control — namely when shortcut application leads to errors. The current study provides first evidence for that such control demands might transfer across incidental learning tasks. It extends recent work showing that learning processes involved in strategy change can generalize across specific stimuli within a task: Strategy change is not confined to learning a shortcut specifically for each stimulus (cf. Logan, 1988, 1992), but instead transfers across stimuli within a task (cf. Gaschler et al., 2014a). For instance, Wilkins and Rawson (2010, p. 1134) conceptualized item-general practice gains as performance improvements "that accrue to all stimulus tokens of a given type, including both practiced and novel tokens of that type." The current work suggests that this might even include different tasks.

Apart from the learning-plus-decision perspective on strategy change discussed above, there is another theoretical perspective that can account for transfer between incidental learning tasks, but makes different predictions for the specific pattern of transfer that should occur: theories that places emphasis on potential psychological resources needed to refrain from shortcut usage. According to research on ego depletion (e.g., Baumeister et al., 2007; Hagger et al., 2010), working on a demanding task can exhaust a control resources that are then not available for the next task to come. Assuming that high demand conditions deplete cognitive control resources more than low demand conditions would have led to the following prediction: Working on a task that demands to refrain from using a shortcut option should have led to more shortcut usage in a later incidental learning task compared to a condition in which participants did not have to refrain from applying a shortcut in the first task. The least depletion of control resources should have taken place if participants do not have to work on a prior task at all. Thus, the baseline condition lacking experience with either variant of Task 1 should have shown the least shortcut usage. Different from these predictions, we obtained the strongest shortcut usage in the SRT in the low control demand condition. The demand to refrain from using a shortcut option in Task 1 should have diminished the capability to secure adherence to instructions in Task 2. As the alphabet verification task is tedious even when a part of the material can be skipped, the low control demand condition should have shown an intermediate level of shortcut usage, while it should have been lowest in the baseline condition (i.e., SRT only, hence least depleted). As we did not include independent measures of depletion (e.g., a pre–post-test comparison of self-reported fatigue) we are cautious to over-interpret our results with respect to resource-theories of self-control. Note that the unexpected high error rate on regular trials in the baseline condition is at odds with the resource perspective as well – rather predicting a lower error rate in the least depleting condition. Instead, our results are in line with theories that conceptualize strategy change in incidental learning tasks as a phenomenon involving (a) learning of the task regularity and (b) a decision to apply or not apply the shortcut (e.g., Strayer and Kramer, 1994; Touron and Hertzog, 2004a,b; Haider and Frensch, 2005; Haider et al., 2005).

While past work has documented that shortcut application can take place in an all-or-none manner, generalizing even to novel stimuli within a task (e.g., Gaschler and Frensch, 2007; Gaschler et al., 2014a), the current work, in addition, presents first evidence for transfer across different incidental learning tasks. Distinguishing between influences of specific processing episodes versus adaptation to general task structure is a key-problem shared by the literature on conflict adaptation (e.g., Jacoby et al., 2003; Mayr et al., 2003; Ullsperger et al., 2005; Schmidt and Besner, 2008) and the literature on strategy change in skill acquisition. One research strategy to accumulate evidence for adaptation to control demands (rather than to specific processing episodes with specific stimuli) has been the study of transfer of control from one task to another in task switching (e.g., Egner, 2008; Fernandez-Duque and Knight, 2008; but see Rünger et al., 2010). In that work the focus is on transfer of control between tasks on a trial-by-trial basis. Others, have focused on learning of parameters controlling strategy selection over many trials (e.g., Gray et al., 2006; Gaschler and Frensch, 2009; Schouppe et al., 2014). Models of strategy selection (e.g., Rieskamp and Otto, 2006; Marewski and Schooler, 2011) might be expanded such that they can capture task-general learning of applicability vs. non-applicability of shortcuts.

Last we would like to highlight that it is interesting to consider alternative task orders to study transfer across incidental learning tasks. Transfer of control demands across incidental learning tasks could be studied in either direction taking the alphabet verification task or the SRT as independent and dependent variable or *vice versa*. Control demands could be manipulated in the SRT by varying the amount of randomly inserted stimuli breaking the repeating sequence (cf. Verwey and Wright, 2014). However, for issues tied to task difficulty, we decided to use performance in the alphabet verification task as the independent variable in our experiment, varying whether participants (a) could safely apply a shortcut option (b) should not apply the shortcut or (c) did not have to work on this task at all. While the SRT is instructed as a simple choice reaction task and can be solved at reaction time levels of one second or less per trial – even without applying a shortcut – the alphabet verification task is much more tedious. Reaction times rarely reduce below three seconds per trial. From the perspective of the ego depletion theory, this task should exhaust more control resources when participants have to refrain from applying a shortcut and less, when control demands are low. Yet, even when participants can safely use the shortcut and skip to check some positions of the alphanumeric strings, each trial still contains a substantial amount of string positions to be checked. Thus, even the low control demand condition should be affected by exhaustion of control resources and thus show more shortcut usage in Task 2 as compared to the baseline condition.

In the current setup we tested whether being able to use vs. having to forego using a shortcut option in a demanding task affects shortcut usage in second incidental learning task. Reversing the order of the incidental learning tasks in future studies could additionally challenge control resource accounts. Assuming that the SRT is comparatively less demanding, withholding shortcut usage in the SRT should not lead to a substantial exhaustion of control resources. From this perspective, shortcut usage in the alphabet verification task should not differ depending on prior applicability of sequence knowledge in the SRT. The learning-plus-decision perspective on strategy change however suggests that providing participants with the opportunity to find and apply a shortcut in a relatively easy task could strengthen shortcut usage in a more difficult task provided later on. Experiments on shortcut usage in arithmetic (Godau et al., 2014) indeed reflect that that offering an easy-to-find shortcut option can increase later shortcut usage. Therefore, the sequential regularity in the SRT (which can be detected and applied rather easily) could foster later shortcut usage in the alphabet verification task, if the tasks would be applied in the reversed order.

In summary, the present study offers a cognitive control perspective on strategy change in incidental learning tasks. In line with theories conceptualizing strategy change in incidental learning as a learning-plus-decision phenomenon, we observed transfer of control demands across incidental learning tasks. This provides further evidence for that control processes can be distinguished from adaptation to the specific material practiced.

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Resolved but not forgotten: Stroop conflict dredges up the past

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The magnitude of congruency effects depends on, among other things, the specifics of previous trials. To explain these modulating effects, a host of mechanisms by which previous trials affect the processing of relevant and irrelevant information on the present trial have been proposed, including feature repetition advantages, negative priming, item-specific proportion congruency (ISPC) effects, display frequency effects, and sequential modulations of both congruency and frequency effects. However, few experiments have been designed to independently manipulate these factors. In the present study, we used a four-choice Stroop task in which we hold constant the frequencies of the stimulus features and responses, but manipulate the frequencies of their conjunctions. We modified the procedure used by Jacoby et al. (2003), under which the possible word–color pairings differed in terms of proportion occurrence, by adding neutral trials to obtain independent estimates of the effects of display frequency. The results indicate that feature repetitions, display frequency, and sequential modulations of both congruency and frequency effects all affect response time. However, no evidence for an ISPC effect was obtained; the display frequency effect measured on the neutral trials accounted for all differences in the congruency effect, as proposed by Schmidt and Besner (2008). Sequential modulations of congruency effects were observed when the overall proportion of congruent trials was held to a chance level and marginal display frequency was also held constant.

Keywords: conflict adaptation, contingency, executive control, response conflict, response conflict adaptation, congruency sequence effect, ISPC effect

INTRODUCTION

Our perceptual worlds are cluttered with information, only a small fraction of which should drive behavior at a given time. Thus, it is necessary to differentiate between behaviorally relevant and irrelevant sources of information so that we do not reflexively act on the biggest, shiniest object we perceive. To study selection processes, researchers use the Stroop (1935), Simon (1969), and Eriksen and Eriksen (1974) tasks. These have revealed that selection is imperfect; performance is typically worse on incongruent trials, where the irrelevant and relevant sources of information indicate different responses, than on congruent trials, where the irrelevant and relevant sources of information indicate the same response.

SEQUENTIAL MODULATIONS OF CONGRUENCY EFFECTS

Among the many factors influencing the effectiveness of our ability to select a source of information are immediately previous events. Gratton et al. (1992) is credited as being the first to report that the magnitude of the difference in response times (RTs) between incongruent and congruent trials is larger following a congruent trial than following an incongruent trial. This phenomenon has since been given many names, including conflict adaptation (Botvinick et al., 2001), the Gratton effect (Notebaert and Verguts, 2008), sequential modulation (Hazeltine et al., 2011b), and the congruence sequence effect (Lee and Cho, 2013). Given that one goal of this paper is to examine the various sources that might contribute to this effect, we will use the atheoretical term "sequential modulation."

When discovered in a flanker task, sequential modulations were thought to reflect the operation of control mechanisms that dynamically weight the various sources on information in concert with task goals. Following Botvinick et al.'s (2001) influential paper proposing a model in which response conflict triggered a control process that changes the relative weightings of task-relevant and task-irrelevant information, a sizeable literature emerged examining sequential modulations (e.g., Ullsperger et al., 2005; Wendt et al., 2006; Akçay and Hazeltine, 2007, 2011; Chen and Melara, 2009; Schmidt and De Houwer, 2011; Schmidt, 2013a), their time course (e.g., Notebaert et al., 2006; Egner et al., 2010; Duthoo et al., 2014), and their boundary conditions (e.g., Kiesel et al., 2006; Egner et al., 2007; Freitas et al., 2007; Akçay and Hazeltine, 2008; Notebaert and Verguts, 2008; Funes et al., 2010; Hazeltine et al., 2011b; Lee and Cho, 2013; Braem et al., 2014; Kim and Cho, 2014).

However, as researchers have probed deeper into this phenomenon, a host of potential ways that a previous trial can affect the current one has emerged (see Schmidt, 2013a). The claim that sequential modulations reflected changes in the weighting of particular stimuli or stimulus dimensions was first challenged by Mayr et al. (2003), who noted that many experiments examining sequential modulations used two-choice tasks so that when a congruent trial followed a congruent trial or an incongruent trial followed an incongruent trial, exact repetitions of the stimuli were possible, but when an incongruent trial followed a congruent trial or a congruent trial followed an incongruent trial, no exact repetitions were possible. In other words, the shorter RTs stemming from exact repetitions of stimuli (and the absence of the requirement to rebind stimulus features, see Hommel et al., 2004) only benefits congruent trials following congruent trials and/or incongruent trials following incongruent trials. Thus, the pattern of RTs attributed to control processes changing the weightings of various sources of information could be accounted for simply in terms of the effects of repetitions and alternations of stimulus features.

To address this confound, many researchers turned to four-choice tasks, in which all types of congruency sequences can be obtained using stimulus features that did not appear on the immediately preceding trial. Many studies (Ullsperger et al., 2005; Akçay and Hazeltine, 2011; Hazeltine et al., 2011a,b; Lee and Cho, 2013; Kim and Cho, 2014) restrict the analyses of sequential effects to complete alternations, which can be done for all two-trial sequences of congruent and incongruent trials when the task is four-choice. It is also possible to remove trials in which the irrelevant feature on the previous trial indicates the same response as the relevant feature on the current trial (negative priming trials) and trials in which the relevant feature of the previous trial indicates the same response as the irrelevant feature of the current trial, given that these types of transitions may also affect RT, but these are less consistently eliminated from analyses of sequential modulations.

However, the use of four-choice tasks, even when all types of repetitions are removed, can give rise to additional issues for examining sequential modulations. In a typical four-choice conflict task, there are four possible relevant stimulus features each associated with a unique response and four possible irrelevant stimulus features each associated with one of those responses. When the relevant and irrelevant features are randomly paired, only 1/4 of the trials are congruent. Thus, a sequence of two congruent trials represents only 1/16 of the two-trial sequences, whereas a sequence of two incongruent trials, for example, represents 9/16 of the two-trial sequences. This imbalance changes depending on which feature repetitions are eliminated from the analyses (for a full discussion of this issue, see Mordkoff, 2012), but some researchers (e.g., Akçay and Hazeltine, 2007, 2011; Hazeltine et al., 2011b) have opted to increase the rate of congruent trials to obtain more balanced numbers of trials in each of the cells for the analysis, as well as to maintain an equal probability of congruent and incongruent trials.

DISPLAY FREQUENCY AND CONTINGENCY

And yet increasing the probability of congruent trials causes at least two new confounds with other potential contributors to RT: display frequency and contingency. Display frequency refers to the likelihood of a particular stimulus (i.e., combination of relevant and irrelevant information) appearing on a given trial. To make congruent trials as frequent as incongruent trials in standard four-choice designs as described above, each congruent stimulus must appear three times as often as any given incongruent stimulus, because there are three times as many incongruent stimuli as congruent stimuli. It has been shown that more frequently presented stimuli produce shorter RTs than less frequently presented stimuli (Hick, 1952; Hyman, 1953).

Contingency effects emerge when the relative likelihood of a particular task-relevant feature or response given a particular task-irrelevant feature is different from the overall (or unconditional) probability of that particular task-relevant feature or response. For example, in a Stroop task where the red-colored stimuli are the word "RED" on 1/2 of the trials, and the words "GREEN," "BLUE," and "YELLOW" each appear 1/6 each, not only is RED-in-red more frequent than GREEN-, BLUE-, or YELLOW-in-red, but contingencies now exist between the task-irrelevant word "RED" and the task-relevant red color and, therefore, the "red" response. Thus, even though participants are instructed not to attend to the word, it contains information indicating the likely response. Humans are highly sensitive to these contingencies, even when they occur in sources that are to be ignored (Miller, 1987; Mordkoff, 1996).

In this way, both display frequency and contingency may act to reduce RTs to congruent stimuli. Note that in Stroop tasks, where there is a one-to-one mapping between values of the relevant feature and the responses, it is not possible to distinguish between display frequency effects and contingency effects. Note, also, that there is evidence that performance can be affected by the frequency and contingencies that are associated with conjunctions of features, even when these conjunctions are unattended (Mordkoff and Halterman, 2008). While the effects of frequency and contingency are typically insufficient on their own to account for sequential modulations, they can contaminate measures of the congruency. Also, there is evidence that contingency effects may themselves be subject to sequential modulations (Schmidt et al., 2007). That is, the effects of contingency may be larger after trials in which a more frequent pairing of irrelevant and relevant information was presented than after trials in which a less frequent pairing was presented. Thus, the sequential modulation of contingency may be misinterpreted as the more standard modulation of congruency if congruency and contingency are confounded.

DISPLAY FREQUENCY, CONTINGENCY, AND THE ITEM-SPECIFIC PROPORTION CONGRUENCY (ISPC) EFFECT

Display frequency and contingency effects not only complicate the interpretation of sequential modulations, but, as noted by Schmidt and Besner (2008), they can also provide an alternative explanation for the item-specific proportion congruency (ISPC) effect reported by Jacoby et al. (2003). It had been established that the magnitude of congruency effects depended on the overall proportion of congruent trials (Logan and Zbrodoff, 1979; Tzelgov et al., 1992), but Jacoby et al. (2003) found that the magnitude of the congruency effect for a given task-irrelevant feature can depend on the proportion of trials on which that particular feature is paired with a congruent versus incongruent task-relevant feature. Jacoby et al. (2003) concluded that item-specific processes modulate the influence of task-irrelevant information in the Stroop task, a proposal that has been incorporated into recent models of sequential modulations (e.g., Verguts and Notebaert, 2008; Blais and Verguts, 2012). The proposal that control is implemented in a feature-specific manner has broad implications for theories of attention and executive function, so determining whether the ISPC effect does indeed reflect the tracking of the usefulness of individual feature values is a critical issue.

However, in Stroop tasks in which some word–color pairs are presented more frequently than others, irrelevant features become predictive of both relevant features and correct responses, and Schmidt and Besner (2008) showed that such contingencies could completely explain the pattern of results without appealing to differences in attentional control (see also, Grandjean et al., 2013). While both frequency and contingency provide possible explanations for the ISPC effect, the design used by Jacoby et al. (2003) did not allow for the independent measurement of these effects, because both were confounded with the putative ISPC effect. To address this, Schmidt (2013b) designed a Stroop task in which there were three types of incongruent trials: frequently paired colors and words with words that were usually incongruent (high/low), infrequently paired colors and words with words that were usually incongruent (low/low), and infrequently paired colors and words with words that were usually congruent (low/high). The high/low were performed 40 ms faster than the low/low trials, indicating a robust contingency effect, but there was no difference in RT for the low/high and low/low trials, suggesting that the proportion congruence had little impact on performance.

Thus, there is evidence to suggest that the ISPC is really driven by contingency. Here, we directly test whether ISPC effects and display- frequency/contingency effects are approximately the same size to determine if the latter can account for the former. In essence, the question is whether a specific feature value (e.g., the word "red") can be associated with something abstract like congruence or incongruence rather than being associated with a particular response.

EXPERIMENT

The goal of the present study was to tease apart the various potential modulators of congruency effects: repetition effects, frequency (and contingency) effects, and sequential modulations. To do this, we use a four-choice Stroop task in which we hold constant the marginal frequencies of the relevant and irrelevant stimulus features, as well as the responses, but manipulate the frequencies of their conjunctions. We modify the procedure used by Jacoby et al. (2003) and Schmidt and Besner (2008) in which the possible word–color pairings differ in terms of the proportion of trials on which they occur (display frequency; see also, Schmidt, 2013b). In a Stroop task where there is a one-to-one mapping between the relevant stimulus feature (color) and the response, changing the display frequencies of individual color/word pairings necessarily changes the proportion of congruence for the particular words. That is, the proportion congruence of a word can be increased only by making the congruent color/word pairs more frequent than the incongruent color/word pairs. In order to decrease the proportion congruence of a word, it must be presented in an incongruent color more frequently than in the congruent color. Therefore, to obtain separate and independent measures of the effects of frequency in the absence of congruency, we include neutral trials. On these trials, the irrelevant word is not associated with an option within the response set, but different words appear more frequently in some colors than in others, matching (exactly) the frequency differences of the congruent and incongruent trials.

METHOD

Participants

One hundred and six undergraduate students (58 females) at the University of Iowa participated to fulfill their course requirements. All were self-reported to be native English speakers with corrected-to-normal vision. Participants provided informed consent but were naïve to the study's design and purpose.

Stimuli and apparatus

The stimuli were presented against a black background on a 17 inch LCD monitor of a personal computer. The viewing distance was approximately 120 cm. Visual basic software was used to control and present the stimuli. The Speech Recognition software from Windows XP was used to record the RT and accuracy of each trial.

Four ink colors (red, yellow, green, and blue) were paired with four color words (RED, BLUE, YELLOW, and GREEN) and four neutral words (CAR, LINE, FOLDER, and SHIRT) to form Stroop trials and neutral trials (see **Table 1**). All colors were presented at chance level (16 of 64 presentations) and all words were presented at chance level (8 of 64 presentations), as well. The overall proportion congruent of the block was at chance level (8 of 64 presentations). The trial frequencies of the neutral trials were selected to match those of the congruent and incongruent Stroop trials. The four neutral words (CAR, LINE, FOLDER, and SHIRT) were chosen to match the length of the color words.

To manipulate item frequency, the pairings of colors and words was arranged so that for each color there was one color word and one non-color word paired with it four times every 64 trials (frequent pairing), one color word and one non-color word paired with it twice every 64 trials (moderate pairing), and two color words and two non-color words paired with it once every 64 trials (infrequent pairing; see **Table 1**). Participants were randomly assigned to one of four color-word pairing mappings so that the roles of the four colors were counterbalanced across participants. With this arrangement, we were able to vary the frequencies for the contingencies between words and colors while having a neutral match for each congruent and incongruent word.

Procedure

Each trial started with a fixation cross of 500 ms. After a blank of 300 ms, target stimulus was presented. Participants were instructed to name the ink color as quickly and accurately as possible. Participants had 5,000 ms to respond. After an incorrect response, a display with the words "you said:" followed by the word recorded by the voice recognition system on one line and "correct response:" followed by the correct color name on another line. All of the words in the error display were white presented on a black background. The incorrect response and correct word were presented in white for 1000 ms following an error trial. After a correct response or the error display, a blank display was presented for 700 ms. Participants performed 15 blocks of 64 trials each. The first two blocks were treated as practice blocks.

Table 1 | Example design in terms of display frequency as a function of task-relevant color and task-irrelevant word.

	Word (congruent/incongruent trials)				Word (neutral trials)			
Color	**RED**	**BLUE**	**YELLOW**	**GREEN**	**CAR**	**LINE**	**FOLDER**	**SHIRT**
Red	**1**	2	4	1	1	2	4	1
Blue	4	**1**	1	2	4	1	1	2
Yellow	1	4	**2**	1	1	4	2	1
Green	2	1	1	**4**	2	1	1	4
PC	1/2 C	1/2 C	C	2 C				

Row labels (in lower case) indicate colors; column labels (in upper case) indicate words. Table entries (numbers) indicate trials per block. Numbers in bold indicate congruent trials. PC indicates the proportion congruence for a word; C, chance.

RESULTS AND DISCUSSION

Two participants were removed from the analysis due to the malfunctioning of the speech recognition system. The mean proportion correct was 0.97. Inspection of the cell means of accuracy indicated that any effects of accuracy would be small (<3%), so our analyses focused on RT. The first two trials of each block, error trials (2.8%), trials immediately following an error trial (2.8%), and trials with RTs less than 150 ms (3.0%) or greater than 2,000 ms (0.3%) were excluded from the analysis.

Repetition effects

Our questions concern the various ways that the composition of previous trials affects performance on subsequent trials (see Dalrymple-Alford and Budayr, 1966), so we first assessed the effects relating to feature repetitions. Five types of trials were considered: trials with no repeated features (NO), trials in which the relevant feature (color) repeated (CC), trials in which the irrelevant feature (word) repeated (WW), trials in which the relevant feature on the previous trial indicated the same response as the irrelevant feature on the current trial (CW), and trials in which the irrelevant feature on the previous trial indicated the same response as the relevant feature on the current trial (WC). We eliminated from the analysis trials in which multiple forms of repetition occurred. Because the frequencies of these various forms of repetitions differ depending on the congruency of the previous and current trial, we restricted our analyses to incongruent trials that followed incongruent trials. Furthermore, to avoid any confounding effects of frequency (the mean frequencies of the four repetition types and control trials differed), we also restricted the analyses to frequent color-word pairings. It was not possible to hold constant the frequency of the previous trial, because this eliminated the possibility of WW and CC trials. Moreover, with these restrictions, it was not possible to perform an ANOVA with the presence/absence of each type of repetition as a factor. Instead, we performed a one-way ANOVA on these five trial types, which revealed a significant effect, $F(4{,}408) = 34.26$, $p < 0.0001$.

To examine this finding more closely, we directly compared each repetition type (CC, CW, WC, and WW) to the NO trials (**Figure 1**). Because our focus was on the potential contaminating effect of these repetitions on measures of congruency rather than on the repetition effects themselves, we adopted a liberal statistical threshold uncorrected for multiple comparisons. RTs on the trials on which only color repeated (CC) were 109 ms shorter (575 ms) than RTs on NO trials, $t(103) = 8.16$; $p < 0.0001$. RTs on the trials on which only the word repeated (WW) were 31 ms shorter (653 ms) than RTs on NO trials, $t(103) = 2.40$; $p < 0.05$. Thus, the benefit associated with having to inhibit the same irrelevant word that had to be inhibited on the previous trial was larger than any cost associated with rebinding a repeated word with a novel color. The mean RT for CW trials (657 ms) was 27 ms faster than for NO trials, $t(103) = 2.42$; $p < 0.05$. Because we analyzed only incongruent trials, this result suggests that it is easier to suppress an inappropriate response when it was produced on the immediately preceding trial. Finally, the mean RT on WC trials (690 ms) was 6 ms longer than NO trials, but this difference was not significant, $t < 1$. Thus, there was little evidence that this form of negative priming affected RT in this experiment.

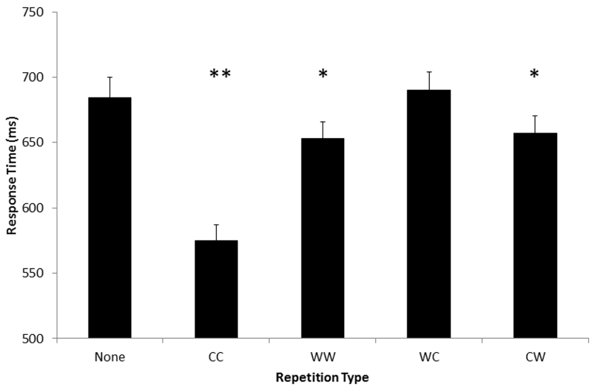

FIGURE 1 | Mean response times (RTs) for the no repetition trials (None) and trials with just one of the four types of repetitions: Color→Color (CC), Word→Word (WW), Color→Word (CW), and Word→Color (WC). Asterisks indicate significant differences from the No repetition condition, $^*p < 0.05$; $^{**}p < 0.0001$.

The absence of any costs associated with partial repetitions was unexpected (see, e.g., Hommel, 1998), so to further examine whether rebinding costs played a role in RT, we examined sequences of neutral trials in which the irrelevant word was not associated with a color and therefore should not have produced much response competition. Frequent neutral word–color combinations following neutral word–color combinations produced mean RTs of 578 ms when no features repeated. When only the word repeated, these trials produced RTs of 582 ms, which was not significantly different, $t < 1$. This finding suggests that rebinding costs did not play a major role in the RT in the present experiment, possibly because there were many possible relevant and irrelevant features.

Frequency and the ISPC effect

Three of the four types of repetition (CC, WW, CW) produced effects on RT on their own that were similar or greater in magnitude to some modulations of congruency effects. Therefore, we adopted a conservative approach and eliminated trials with any of the four possible forms of repetitions before testing whether display frequency is sufficient to account for the ISPC effect. Moreover, because frequency may modulate congruency effects, as in the ISPC effect, we first examined frequency in the neutral trials only. Note that with the present design, the effects of display frequency and contingency are confounded, so we use the term "frequency" to refer to the combined effects of both. An estimate of the frequency effect was obtained with a one-way ANOVA, which revealed a significant effect, $F(2,206) = 10.02$, $p < 0.0001$, MSE $= 356.16$, $\eta_p^2 = 0.09$. Within the neutral trials, frequent combinations of relevant and irrelevant features produced RTs of 577 ms, chance combinations produced RTs of 582 ms, and infrequent combinations produced RTs of 589 ms.

Our next step was to determine whether the frequency effect could account for any observed ISPC effect in the congruent and incongruent trials. Thus, we first determined whether the data indicated ISPC effects and then assessed whether this effect could be explained by frequency as measured in the neutral trials. Therefore, we categorized each trial according to whether the task-irrelevant word was paired with a congruent color frequently (1/2 of trials that the word appeared), at chance (1/4 of trials) or infrequently (1/8 of trials; see **Table 1**) as in Schmidt and Besner (2008). The data were then submitted to a two-way ANOVA with this factor and congruency (without the neutral trials).

There was a significant main effect for congruency, $F(1,103) = 241.34$, $p < 0.0001$, MSE $= 6158.87$, $\eta_p^2 = 0.70$, but not for the proportion of congruency of the word, $F < 1$. Critically, the interaction between the two factors was significant, $F(2,206) = 4.59$, $p < 0.05$, MSE $= 1972.60$, $\eta_p^2 = 0.43$, indicating a significant ISPC effect. When the irrelevant word was frequently congruent, the congruency effect was 111 ms (incongruent 688 ms; congruent 577 ms). When the irrelevant word was congruent at a chance rate, the congruency effect was 97 ms (incongruent 683 ms; congruent 586 ms), and when the irrelevant word was infrequently congruent, the congruency effect was 85 ms (incongruent 678 ms; congruent 593 ms).

However, as pointed out by Schmidt and Besner (2008), this analysis confounds frequency and ISPC effects, so we next examined whether this effect could be accounted for with the frequency effect as measured in the neutral trials. For congruent trials, pairs that include frequently congruent words are themselves more frequent; that is, the frequency of the word–color combination and the proportion that the word is paired with a congruent color is perfectly confounded for congruent trials. Thus, differences in the effect of frequency on the congruent trials and the effect of frequency on the neutral trials provide evidence for an ISPC effect. Frequent congruent trials were performed 16 ms faster than infrequent congruent trials (577 vs. 593 ms), and frequent neutral trials were performed 12 ms faster than infrequent neutrals trials (577 vs. 589 ms); the magnitude of the frequency effect did not differ for the two trial types, $t < 1$ (**Figure 2A**), so it does not appear that the RTs of congruent trials are affected by the proportion of trials in which the word is congruent beyond what would be expected by the proportion of trials in which the word is paired with that color.

Of course, congruent trials represent only one half of the congruency effect, and thus do not provide a strong test of whether the ISPC effect can be accounted for by frequency effects on their

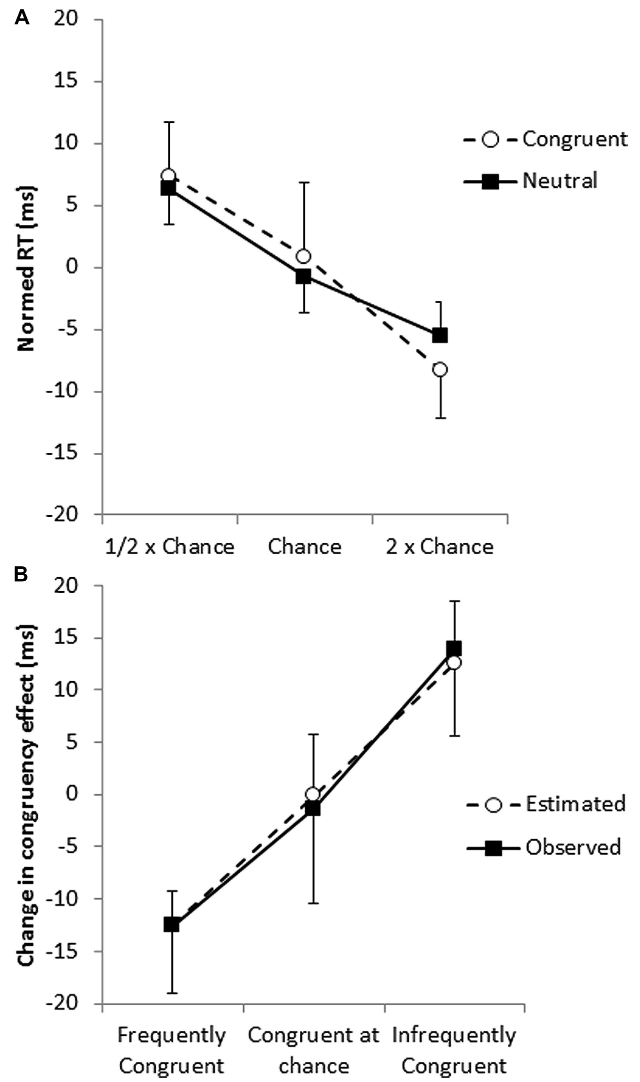

FIGURE 2 | (A) Frequency effects for congruent (open circles) and neutral (filled squares) trials with the overall mean of the trial type (congruent or neutral) subtracted out. **(B)** The change in the magnitude of the congruency effect (the ISPC effect) as observed in the congruent and incongruent trials (filled squares) and estimated from the neutral trials (open circles).

own. However, while frequency and proportion congruent are perfectly confounded in the congruent trials, the relationship between the two factors is much more complex for the incongruent trials. For example, the infrequent incongruent stimuli are composed of words that are frequently incongruent, incongruent at chance rate and infrequently incongruent. In short, the effect of frequency on the incongruent trials is less informative regarding the effect of proportion congruency than the congruent trials, because of the complex relationship between the two factors.

The critical question is whether the observed frequency effect in the neutrals can account for the interaction between frequency and proportion congruent (that is, the ISPC effect) in the congruent and incongruent trials. However, when determining the congruency effect for frequently congruent words, for example, RTs from trials in which the word is frequently paired with the congruent color are compared to RTs from trials in which the same word is paired with other colors that it appears with less frequently. Similarly, when determining the congruency effect for the infrequently congruent words, RTs from trials in which the word is infrequently paired with the congruent color are compared to RTs from trials in which the same word is paired with a mixture of other colors that were not infrequently paired with the word.

Therefore, to evaluate whether the ISPC effect could be explained by display frequency, we calculated for each participant the expected change in the magnitude of the congruency effect based on the frequency effect observed in the neutral trials and the proportions of frequent, chance, and infrequent pairings making up the incongruent trials. This procedure predicted a 25 ms change in the congruency effect between trials with frequently congruently words and trials with infrequently congruent words, which was similar to the observed 26 ms change in the congruency effect (i.e., the ISPC effect), $t < 1$ (**Figure 2B**). In short, it appears that display frequency can account for the ISPC effect without assuming that individual irrelevant items have individually modulated congruency effects based on the likelihood that the irrelevant item is congruent. Consistent with the conclusions of Schmidt and Besner (2008), there is no evidence for item-specific control processes.

Sequential modulations

We now return to the debate about whether sequential modulations of congruency effects can be observed without repetition and frequency confounds (e.g., Mayr et al., 2003; Hommel et al., 2004; Ullsperger et al., 2005; Akçay and Hazeltine, 2007, 2011; Hazeltine et al., 2011b; Schmidt, 2013a). To examine this in the present data set, we selected the trials without any of the four types of repetitions and looked only at the most frequent combinations of colors and words because, with repetitions removed, the different types of transitions consist of trials with different mean frequencies. These criteria led to five participants not having any trials in which a congruent trial followed a congruent trial, so these individuals were removed from the analysis. The data from remaining 99 participants were submitted to a two-way ANOVA with previous congruency and current congruency as factors. This produced main effects previous congruency, $F(2{,}196) = 15.86$, $p < 0.0001$, $\text{MSE} = 3833.75$, $\eta_p^2 = 0.14$, congruency, $F(2{,}196) = 169.41$, $p < 0.0001$, $\text{MSE} = 6259.28$, $\eta_p^2 = 0.63$, and an interaction between the two factors, $F(4{,}392) = 6.45$, $p < 0.0001$, $\text{MSE} = 2706.71$, $\eta_p^2 = 0.06$.

As depicted in **Figure 3**, the congruency of the previous trial affects the magnitude of the congruency on the current trial, even when repetitions and contingency effects are accounted for. The pattern is somewhat atypical in that the smallest congruency effects are observed after neutral trials (83 ms) rather than after incongruent trials (111 ms). As is typical when sequential modulations are observed, the congruency effect was largest (134 ms) after congruent trials.

Finally, we examined whether the frequency effect was moderated by the frequency of the previous trial (see, Schmidt et al., 2007). To do this, we looked at neutral trials that followed neutral trials, so that sequential modulations of frequency effects would not be confounded with sequential modulations of congruency effects. A two-way ANOVA with current frequency and previous frequency as factors, looking only at trials with no repetitions of any kind, revealed a significant effect of current frequency, $F(2{,}206) = 3.77$, $p < 0.05$, $\text{MSE} = 2271.17$, $\eta_p^2 = 0.04$, and a significant interaction between current frequency × previous frequency, $F(4{,}412) = 2.41$, $p < 0.05$, $\text{MSE} = 2364.36$, $\eta_p^2 = 0.02$. The difference between frequent items and infrequent items was 1 ms following infrequent items, 21 ms following moderately frequent items and 7 ms following frequent items. Thus, while the interaction was significant, replicating Schmidt et al. (2007), the effect appeared to be small and, as above, non-monotonic. Nonetheless, this finding indicates that when congruency is confounded with frequency, the source of sequential modulations is ambiguous.

GENERAL DISCUSSION

In the present study we sought to assess the various contributors to the magnitude of congruency effects using a four-choice Stroop task with neutral trials and a large number of participants. The results indicate that the majority factors did indeed impact performance, making them candidate sources of changes in the

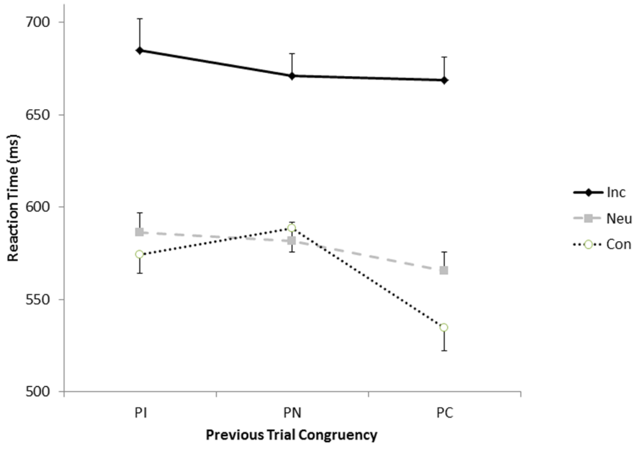

FIGURE 3 | Response times for the congruent (open circles), neutral (gray squares), and incongruent (black diamonds) as a function of the congruency of the previous trials.

congruency effect. Feature repetitions, frequency, and sequential modulations of both congruency and frequency all produced significant effects on RT when the other factors were held constant.

The present findings make three principle contributions. First and most generally, they indicate RTs depend on a host of factors. While these factors are difficult to dissociate, in the present study we ran a relatively large number of subjects and manipulated item frequency independently from the frequency of individual features (e.g., the color blue or the word "shirt"), which was held constant. Under these conditions, it was apparent that repetitions of irrelevant sources of information shortened RT.

Second, the findings demonstrate that trial-frequency effects (and/or contingency effects), as measured in neutral trials, are of a magnitude that is sufficient to allow them to account for the ISPC effect on their own. That is, while previous work (e.g., Schmidt and Besner, 2008; Schmidt, 2013b) demonstrated that contingency effects provided a possible explanation for the ISPC effect, the present findings indicate that the ISPC effect is the same magnitude as what is predicted by the contingency effect, as measured in the neutral trials. Thus, we conclude that there is no evidence for an ISPC effect.

Third, the findings indicate that sequential modulation of congruency effects can be observed when feature repetitions, negative priming, and frequency effects are all controlled, and congruent and incongruent trials occur at overall frequencies equal to chance. While previous studies have shown that sequential modulations can occur without contingency effects (e.g., Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014), the present study shows that sequential modulations and contingency effects can co-occur, indicating that the former do not only emerge in the absence of the latter (see Bugg, 2014). Moreover, just one eighth of the trials in the present experiment were congruent, which may account for the somewhat unusual pattern of sequential modulations (**Figure 3**). The relative relatedness between the congruent and incongruent words may also have affected the pattern of sequential modulations, just as it may have affected the pattern of partial repetitions. This would explain why congruency effects were smallest after neutral trials rather than after incongruent trials. In any case, the data suggest that the congruency of the previous trial can affect the magnitude of the congruency effect on the current trial independent of feature repetitions and display frequency.

In sum, the data indicate that RTs reflect a set of processes that are sensitive to a range of factors that include both specific (e.g., the individual features) and abstract (e.g., the congruency and frequency of the conjoined item) information relating to the previous trial. However, there is no evidence that modulations of the congruency effect are implemented at the level of individual features; that is, there is no evidence for an ISPC. On the other hand, sequential modulations of congruency effects are apparent when repetitions are eliminated and display frequencies held constant. However, given that the events of the previous trial affect the processing of the current trial in a myriad of ways, sequential modulations may be difficult to study using tasks that do not allow for the various contributors to be isolated and estimated. As in other domains, the resolution of conflict is complicated when so little of the past is forgotten.

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The Gratton effect remains after controlling for contingencies and stimulus repetitions

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Highlights:

- The conflict monitoring hypothesis signals the need for cognitive control
- The Gratton effect is a key result attributed to the conflict monitoring hypothesis
- Some argue that controlling binding confounds eliminates the Gratton effect
- A Gratton effect remains in a vocal Stroop task after eliminating confounds

The Gratton effect, the observation that the size of the Stroop effect is larger following a congruent trial compared to an incongruent trial, is one pivotal observation in support of the conflict-monitoring hypothesis. Previous reports have demonstrated that non-conflict components, such as feature binding, also contribute to this effect. Critically, Schmidt and De Houwer (2011) report a flanker task and a button-press Stroop task suggesting that there is no conflict adaptation in the Gratton effect; it is entirely caused by feature binding. The current investigation attempts to replicate and extend this important finding across two experiments using a canonical four-choice Stroop task with vocal responses. In contrast to Schmidt and De Houwer, we observe reliable conflict adaptation after controlling for feature binding. We argue that the overall strength of conflict is critical for determining whether a conflict adaptation component will remain in the Gratton effect after explaining binding components.

Keywords: conflict monitoring, cognitive control, cognitive control mechanisms, Stroop effect, conflict adaptation, Gratton effect, feature binding

INTRODUCTION

The Gratton effect (Gratton et al., 1992) refers to the finding that congruency effects (i.e., Stroop and flanker effects) are reduced following incongruent trials compared to congruent trials. The most widespread explanation of the Gratton effect is the conflict adaptation hypothesis (Carter et al., 1998; Botvinick et al., 2001, 2004), which states that response conflict from the previous trial signals a *need for control* that manifests as a modulation of response times and error rates on the subsequent trial. The net result is that the size of the Stroop effect is larger following a congruent trial than following an incongruent trial. First observed in a flanker task (Eriksen and Eriksen, 1974), it has been observed in many conflict tasks such as Stroop (Kerns et al., 2004; Mayr and Awh, 2009) and Simon (Akçay and Hazeltine, 2007).

An account of the Gratton effect based solely on conflict adaptation is unlikely. Others have pointed out that binding effects, namely feature repetition biases, also contribute to the Gratton effect (Mayr et al., 2003; Hommel et al., 2004; Mayr and Awh, 2009). While there is considerable debate in the field, most would acknowledge that any Gratton effect that remains after all binding sources have been eliminated is consistent with conflict adaptation.

Schmidt and De Houwer (2011) were the first to assess two different binding effects, namely contingency biases and congruency switch costs. They reported a Stroop and a flanker experiment in which they eliminated three possible binding confounds: (1) feature repetition biases, (2) sequential contingency biases, and (3) congruency switch costs. Controlling the first two biases was sufficient to eliminate the Gratton effect in RTs for both tasks, and errors in the flanker task. They attributed the remaining Gratton effect in the error data for the Stroop task to a congruency switch cost by showing that there was no congruency repetition by congruency interaction (see below). Thus, the critical question is whether there is indeed a Gratton effect after such sources have been eliminated.

The implications of this finding are profound. The Gratton effect is one of the key findings in support of the conflict-monitoring hypothesis (Carter et al., 1998), and demonstrating that the Gratton effect is entirely the result of feature binding would necessitate reinterpreting literally hundreds of experiments. Therefore, the current paper reviews the three binding confounds, citing literature which shows that no one confound alone can explain the Gratton effect. We then attempt to replicate Schmidt and De Houwer (2011), first by reanalyzing data from a large-scale Stroop study (Blais et al., 2010), and second with a new experiment. In contrast to the findings reported by Schmidt and De Houwer, a robust Gratton effect is observed reasserting that, at least in some tasks, conflict adaptation does contribute to the size of the Gratton effect.

Schmidt and De Houwer (2011) provided an in depth discussion of the following three confounds. They are briefly outlined here to orient readers to the issues.

FEATURE REPETITIONS

Following Mayr et al.'s (2003) seminal paper demonstrating that feature repetitions cause the Gratton effect, it has become standard practice to, at the very least, eliminate complete repetitions when both the target feature and distractor feature repeat on the next trial. To eliminate all possible sources of feature repetitions (i.e., target->target, distractor->distractor, target->distractor, and distractor->target transitions), it is necessary to use at least a four-choice task. Studies that have used both a four-choice task, and eliminated all the feature repetition conditions tend to show that, although the Gratton effect is reduced in size, it is not eliminated (e.g., in a Simon task: Akçay and Hazeltine, 2007; in a flanker task: Verbruggen et al., 2006).

CONTINGENCY CONFOUNDS

Contingency biases are another confound that may also increase the size of the Gratton effect. Participants are often presented with color words in their congruent color more often than would be expected by chance. These types of contingencies are problematic because participants learn them and end up responding faster to the high contingency trials (i.e., when the word is presented in its most frequent color) compared to the low contingency trials (i.e., when the word is presented in a color other than its more frequent color).

Mayr et al. (2003) manipulated proportion congruency between subjects in a flanker task and reported an increase in the size of the Gratton effect. In addition, Schmidt et al. (2007) had subjects identify the color of a non-color word (e.g., MOVE). Critically, the authors systematically paired how often each word would appear in a specific color to create high and low contingency items. For example, in the 75% contingent condition, MOVE might appear in orange on 75% of trials and in red, blue, or green on the remaining 25% of trials. Although there is no conflict *per se*, in addition to finding contingency effects (i.e., subjects were faster to press the orange key if the word was MOVE compared to any other word), they also observed a pseudo-Gratton effect. That is, the contingency effect was larger if preceded by a high contingency item in comparison to a low contingency item.

CONGRUENCY SWITCH COSTS

Schmidt and De Houwer (2011) proposed the congruency switch hypothesis, a novel third confound against the conflict adaptation account of the Gratton effect. The logic is similar to the task-switch hypothesis (e.g., Monsell, 2003). In short, for an incongruent trial, the system must select between two activated response codes, and bind one to the color and the other to the word. But, for congruent trials, the system simply binds the one activated response code to both the color and the word. Thus, it is conceivable that the system must be reconfigured to respond to a congruent trial compared to an incongruent trial. If so, there may be a cost associated with it. More generally, if even slightly different strategies are used in response to congruent vs. incongruent trials, then switching from one trial type to the next may incur a cost.

To test if a congruency switch is contributing to the Gratton effect, Schmidt and De Houwer (2011) suggest that "analyzing congruency as a function of switch rather than n-1 congruency should lead to roughly additive effect of congruency and switch." (p. 179). They noted that visual inspection of Freitas et al. (2007) seems to support this hypothesis.

EXPERIMENT 1: A GRATTON ANALYSES OF Blais et al. (2010)

To summarize, there are at least three binding confounds that preclude a conflict adaption hypothesis of the Gratton effect. The evidence in the literature suggests that (1) feature repetitions alone cannot account for a Gratton effect, (2) contingency effects can lead to a pseudo-Gratton effect, and (3) there is speculative evidence that congruency switch costs can yield a Gratton effect.

The current paper reports a series of new analyses from a recent large-scale Stroop study which looked at the role of one's awareness of the proportion of congruent trials on the size of the Stroop effect (Blais et al., 2010). Two important findings emerge. First, when there is no contingency between the color and the word (the 25% congruent condition), there is still a strong Gratton effect after stimulus repetition trials are removed that cannot be explained by a congruency switch. Second, across the entire range of proportions between the 10 and 80% range, the size of the Gratton effect is statistically equal both when stimulus repetition trials are included, and excluded, from the analysis. This finding suggests that color-word contingency plays no role in the Gratton effect.

METHOD

For full methodological details, see Blais et al. (2010). Briefly, fifteen subjects spent 8–10 h in the lab performing 19,000 trials in a vocal Stroop task. These trials were administered in blocks of 100 trials across 19 different proportion congruency conditions ranging from 5 to 95% in increments of 5. The order of the 190 blocks was randomized, and each participant received the same order. Each subject responded vocally to the color (RED, BLUE, YELLOW, or GREEN) that the word (red, blue, yellow, or green) was presented in. Stimuli for each block were sampled randomly *with* replacement from the set of 16 possible stimuli such that, if the proportion congruency level was 30%, then 30 congruent stimuli were selected, followed by 70 incongruent stimuli. These 100 items were then randomly sorted and presented to the subject.

It is important to note that 9 of these subjects were asked to estimate the proportion of congruent trials and rate their confidence of this estimate following each block of 100 trials. There was no difference between these two groups on any of the analysis reported here, and so they are treated as a homogenous group of 15 subjects. Since this is a four-choice task, there is no contingency between the color and the word (i.e., the word green is equally likely to appear in any of the four colors) in the 25% congruent condition.

The same correct RTs data as in the original report were used. That is, correct RTs longer than 2000 ms (outliers) or shorter than

200 ms (anticipatory) were excluded, along with any RTs more than 2.5 standard deviations away from the mean within each subject by block by congruency cell.

RESULTS

ANALYSES OF RTs

Including stimulus repetitions

Table 1 shows the mean RT for the four previous congruency-by-congruency cells at each level of proportion congruency. To maximize power, a 2 (previous congruency) by 2 (current congruency) ANOVA was conducted separately for each proportion condition. The results of this analysis are on the bottom portion of **Table 1**. To summarize, there was a significant main effect of congruency at each level of proportion congruency. There was a significant main effect of previous congruency at all levels of proportion except [15, 70, 75]. Critically, these factors interacted to produce a Gratton effect at all levels of proportion except [10].

The solid black circles in **Figure 1A** show the size of the Gratton effect as a function of the proportion of congruent trials. According to pure contingency accounts of the Gratton effect, an increase in the contingency between the color and the word should result in an increase in the size of the Gratton effect. Even though the trend lines appear relatively flat, a repeated measures regression analysis was performed to check for the presence of a non-zero, positive, slope. Specifically, a slope and intercept estimate was calculated for each subject, and a one-sample t-test was conducted on these estimates. Conceptually, a positive slope indicates that the Gratton effect increases as the proportion of congruent trials increases. The results of this reveal a slope of 0.139 ± 0.131 ms, $t_{(14)} = 1.07$, $p > 0.30$, and an intercept of 31.789 ± 7.701, $t_{(14)} = 4.13$, $p < 0.001$. This is shown as the solid black line in **Figure 1A**. Thus, the proportion of congruent trials does not impact the size of the Gratton effect.

Excluding stimulus repetitions

Table 2 further divides the mean RT from the four previous congruency x congruency cells in **Table 1** into the 15 cells that comprise the non-orthogonal feature repetition types (word->word, color->color, word->color, color->word). In this four-choice task, this amounts to keeping approximately 56% of trials per subject. The same set of ANOVAs reported above were conducted on the cells for which there are no stimulus-repetitions, indicated by the bolded rows in the **Table 2**. The results are reported in the middle portion of **Table 2**. There was a significant main effect of congruency at each level of proportion congruency. There was a significant main effect of previous congruency at all levels of proportion except [60, 65, 70, 80]. The Gratton effect was only significant for the [15, 20, 45, 50, 60, 65] conditions.

Although many of the Gratton effects were no longer statistically significant, paired t-tests showed that the reduction in the size of the Gratton effect from **Tables 1**, **2** was only reliable for the [55], $p < 0.027$, condition, and was marginal in the [30], $p = 0.070$, condition. For all other comparisons, ps > 0.15. For the present purposes, the fate of the 25% congruent condition is most critical, although the size of the Gratton effect was marginal,

Table 1 | Response times for each of the four 2 (previous congruency) × 2 (congruency) cells as a function of the proportion of congruent trials.

Condition		Proportion of congruent trials (%)														
		10	15	20	25	30	35	40	45	50	55	60	65	70	75	80
Congruent-congruent		610	608	633	640	614	622	609	621	611	601	592	592	592	594	579
Congruent—incongruent		692	701	723	727	714	712	715	739	727	720	709	721	724	740	730
Incongruent-congruent		644	641	668	671	651	646	636	650	642	631	621	627	622	624	616
Incongruent-incongruent		700	693	718	724	715	706	708	727	714	711	706	710	708	729	726
ANOVA EFFECTS																
Previous congruency	$F_{(1, 14)}$	**4.69**	3.38	**17.97**	**5.89**	**20.08**	**5.05**	**9.15**	**7.33**	**5.65**	**10.22**	**8.37**	**5.12**	2.10	2.20	**5.14**
	p	**0.048**	0.087	**<0.001**	**0.029**	**<0.001**	**0.041**	**0.009**	**0.017**	**0.032**	**0.006**	**0.012**	**0.040**	0.169	0.160	**0.040**
	mean ± std error	**21 ± 9**	13 ± 7	**15 ± 3**	**13 ± 5**	**19 ± 4**	**9 ± 4**	**10 ± 3**	**8 ± 3**	**9 ± 4**	**10 ± 3**	**13 ± 4**	**12 ± 5**	7 ± 5	10 ± 6	**17 ± 7**
Congruency	$F_{(1, 14)}$	**30.95**	**42.15**	**53.97**	**60.97**	**87.12**	**68.23**	**56.16**	**79.16**	**80.32**	**84.12**	**109.24**	**95.80**	**102.22**	**86.27**	**79.13**
	p	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**
	mean ± std error	**69 ± 12**	**72 ± 11**	**70 ± 9**	**70 ± 9**	**82 ± 8**	**75 ± 9**	**89 ± 11**	**97 ± 11**	**93 ± 10**	**100 ± 10**	**101 ± 9**	**105 ± 10**	**110 ± 10**	**126 ± 13**	**130 ± 14**
Previous congruency × Congruency (Gratton Effect)	$F_{(1, 14)}$	3.11	**11.90**	**18.65**	**13.59**	**14.38**	**21.92**	**13.88**	**32.76**	**51.43**	**20.89**	**19.14**	**19.81**	**34.01**	**18.05**	**9.43**
	p	0.100	**0.004**	**<0.001**	**0.002**	**0.002**	**<0.001**	**0.002**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**0.008**
	mean ± std error	27 ± 15	**41 ± 11**	**40 ± 9**	**34 ± 9**	**36 ± 9**	**31 ± 6**	**34 ± 9**	**40 ± 7**	**44 ± 6**	**39 ± 8**	**32 ± 7**	**46 ± 10**	**46 ± 8**	**40 ± 9**	**41 ± 13**

The bottom portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect.

The bold values signify that $p < 0.05$.

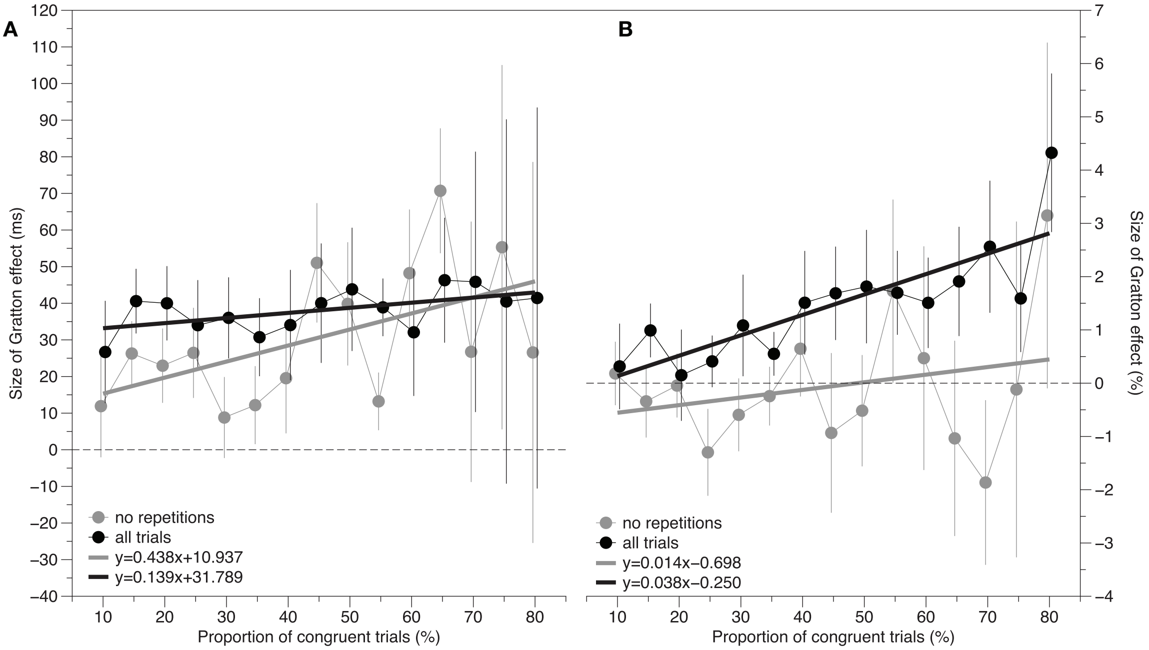

FIGURE 1 | The size of the Gratton effect as a function of the proportion of congruent trials. Black lines represent data in which all trials are included in the estimate. Gray lines represent data in which only the trials without feature repetitions are included in the estimate. The panel to the left **(A)** reflects response time difference scores and the panel to the right **(B)** reflects error rate difference scores. A positive slope is consistent with the hypothesis that the contingency between the color and the word contributes to the size of the Gratton effect. Error bars represent the standard error of the mean.

$p = 0.057$, two-tailed, it was not statistically smaller than when repetitions were included. That is, the 34 ± 9 ms Gratton effect in **Table 1** was statistically equivalent to the 26 ± 12 ms Gratton effect in **Table 2**, $p > 0.50$.

The solid gray circles in **Figure 1A** show the size of the Gratton effect as a function of the proportion of congruent trials. The same repeated measures regression analysis reported above yielded a slope of 0.438 ± 0.382, $t_{(14)} = 1.15$, $p > 0.25$, and an intercept of 10.937 ± 13.026, $t_{(14)} = 0.84$, $p > 0.40$. This is shown as the dotted gray line in **Figure 1A**. Although the slope is numerically larger and the intercept is numerically smaller than in the previous analysis that included repetition effects, both are statistically equivalent to the previous parameter estimates ($p = 0.441$ for the slope, and $p = 0.111$ for the intercept).

Can the switch hypothesis account for the remaining Gratton effect?

Even though the size of the Gratton effects were statistically equivalent across most of the proportion conditions, visual inspection of **Figure 1** suggests that, generally, the Gratton effect is numerically smaller when repetitions are removed. To assess whether the remaining Gratton effect is the result of congruency switching (i.e., Schmidt and De Houwer, 2011), a 2 (Switch) × 2 (Congruency) analysis was conducted at each proportion level. The results of this analysis are shown on the bottom proportion of **Table 2**. An in-depth description of the logic of this analysis is provided by Schmidt and De Houwer (2011). Briefly, additive effects of congruency switch (whether the congruency on the previous trial is the same, or different, on the current trials) and congruency are consistent with an interpretation in which the Gratton effect results from a reconfiguration switch cost. An interaction of these factors cannot rule out a conflict adaption account of the Gratton effect. There was a main effect of congruency at all levels of proportion. There was a switch cost at the following levels of proportion [15, 20, 30, 40, 45, 60, 65]. Critically, these factors interact, rather than add, at each level of proportion except [60, 70, 75, 80]. Furthermore, and even at the levels of proportion at which they fail to interact, the effect seems too large to attribute to purely additive factors.

ANALYSES OF ERRORS

Despite a very low error rate of 2.1%, for the sake of completeness, the entire set of analyses performed on RTs was also done on error rates. It should be noted that the lack of a main effect of congruency effect in many of the analyses, make it difficult to interpret "higher order" effects, such as the Gratton effect, or the increase in the size of the Gratton effect as the proportion of trials increases. It should also be noted that the same analyses performed on efficiency scores (RT divided by accuracy for each cell; Townsend and Ashby, 1983) yields effects which completely replicate the analyses on the RT data.

Including stimulus repetitions

Table 3 mirrors **Table 1** with mean percent error rate in place of RTs. The same analysis contained in **Table 1** is shown at the bottom of **Table 3**. There was a significant main effect of congruency at each level of proportion congruency except [30]. There was only a significant main effect of previous congruency at [80]. Similarly, there was only a Gratton effect at [80].

Table 2 | Response times for each of the four 2 (previous congruency) × 2 (congruency) cells as a function of the proportion of congruent trials after dividing them into whether they contain target->target, distractor->distractor, target->distractor, or distractor->target repetitions.

Condition	Repetition type				Proportion of congruent trials (%)														
	wW	cC	wC	cW	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80
CONGRUENT–CONGRUENT																			
(1) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$					**629**	**627**	**659**	**666**	**636**	**651**	**634**	**646**	**635**	**624**	**614**	**614**	**613**	**614**	**597**
(2) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$	×	×	×	×	557	531	552	567	549	539	533	553	537	533	527	530	526	532	524
CONGRUENT–INCONGRUENT																			
(3) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$					**727**	**741**	**758**	**754**	**750**	**741**	**749**	**764**	**762**	**752**	**743**	**753**	**746**	**772**	**751**
(4) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$	×			×	709	715	733	735	717	721	724	736	732	733	713	721	730	736	729
(5) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$		×	×		613	620	647	669	644	655	644	690	653	657	651	661	671	677	689
INCONGRUENT–CONGRUENT																			
(6) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{green}}$					**673**	**670**	**701**	**692**	**674**	**665**	**655**	**675**	**670**	**652**	**641**	**653**	**641**	**645**	**638**
(7) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$	×		×		676	673	690	695	678	687	659	676	664	662	654	658	655	651	642
(8) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$		×		×	553	554	577	600	580	569	569	571	566	564	549	549	551	556	547
INCONGRUENT–INCONGRUENT																			
(9) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{yellow}}$					**748**	**737**	**760**	**765**	**753**	**755**	**753**	**766**	**757**	**768**	**739**	**758**	**737**	**770**	**755**
(10) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$	×				729	720	747	753	742	741	740	764	748	735	745	779	733	764	781
(11) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{blue}}$		×			601	600	625	635	611	608	601	639	617	611	610	608	604	642	594
(12) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$	×	×			570	556	590	597	590	581	569	586	568	564	567	558	573	580	592
(13) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{red}}$			×		749	743	763	769	770	753	752	765	760	760	764	768	787	814	763
(14) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{green}}$				×	725	719	738	743	739	728	742	762	762	743	745	718	739	735	738
(15) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$			×	×	726	725	766	769	739	733	748	765	743	769	741	732	755	724	785
GRATTON ANOVA EFFECTS																			
Previous congruency	$F_{(1, 14)}$				**21.36**	**23.41**	**27.19**	**10.34**	**10.11**	**7.40**	**5.81**	**12.17**	**10.64**	**16.92**	3.29	3.61	0.95	**5.34**	0.12
	p				**<0.001**	**<0.001**	**<0.001**	**0.006**	**0.007**	**0.017**	**0.030**	**0.004**	**0.006**	**0.001**	0.091	0.078	0.346	**0.037**	0.731
	mean ± std error				**25 ± 5**	**31 ± 6**	**24 ± 4**	**25 ± 8**	**16 ± 5**	**20 ± 7**	**23 ± 9**	**27 ± 7**	**29 ± 9**	**40 ± 9**	19 ± 10	15 ± 7	18 ± 18	**34 ± 14**	−12 ± 34
Congruency	$F_{(1, 14)}$				**56.71**	**62.26**	**76.04**	**49.4**	**60.46**	**68.06**	**57.41**	**60.69**	**66.25**	**47.92**	**64.06**	**58.91**	**76.1**	**50.11**	**30.71**
	p				**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**
	mean ± std error				**153 ± 20**	**150 ± 18**	**146 ± 16**	**143 ± 20**	**147 ± 18**	**153 ± 18**	**162 ± 21**	**153 ± 19**	**160 ± 19**	**164 ± 23**	**154 ± 19**	**186 ± 23**	**147 ± 16**	**156 ± 21**	**175 ± 31**
Previous congruency × Congruency (Gratton effect)	$F_{(1, 14)}$				0.68	**8.26**	**4.77**	4.31	0.59	1.22	1.57	**9.14**	**5.23**	2.62	**7.18**	**16.04**	0.53	1.15	0.24
	p				0.424	**0.012**	**0.046**	0.057	0.455	0.288	0.231	**0.009**	**0.038**	0.128	**0.018**	**0.001**	0.479	0.301	0.629
	mean±std error				12 ± 14	**26 ± 9**	**23 ± 10**	26 ± 12	9 ± 11	12 ± 11	20 ± 15	**51 ± 16**	**40 ± 17**	13 ± 8	**48 ± 17**	**71 ± 17**	27 ± 36	55 ± 50	27 ± 52
CONGRUENCY SWITCH ANOVA EFFECTS																			
Switch	$F_{(1, 14)}$				1.33	**6.07**	**19.04**	0.75	**9.20**	0.01	1.35	**4.68**	**10.88**	1.14	**5.30**	**7.05**	2.99	3.95	1.17
	p				0.267	**0.027**	**<0.001**	0.402	**0.009**	0.944	0.265	**0.048**	**0.005**	0.304	**0.037**	**0.019**	0.106	0.067	0.298
	mean ± std error				11 ± 10	**23 ± 9**	**20 ± 5**	7 ± 8	**17 ± 6**	0 ± 4	8 ± 7	**14 ± 6**	**20 ± 6**	6 ± 6	**15 ± 6**	**17 ± 6**	18 ± 11	16 ± 8	18 ± 17
Congruency	$F_{(1, 14)}$				**45.95**	**38.46**	**33.92**	**48.58**	**77.61**	**56.43**	**62.05**	**60.48**	**81.94**	**51.83**	**67.71**	**67.99**	**68.45**	**58.02**	**92.03**
	p				**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**	**<0.001**
	mean ±std error				**86 ± 13**	**90 ± 15**	**79 ± 14**	**81 ± 12**	**97 ± 11**	**90 ± 12**	**107 ± 14**	**105 ± 13**	**107 ± 12**	**122 ± 17**	**114 ± 14**	**122 ± 15**	**115 ± 14**	**141 ± 19**	**136 ± 14**
Switch × Congruency(Switch effect)	$F_{(1, 14)}$				**6.33**	**5.96**	**7.20**	**5.02**	**15.50**	**5.26**	**5.08**	**5.82**	**8.13**	**19.70**	3.25	**12.68**	1.09	2.45	3.88
	p				**0.025**	**0.028**	**0.018**	**0.042**	**0.001**	**0.038**	**0.041**	**0.030**	**0.013**	**<0.001**	0.093	**0.003**	0.313	0.140	0.069
	mean ± std error				**65 ± 26**	**40 ± 16**	**43 ± 16**	**37 ± 17**	**41 ± 10**	**27 ± 12**	**26 ± 12**	**31 ± 13**	**30 ± 10**	**45 ± 10**	23 ± 13	**45 ± 13**	20 ± 19	29 ± 18	45 ± 23

The middle portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect, and the bottom portion of the table lists the parameter estimates obtained from the ANOVA testing whether the remaining Gratton effect results from the congruency switching hypothesis. Note: The absence of an interaction in the bottom portion is consistent with the switching hypothesis. The bold values signify that $p < 0.05$.

Table 3 | Error rates for each of the four 2 (previous congruency) x 2 (congruency) cells as a function of the proportion of congruent trials.

Condition		Proportion of congruent trials (%)														
		10	15	20	25	30	35	40	45	50	55	60	65	70	75	80
Congruent-congruent		0.8	0.7	1.9	1.1	0.7	1.2	0.7	1.0	0.9	0.7	0.7	0.9	0.7	0.8	0.7
Congruent—incongruent		2.9	2.8	3.5	2.9	3.4	3.0	3.7	4.5	4.0	4.4	4.4	4.8	5.3	5.4	7.1
Incongruent-congruent		0.5	0.9	0.9	1.2	0.9	1.0	1.0	1.0	1.0	1.0	1.0	1.2	0.8	0.9	0.8
Incongruent-incongruent		2.3	2.0	2.4	2.5	2.4	2.4	2.6	2.8	2.3	3.0	3.3	3.3	2.8	3.9	2.9
ANOVA EFFECTS																
Previous congruency	$F_{(1,14)}$	1.12	0.21	4.57	0.20	0.70	2.54	0.58	3.17	1.94	2.25	0.96	1.39	3.39	1.60	**6.85**
	p	0.308	0.657	0.051	0.659	0.415	0.133	0.458	0.097	0.185	0.156	0.344	0.258	0.087	0.226	**0.020**
	mean ± std error	−0.4 ± 0.4	−0.3 ± 0.5	−1.1 ± 0.5	−0.2 ± 0.4	−0.4 ± 0.4	−0.4 ± 0.2	−0.4 ± 0.5	−0.8 ± 0.5	−0.8 ± 0.5	−0.5 ± 0.3	−0.4 ± 0.4	−0.6 ± 0.5	−1.2 ± 0.6	−0.7 ± 0.5	**−2.1 ± 0.8**
Congruency	$F_{(1,\ 14)}$	**5.47**	**14.72**	**7.26**	**5.36**	**5.71**	2.98	**6.47**	**5.51**	**5.14**	**7.37**	**7.27**	**8.02**	**5.66**	**5.96**	**5.79**
	p	**0.035**	**0.002**	**0.017**	**0.036**	**0.032**	0.106	**0.023**	**0.034**	**0.040**	**0.017**	**0.017**	**0.013**	**0.032**	**0.029**	**0.030**
	mean ± std error	**1.9 ± 0.8**	**1.6 ± 0.4**	**1.6 ± 0.6**	**1.5 ± 0.6**	**2.1 ± 0.8**	1.6 ± 0.9	**2.3 ± 0.9**	**2.6 ± 1.1**	**2.2 ± 0.9**	**2.8 ± 1.0**	**3.0 ± 1.1**	**3.0 ± 1.0**	**3.3 ± 1.3**	**3.8 ± 1.5**	**4.3 ± 1.7**
Previous congruency ×	$F_{(1,\ 14)}$	0.14	3.58	0.03	0.66	1.21	1.68	2.25	3.45	2.70	4.32	2.95	3.24	3.98	2.33	**7.88**
Congruency	p	0.711	0.079	0.869	0.429	0.291	0.216	0.156	0.084	0.122	0.056	0.108	0.094	0.066	0.149	**0.014**
(Gratton Effect)	mean ± std error	0.3 ± 0.8	1.0 ± 0.5	0.1 ± 0.9	0.4 ± 0.5	1.1 ± 1.0	0.6 ± 0.4	1.5 ± 1.0	1.7 ± 0.9	1.8 ± 1.1	1.7 ± 0.8	1.5 ± 0.8	1.9 ± 1.0	2.6 ± 1.2	1.6 ± 1.0	**4.3 ± 1.5**

The bottom portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect. The bold values signify that $p < 0.05$.

Despite the general absence of effects, for completeness the same repeated-measures regression analyses performed on the RT data were conducted here. The solid black circles in **Figure 1B** show the size of the Gratton effect as a function of the proportion of congruent trials. The results of this analysis reveal a slope of 0.014 ± 0.032 ms, $t_{(14)} = 0.44$, $p > 0.50$, and an intercept of -0.698 ± 0.802, $t_{(14)} = -0.87$, $p < 0.40$. This is shown as the solid black line in **Figure 1B**.

Excluding stimulus repetitions

Table 4 mirrors **Table 2** with mean percent error rate in place of RTs. The same analysis contained in **Table 2** is shown in the middle of **Table 4**. There was a significant main effect of congruency at each level of proportion congruency [10, 20, 25, 45, 55, 80]. There was a significant main effect of previous congruency only at [40, 65]. There were no significant Gratton effects.

Paired *t*-tests showed that, with the exception of [15, 25, 70] where the ps were marginal (0.095, 0.084, and 0.087 respectively), the size of the Gratton effects in **Tables 3**, **4** were statistically equivalent (*p*s > 0.15).

The solid gray circles in **Figure 1B** show the size of the Gratton effect as a function of the proportion of congruent trials. The same repeated measures regression analysis reported above yielded a slope of 0.038 ± 0.017, $t_{(14)} = 2.26$, $p = 0.040$, and an intercept of -0.250 ± 0.281, $t_{(14)} = -0.89$, $p > 0.35$. This significant slope is difficult to interpret given the fact that there is no significant Gratton effect in any of the proportion conditions. This is shown as the gray line in **Figure 1B**. Although the slope is larger and the intercept is smaller than in the previous analysis which included repetition effects, both are statistically equivalent to the previous parameter estimates, $p = 0.614$ for the slope and $p = 0.627$ for the intercept.

Can the switch hypothesis account for the remaining Gratton effect?

Although there was no significant Gratton effect, a 2 (Switch) × 2 (Congruency) analysis was still conducted at each proportion level. Although these factors only interact at [55, 65, 80], it would be difficult to argue that Switch and Congruency are additive given the absence of a main effect of switch effect at all levels of proportion except the ones at which the factors interact, [55, 65, 80].

DISCUSSION

Experiment 1 reanalyzed a large-scale vocal Stroop study to assess the presence of a conflict adaptation component to the Gratton effect after ruling out three possible sources of binding confounds. There is no contingency bias in the 25% congruency condition. The analyses confirmed the presence of a Gratton effect in this condition, which could not be explained by the congruency switch hypothesis. Experiment 2 provides an independent replication of this result confirming that the Gratton effect remains after controlling for the three biases described by Schmidt and De Houwer (2011).

These analyses revealed two additional findings. First, the size of the Gratton effect in a vocal Stroop task is not affected by the contingencies between the color and the word as indicated by the statistically zero slopes in **Figure 1A**. Second, stimulus repetitions

Table 4 | Error rates for each of the four 2 (previous congruency) × 2 (congruency) cells as a function of the proportion of congruent trials after dividing them into whether they contain target->target, distractor->distractor, target->distractor, or distractor->target repetitions.

Condition	Repetition type				Proportion of congruent trials (%)														
	wW	cC	wC	cW	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80
CONGRUENT–CONGRUENT																			
(1) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$					**1.5**	**0.5**	**2.1**	**1.3**	**0.7**	**1.2**	**0.6**	**1.2**	**1.0**	**0.6**	**0.7**	**0.8**	**0.8**	**0.8**	**0.8**
(2) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$	×	×	×	×	0.0	1.1	0.7	0.7	0.6	1.0	1.0	0.4	0.7	0.9	0.6	1.0	0.5	0.7	0.7
CONGRUENT–INCONGRUENT																			
(3) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$					**2.9**	**3.5**	**4.5**	**3.7**	**3.9**	**4.2**	**4.8**	**5.6**	**4.3**	**6.0**	**5.5**	**6.0**	**6.5**	**6.6**	**8.8**
(4) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$	×			×	4.2	2.2	2.3	1.6	2.2	1.9	2.4	3.6	3.4	1.9	2.9	3.0	3.6	3.8	3.3
(5) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$		×	×		1.8	2.1	3.0	2.4	3.5	2.2	2.7	2.9	4.1	3.7	3.8	4.0	4.2	4.6	6.8
INCONGRUENT–CONGRUENT																			
(6) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{green}}$					**0.9**	**1.0**	**1.3**	**1.1**	**0.7**	**1.0**	**1.2**	**1.0**	**1.0**	**1.0**	**1.1**	**0.9**	**1.1**	**0.8**	**0.9**
(7) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$	×		×		0.3	0.4	0.5	1.6	1.3	1.3	1.4	1.1	1.3	1.3	1.5	2.0	0.6	1.4	0.5
(8) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$		×		×	0.0	1.2	0.6	1.1	0.8	0.7	0.3	1.0	0.9	0.6	0.6	1.1	0.4	0.6	0.8
INCONGRUENT–INCONGRUENT																			
(9) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{yellow}}$					**3.0**	**2.5**	**3.3**	**3.0**	**3.4**	**3.3**	**3.4**	**4.0**	**3.5**	**3.7**	**5.1**	**2.5**	**3.9**	**4.2**	**4.0**
(10) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$	×				2.3	1.8	2.4	2.2	2.0	2.4	2.4	2.4	2.1	4.5	4.9	4.9	2.0	3.8	5.8
(11) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{blue}}$		×			1.7	1.3	1.7	1.4	1.7	1.7	2.2	1.5	1.8	1.9	2.3	1.8	1.7	2.1	1.3
(12) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$	×	×			0.8	1.0	0.9	1.9	0.9	1.1	0.6	0.8	0.9	0.9	1.7	5.3	1.6	1.9	0.0
(13) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{red}}$			×		3.2	3.8	3.3	4.4	3.8	3.8	4.3	4.7	3.1	4.0	5.4	5.3	3.6	8.1	2.5
(14) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{green}}$				×	2.0	1.5	1.8	2.3	2.2	1.5	2.1	2.5	2.0	1.5	0.6	2.7	3.0	3.5	6.1
(15) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$			×	×	1.9	1.5	2.7	2.0	1.4	2.1	2.1	1.8	1.8	3.1	1.3	0.4	3.3	0.0	0.0
GRATTON ANOVA EFFECTS																			
Previous congruency	$F_{(1, 14)}$				4.44	1.31	4.22	0.07	4.24	3.83	**5.42**	2.93	2.63	0.01	0.11	**6.61**	0.99	0.04	0.01
	p				0.054	0.272	0.059	0.789	0.059	0.071	**0.035**	0.109	0.127	0.934	0.748	**0.022**	0.337	0.846	0.932
	mean ± std error				0.8 ± 0.4	0.5 ± 0.4	0.9 ± 0.4	0.1 ± 0.4	1.1 ± 0.5	0.7 ± 0.4	**1.3 ± 0.5**	1.2 ± 0.7	1.1 ± 0.6	0.1 ± 0.8	0.4 ± 1.1	**−3.0 ± 1.1**	1.0 ± 1.0	0.3 ± 1.4	−0.2 ± 2.7
Congruency	$F_{(1, 14)}$				**8.05**	3.66	**8.03**	**6.26**	3.01	4.12	2.88	**8.7**	3.89	**5.43**	3.04	0.03	1.92	2.42	**6.74**
	p				**0.013**	0.076	**0.013**	**0.025**	0.105	0.062	0.112	**0.011**	0.069	**0.035**	0.103	0.867	0.188	0.142	**0.021**
	mean ± std error				**1.4 ± 0.5**	1.0 ± 0.5	**1.6 ± 0.5**	**1.0 ± 0.4**	1.5 ± 0.8	1.5 ± 0.7	1.5 ± 0.8	**2.0 ± 0.7**	1.4 ± 0.7	**2.7 ± 1.1**	3.0 ± 1.7	0.2 ± 1.2	1.3 ± 0.9	2.0 ± 1.2	**4.2 ± 1.6**
Previous congruency × Congruency	$F_{(1, 14)}$				0.09	0.24	0.01	2.36	0.70	0.18	0.49	0.36	0.23	0.94	0.05	0.30	1.36	0.00	0.88
(Gratton effect)	p				0.772	0.635	0.944	0.146	0.416	0.676	0.497	0.557	0.640	0.348	0.832	0.594	0.263	0.972	0.365
	mean ± std error				0.2 ± 0.6	−0.3 ± 0.7	0.0 ± 0.6	−1.3 ± 0.8	−0.6 ± 0.7	−0.2 ± 0.6	0.6 ± 0.9	−0.9 ± 1.5	−0.5 ± 1.0	1.7 ± 1.7	0.5 ± 2.1	−1.0 ± 1.8	−1.9 ± 1.5	−0.1 ± 3.2	3.1 ± 3.2
CONGRUENCY SWITCH ANOVA EFFECTS																			
Switch	$F_{(1, 14)}$				0.17	2.90	0.05	0.41	0.39	0.50	1.70	1.26	0.39	**10.37**	1.39	**5.15**	1.71	1.47	**14.12**
	p				0.684	0.110	0.826	0.534	0.540	0.491	0.214	0.280	0.540	**0.006**	0.257	**0.040**	0.212	0.246	**0.002**
	mean ± std error				−0.3 ± 0.8	0.8 ± 0.4	0.1 ± 0.6	0.3 ± 0.4	0.2 ± 0.4	0.4 ± 0.5	1.1 ± 0.8	0.7 ± 0.6	0.4 ± 0.7	**1.4 ± 0.4**	0.4 ± 0.4	**1.8 ± 0.8**	1.4 ± 1.1	1.2 ± 1.0	**2.5 ± 0.7**
Congruency	$F_{(1, 14)}$				1.73	**7.79**	**6.11**	4.48	**5.44**	**5.50**	**7.10**	**5.84**	**4.97**	**5.45**	3.83	**4.71**	**5.58**	4.06	2.96
	p				0.210	**0.014**	**0.027**	0.053	**0.035**	**0.034**	**0.019**	**0.030**	**0.043**	**0.035**	0.071	**0.048**	**0.033**	0.064	0.107
	mean ± std error				1.8 ± 1.4	**2.2 ± 0.8**	**2.2 ± 0.9**	2.2 ± 1.0	**2.9 ± 1.3**	**2.6 ± 1.1**	**3.2 ± 1.2**	**3.7 ± 1.5**	**2.9 ± 1.3**	**4.1 ± 1.7**	4.4 ± 2.2	**3.4 ± 1.6**	**4.2 ± 1.8**	4.6 ± 2.3	5.6 ± 3.2
Switch × Congruency	$F_{(1, 14)}$				0.09	0.23	2.37	1.97	0.33	0.65	0.29	2.22	0.56	**5.51**	0.00	**6.28**	1.09	1.53	**10.32**
(Switch effect)	p				0.766	0.640	0.146	0.182	0.574	0.435	0.601	0.158	0.467	**0.034**	0.945	**0.025**	0.315	0.237	**0.006**
	mean ± std error				−0.5 ± 1.5	−0.5 ± 1.0	−2.0 ± 1.3	−1.0 ± 0.7	−0.5 ± 0.9	−1.0 ± 1.3	−0.8 ± 1.5	−1.8 ± 1.2	−0.8 ± 1.1	**−1.9 ± 0.8**	−0.1 ± 0.9	**−3.5 ± 1.4**	−2.3 ± 2.2	−2.4 ± 1.9	**−4.7 ± 1.5**

The middle portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect, and the bottom portion of the table lists the parameter estimates obtained from the ANOVA testing whether the remaining Gratton effect results from the congruency switching hypothesis. Note: The absence of an interaction in the bottom portion is consistent with the switching hypothesis. The bold values signify that $p < 0.05$.

have little effect on the size of the Gratton effect in a vocal Stroop task. Although excluding stimulus repetition, in general, reduces the size of the effect, the real impact appears to be on the amount of variance in the size of the Gratton effect. That is, the average Gratton effect collapsed across proportions is 38 ± 5 ms with repetitions and 31 ± 8 ms without. These estimates are statistically equal ($p > 0.30$). However, the standard deviation of the size of the Gratton effect across proportions is 30 ± 3 ms with repetitions and 86 ± 10 ms without. These estimates are quite different, $t_{(14)} = 6.04, p < 0.001$. It is difficult to know whether the increase in variance occurs because of the fact that various stimulus repetition types account for 24.8, 50.5, 49.8, and 83.0% of trials in the CC, CI, IC, and II conditions respectively, or due an unknown psychological construct.

POTENTIAL ISSUES

The astute reader will have identified one potentially important problem with these re-analyses: the proportion of congruent trials—the color-word contingency—was manipulated within-subjects. So, even though a given block of 100 trials in the middle of the session may have a chance-level contingency (i.e., the 25% blocks) between the word and the color, that block was preceded by many blocks of trials in which the contingency between the color and the word was greater that chance.

As noted in the methods, each subject received the same random block order. Coincidently, the first block was the 25% congruent condition. Thus, to avoid any possible longer-term association confounds, analyses looking only at this first block of 100 trials[1] were conducted both including and excluding repetitions. A Gratton effect measuring 42 ± 14 ms was observed with repetitions, $F_{(1,\ 13)} = 8.76, p = 0.011$, and measuring 52 ± 18 ms was observed without repetitions, $F_{(1,\ 13)} = 8.44$, $p = 0.012$. In addition, the remaining Gratton effect cannot be explained by the congruency switch hypothesis. Specifically, response times, excluding repetitions, yields a 70 ± 28 ms congruency switch x congruency interaction $F_{(1,\ 13)} = 6.83, p = 0.020$ comprised of a Stroop effect of 99 ± 20 ms, $t_{(13)} = 5.27$, $p < 0.001$, following a congruency repetition and 29 ± 20 ms, $t_{(13)} = 1.53$, $p = 0.153$, following a congruency switch.

EXPERIMENT 2: A GRATTON ANALYSIS OF THE 25% CONGRUENCY CONDITION

Excluding feature repetitions in Experiment 1 raised an important issue; the absence of a reliable reduction in the Gratton effect when feature repetitions are excluded is inconsistent with the results of previous studies on this issue. We suspect this may have occurred because (1) we had a relatively small sample size for addressing this issue and (2) because proportion was manipulated within-subjects, it may be that subjects actually formed non-zero contingencies between the color and word pairs. To address these concerns, we conducted a new experiment with the goal of replicating these results with a larger sample size and using only the 25% congruency condition. The results from Experiment 2 indicated that there is still a strong Gratton effect after stimulus repetition trials are removed that cannot be explained by a congruency switch, suggesting that color-word contingency plays no role in the Gratton effect.

METHODS

For full methodological details, see Blais et al. (2010). Briefly, thirty subjects were asked to perform 820 trials in a vocal Stroop task. The first 20 trials were considered practice and were used to calibrate the microphone and not included in any of the reported analyses. The remaining 800 trials were divided into four blocks of 200 trials with a self-paced break between them. Importantly, the trials were 25% congruent; 200 trials were congruent and 600 were incongruent, thus any of the four words were equally likely to appear in any of the four colors thereby eliminating all word-color association biases.

RESULTS

ANALYSES OF RTs

Including stimulus repetitions

The same 2 (previous congruency) by 2 (current congruency) ANOVA reported in Experiment 1 was conducted on these data. The results are shown in **Table 5**. There was a significant main effect of congruency, with congruent trials (761 ms) responded to faster than incongruent trials (833 ms), and a significant main effect of previous congruency, with trials on which the previous trial was congruent (783 ms) being responded to faster than trials on which the previous trial was incongruent (811 ms). Critically, these factors interacted to produce a Gratton effect, $F_{(1,\ 29)} = 15.9$, $p < 0.001$: the Stroop effect was larger following congruent trials (92 ± 11 ms) than following incongruent trials (54 ± 8 ms).

Table 5 | Response times and error rates for each of the four 2 (previous congruency) × 2 (congruency) cells in Experiment 2.

Condition		Measure	
		RT	**% errors**
Congruent-congruent		740	0.5
Congruent—incongruent		831	2.9
Incongruent-congruent		784	0.4
Incongruent-incongruent		838	2.0
ANOVA EFFECTS			
Previous congruency	$F_{(1,\ 29)}$	**20.5**	**8.7**
	P	**<0.001**	**0.006**
	mean ± std error	**26 ± 6**	**−0.6 ± 0.2**
Congruency	$F_{(1,\ 29)}$	**79.0**	**49.1**
	P	**<0.001**	**<0.001**
	mean ± std error	**73 ± 8**	**1.9 ± 0.3**
Previous congruency × Congruency (Gratton effect)	$F_{(1,\ 29)}$	**14.0**	**5.0**
	p	**<0.001**	**0.033**
	mean ± std error	**37 ± 10**	**0.8 ± 0.4**

The bottom portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect. The bold values signify that $p < 0.05$.

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[1] One subject was dropped from the analysis because they had no observations in the congruent-congruent condition after excluding repetition trials.

Excluding stimulus repetitions

The same 2 (previous congruency) by 2 (current congruency) ANOVA reported in Experiment 1, but excluding stimulus repetitions, was conducted on these data. The results are shown in **Table 6**. There was a significant main effect of congruency, with congruent trials (795 ms) responded to faster than incongruent trials (867 ms), $F_{(1,\ 29)} = 57.6$, $p < 0.001$ and a significant main effect of previous congruency, with trials on which the previous trial was congruent (817 ms) being responded to faster than trials on which the previous trial was incongruent (845 ms), $F_{(1,\ 29)} = 14.1$, $p < 0.001$. Critically, these factors interacted to produce a Gratton effect, $F_{(1,\ 29)} = 10.1$, $p < 0.005$: the Stroop effect was larger following congruent trials (89 ± 11 ms) than following incongruent trials (56 ± 10 ms).

Can the switch hypothesis account for the remaining Gratton effect?

To assess whether the remaining Gratton effect is the result of congruency switching (i.e., Schmidt and De Houwer, 2011), a 2 (Switch) × 2 (Congruency) analysis was conducted on data after excluding stimulus repetitions. Again, an in-depth description of the logic of this analysis is provided by Schmidt and De Houwer (2011). In short, additive effects of congruency switch (whether the congruency on the previous trial is the same, or different, on the current trials) and congruency are consistent with an interpretation in which the Gratton effect results from a reconfiguration switch cost. An interaction of these factors cannot rule out a conflict adaption account of the Gratton effect. There was a main effect of congruency, with congruent trials (795 ms) responded to faster than incongruent trials (867 ms), $F_{(1,\ 29)} = 57.6$, $p < 0.001$. There was a switch cost, congruency switch trials (839 ms) were responded to slower than congruency repetition trials (823 ms), $F_{(1,\ 29)} = 10.1$, $p < 0.005$. Critically, these factors interact, rather than add, $F_{(1,\ 29)} = 14.1$, $p < 0.001$, the Stroop effect is larger on congruency repetition trials (101 ± 12 ms) vs. congruency switch trials (45 ± 12 ms), thereby ruling out the switch hypothesis as an explanation for the remaining Gratton effect.

ANALYSES OF ERRORS

The overall rate rates was only 1.4%, but for the sake of completeness, the entire set of analyses performed on RTs was also done on error rates.

Including stimulus repetitions

The same 2 (previous congruency) by 2 (current congruency) ANOVA reported in Experiment 1 was conducted on these data. There was a significant main effect of congruency, with congruent trials (0.4%) responded to more accurately than incongruent trials (2.3%), and a significant main effect of previous congruency, with trials on which the previous trial was congruent (1.2%) being responded to more accurately than trials on which the previous trial was incongruent (1.7%), $F_{(1,\ 29)} = 7.5$, $p < 0.05$. Critically, these factors interacted to produce a Gratton effect; the Stroop effect was larger following congruent trials (2.4 ± 0.4%) than following incongruent trials (1.5 ± 0.2%).

Table 6 | Response times and error rates for each of the four 2 (previous congruency) × 2 (congruency) cells in Experiment 2 after dividing them into whether they contain target->target, distractor->distractor, target->distractor, or distractor->target repetitions.

Condition	Repetition type				Measure	
	wW	cC	wC	cW	RT	% errors
CONGRUENT–CONGRUENT						
(1) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$					**773**	**0.6**
(2) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$	×	×	×	×	644	0.4
CONGRUENT–INCONGRUENT						
(3) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$					**863**	**3.1**
(4) $\text{BLUE}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$	×			×	823	2.1
(5) $\text{BLUE}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$		×	×		777	3.0
INCONGRUENT–CONGRUENT						
(6) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{green}}$					**817**	**0.3**
(7) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{red}}$	×		×		800	0.4
(8) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{blue}}$		×		×	697	0.2
INCONGRUENT–INCONGRUENT						
(9) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{yellow}}$					**878**	**2.1**
(10) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{green}}$	×				859	1.9
(11) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{blue}}$		×			764	1.5
(12) $\text{RED}_{\text{blue}} \rightarrow \text{RED}_{\text{blue}}$	×	×			698	0.6
(13) $\text{RED}_{\text{blue}} \rightarrow \text{GREEN}_{\text{red}}$			×		894	2.4
(14) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{green}}$				×	846	1.3
(15) $\text{RED}_{\text{blue}} \rightarrow \text{BLUE}_{\text{red}}$			×	×	873	2.8
GRATTON ANOVA EFFECTS						
Previous Congruency	$F_{(1,\ 29)}$				**16.2**	**5.2**
	p				**<0.001**	**0.030**
	mean ± std error				**29 ± 7**	**−0.6 ± 0.3**
Congruency	$F_{(1,\ 29)}$				**57.9**	**31.9**
	p				**<0.001**	**<0.001**
	mean ± std error				**76 ± 10**	**2.1 ± 0.4**
Previous congruency × Congruency (Gratton effect)	$F_{(1,\ 29)}$				**4.8**	1.7
	p				**0.036**	0.204
	mean ± std error				**28 ± 13**	0.7 ± 0.6
CONGRUENCY SWITCH ANOVA EFFECTS						
Switch	$F_{(1,\ 29)}$				**4.8**	1.7
	p				**0.036**	0.204
	mean ± std error				**14 ± 6**	−0.4 ± 0.3
Congruency	$F_{(1,\ 29)}$				**57.9**	**31.9**
	p				**<0.001**	**<0.001**
	mean ± std error				**76 ± 10**	**2.1 ± 0.4**
Switch × Congruency (Switch effect)	$F_{(1,\ 29)}$				**14.5**	**5.2**
	p				**<0.001**	**0.030**
	mean ± std error				**58 ± 14**	**1.2 ± 0.6**

The middle portion of the table lists the parameter estimates obtained from the ANOVA testing for the presence of a Gratton effect, and the bottom portion of the table lists the parameter estimates obtained from the ANOVA testing whether the remaining Gratton effect results from the congruency switching hypothesis. Note: The absence of an interaction in the bottom portion is consistent with the switching hypothesis. The bold values signify that p < 0.05.

Excluding stimulus repetitions

The same 2 (previous congruency) by 2 (current congruency) ANOVA reported in Experiment 1, but excluding stimulus repetitions, was conducted on these data. There was a significant main effect of congruency, with congruent trials (0.5%) responded to more accurately than incongruent trials (2.6%), and a significant main effect of previous congruency, with trials on which the previous trial was congruent (1.8%) being responded to less accurately than trials on which the previous trial was incongruent (1.2%). These factors failed to interact to produce a Gratton effect; the Stroop effect was statistically equivalent following congruent trials (2.5 ± 0.6%) than following incongruent trials (1.8 ± 0.3%).

Can the switch hypothesis account for the remaining Gratton effect?

To assess whether the remaining Gratton effect is the result of congruency switching (i.e., Schmidt and De Houwer, 2011), a 2 (Switch) × 2 (Congruency) analysis was conducted on data after excluding stimulus repetitions. Again, an in-depth description of the logic of this analysis is provided by Schmidt and De Houwer (2011). In short, additive effects of congruency switch (whether the congruency on the previous trial is the same, or different, on the current trials) and congruency are consistent with an interpretation in which the Gratton effect results from a reconfiguration switch cost. An interaction of these factors cannot rule out a conflict adaption account of the Gratton effect. There was a main effect of congruency, with congruent trials (0.5%) responded to faster than incongruent trials (2.6%). There was no switch cost; congruency switch trials (1.7%) were responded equivalently to congruency repetition trials (1.3%). Critically, these factors interact, rather than add; the Stroop effect is larger on congruency repetition trials (2.8 ± 0.6%) vs. congruency switch trials (1.5 ± 0.3%), thereby ruling out the switch hypothesis as an explanation for the remaining Gratton effect.

DISCREPANCIES WITH SCHMIDT AND DE HOUWER

What seems clear from the current set of experiments using a vocal Stroop task is that at least part of the Gratton effect results from conflict adaptation. The vocal Stroop task has been called the gold standard of attention measures (e.g., MacLeod, 1992). Anyone who has done a standard vocal Stroop task knows how difficult it is to not blurt out the word. That is, individuals must suppress an extremely strong, obligatory urge to read the word. That is, it seems fairly obvious that the desire to read aloud the word is the major source of conflict. Thus, conflict adaptation is more likely in this task because conflict is more pervasive, and hence more disturbing, for participants (e.g., Desender et al., 2014).

In the manual Stroop task and the flanker task, the source of conflict is less clear. That is, it seems that there are at least two major sources of conflict. The first source is the distractor item. The presence of Stroop and flanker effects is consistent with this interpretation. But, there must be at least one other source of conflict given that many errors are random in the sense that subjects are not responding to the distractor feature, but to a non-presented feature (i.e., the participant said “red” to $\text{BLUE}_{\text{green}}$). For example, in a four-choice flanker task, you can make an error by hitting any one of the three non-target keys. If errors are completely random, one would expect a 33.3% chance of hitting any of them. As it turns out, subjects tend to make significantly more “distractor errors”; 46.6% compared to 26.7% (e.g., see Maier et al., 2011). Although significant, subjects still make a majority of these random errors, perhaps because they are responding too quickly. In the vocal Stroop task, this type of error is rare. In fact, Experiment 1, containing over 270,000 trials, there were less than a dozen errors of this nature where “blue” was said to $\text{YELLOW}_{\text{red}}$. The high random error rate in the manual tasks is consistent with the idea that subjects have a much weaker internal representation of which key is associated with which response. This continual need for control to maintain the response sets likely creates a second source of conflict.

Errors in the vocal Stroop task are rare, but errors are among the most subjectively salient need-for-control cues. Behaviorally, they yield a post-error slowing effect on subsequent trials (Rabbitt, 1966; Laming, 1979; Unsworth et al., 2012). This slowdown has been correlated with activity in anterior cingulate cortex (e.g., Yeung et al., 2003). In fact, this error-related negativity is often strong enough to be observed on single trials in unprocessed event related potentials (ERPs) and fMRI BOLD signals. However, Schmidt and De Houwer fail to exclude such trials from their analysis. Perhaps more important than post-error slowing is the observation that errors are often followed by a reduction of conflict effect producing a Gratton-like effect. For instance, Maier et al. (2011) have shown that this effect occurs only after incongruent flanker errors.

Therefore, there are two additional sources of conflict that might better be characterized as sustained control in the manual data. First, subjects must maintain, relatively unpracticed, button mappings. This source of conflict is unlikely to vary trial-to-trial. Second, these weak button maps lead to a large number of errors. Indeed, the subjects in Experiment 1 from Schmidt and De Houwer (2011) made approximately 13% errors overall; our subjects were <2%. Critically, trials following errors are slowed. This will obviously vary from trial-to-trial, but Schmidt and De Houwer failed to remove this source of conflict[2]. In theory, this should lead to the observation of a Gratton effect, but due to the unusually high number of errors overall, it is unclear if hypothesis this will hold.

CONCLUSION

Starting with the original Mayr et al. (2003) paper, there has been a heated debate as to whether conflict adaptation plays a role in the Gratton effect over and above feature repetition binding biases. The answer to this question appears to be yes (e.g., Ullsperger et al., 2005). Schmidt and De Houwer (2011) identified two additional binding confounds: inherent contingencies between the relevant and irrelevant dimensions, and congruency switching effects. Using manual versions of the Stroop and flanker tasks, they make the strong claim that conflict adaptation is not necessary to explain the Gratton effect. Yet in a vocal Stroop task, we show that nearly all of the Gratton effect is the result of conflict adaption. The more important question, however, is whether conflict adaptation occurs “in real life”; not in any particular lab task. Further research is necessary to allow us to identify the role of conflict adaptation in more naturalistic situations.

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[2] Excluding trials following errors has no qualitative impact on our findings.

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Contingency and congruency switch in the congruency sequence effect: a reply to Blais, Stefanidi, and Brewer (2014)

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A commentary on

The Gratton effect remains after controlling for contingencies and stimulus repetitions

by Blais, C., Stefanidi, A., and Brewer, G. (2014). Front. Psychol. 5:1207. doi: 10.3389/fpsyg.2014.01207

INTRODUCTION

The congruency sequence effect (CSE) is the observation that the congruency effect is reduced following an incongruent trial (Gratton et al., 1992). Generally, the CSE is interpreted in terms of *conflict adaptation*, the idea that participants decrease attention to the distracter and/or increase attention to the target after experiencing conflict (e.g., Botvinick et al., 2001). An alternative *learning and memory* account proposes that the CSE is instead due to basic learning confounds (for a review, see Schmidt, 2013). For instance, systematic differences in the types of feature repetitions that are possible in each cell of the design might produce a CSE (Mayr et al., 2003; Hommel et al., 2004). Schmidt and De Houwer (2011) considered two additional learning and memory biases: sequential contingencies and congruency switch costs. However, Blais et al. (2014) present data which they suggest argue against a role of these two biases. This article illustrates some issues with this work and suggests that contingency and congruency switch biases may play a role after all.

CONTINGENCY ANALYSIS UNDERPOWERED

Schmidt and De Houwer (2011) introduced the idea that *sequential contingency* biases might influence the CSE. Often, each distracter is presented more frequently in the congruent color than in each incongruent color. Unfortunately, this introduces a contingency, whereby words are predictive of the congruent response. Contingency biases are larger following an accurately predictive trial than following an incorrectly predictive trial (Schmidt et al., 2007). Thus, contingency biases can contribute to the CSE. Indeed, Mordkoff (2012) showed that, with feature repetitions removed, the CSE *is* present in a contingency-biased Simon task, but *is not* observed in a contingency-unbiased version of the same task.

Blais et al. (2014) report a reanalysis of verbal Stroop data in which each participant performed several blocks of trials with varying contingencies (as manipulated with proportions of congruent trials) from 5 to 95% in increments of 5% (though only 10 to 80% could be analyzed). Overall, CSEs were not reliable for most contingency levels. Critically, the CSE did not significantly increase as a function of contingency in response times. With these data, the authors argued that contingencies are unlikely to play a role in the CSE.

However, statistical power of the sample of 15 participants is a concern. Indeed, the slope was notably positive, but with considerable error, $B = 0.438 \pm 0.382$, $t_{(14)} = 1.15$, $p > 0.25$. The B parameter is the amount of change in the CSE for a 1% contingency increment (i.e., 2.2 ms for each 5% increment, and 31 ms overall). Though not significant, this represents a medium effect size ($\beta = 0.315$). As **Figure 1** illustrates, the study only had high (0.8) power to detect a large effect size ($\beta = 0.661$). As a further concern, contingencies were manipulated between blocks. Contingency biases are known to transfer across blocks (Schmidt et al., 2010), causing contamination. Thus, contingency biases were probably underestimated. Curiously, the slope for the errors *was* significant and in the predicted direction, $B = 0.038 \pm 0.017$, $t_{(14)} = 2.26$, $p = 0.040$. Though seemingly confirming a role of contingencies, the authors reasoned that this slope is difficult to interpret given that none of the CSE estimates for the various contingency levels were significant. This argument does not seem particularly convincing, only feeding concerns about statistical power.

CONGRUENCY SWITCH HYPOTHESIS, REVISED

Schmidt and De Houwer (2011) further considered the possibility that there might be encoding costs associated with "switching" from a congruent to an incongruent trial, or vice versa, relative to repeating the same type of trial. Thus, following an incongruent trial, incongruent trials will incur a benefit and congruent trials a loss. The reverse is true following a congruent

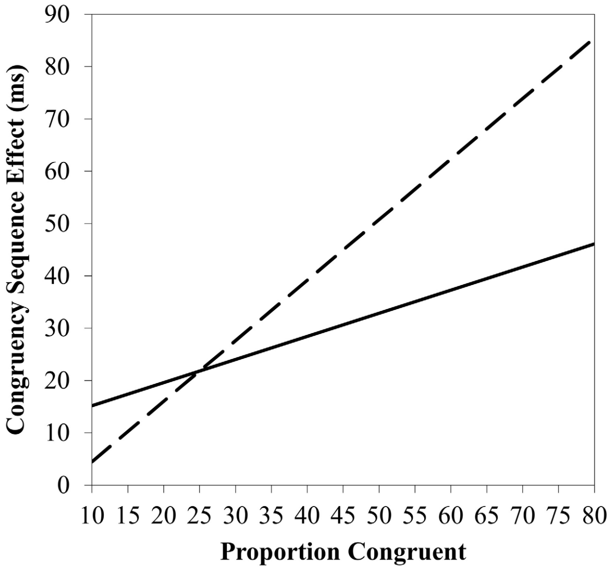

FIGURE 1 | Congruence sequence effect as a function of proportion congruency, with observed trend line (solid line) and trend line that would have been required for a high power test given the sample size and error (dashed line).

trial. As a result, *congruency switch costs* can further explain variance in the CSE.

Schmidt and De Houwer (2011) suggested that the cost of switching from congruent to incongruent might be "roughly" the same as the reverse, but Blais et al. (2014) did not observe this additivity. In retrospect, this was a misguided prediction. It is known that switching from a hard (non-dominant) to an easy (dominant) task sometimes incurs a larger cost on performance than the reverse, known as *switch cost asymmetry* (see Allport et al., 1994). The same might be predicted here, where a congruent "encoding shortcut" might be especially fast following a congruent trial, whereas the harder encoding task on incongruent trials will take long regardless of the previous trial congruency. This is an intriguing suggestion, because the conflict adaptation account should predict the exact opposite: because Stroop effects are primarily interference driven (see MacLeod, 1991), changes in attention to the word should be reflected primarily in incongruent trials.

Looking closely at the data of Blais et al. (2014), it can be seen that the interaction between congruency and congruency switch is due entirely to a larger effect of congruency switch for congruent trials (Experiment 1: 32 ms; Experiment 2: 44 ms) than for incongruent trials (Experiment 1: −4 ms; Experiment 2: −15 ms). Thus, this interaction is inconsistent with the conflict adaptation account, but is consistent with a revised version of the congruency switch hypothesis.

CONCLUSION

Though the current paper does not contest the notion that CSEs can be observed independent of feature repetition and contingency learning biases (e.g., Kim and Cho, 2014; Schmidt and Weissman, 2014; Weissman et al., 2014), three inferences of Blais et al. (2014) are contestable. First, contingency biases probably do play a role in the effect, as indicated by the significant effect in the errors, the underpowered but notable trend in the response times, and the data of Mordkoff (2012). Second, congruency switch effects might also play a role, as indicated by the direction of the switch cost asymmetry. Third, Blais and colleagues too quickly attribute the remaining CSEs to conflict adaptation. Yet other accounts still remain, such as the temporal learning and activation-suppression accounts, which actually seem to fit the extant data better than the conflict adaptation account (e.g., see Weissman et al., 2014, Schmidt and Weissman, in review).

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Context-specific temporal learning with non-conflict stimuli: proof-of-principle for a learning account of context-specific proportion congruent effects

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The conflict adaptation account proposes that participants adjust attention to target and distracting stimuli in response to conflict. This is argued to explain the proportion congruent effect, wherein the congruency effect decreases as the proportion of conflicting incongruent trials increases. Some reports further argue that this conflict adaptation process can be context-specific. This paper presents a proof-of-principle for a competing account. It is suggested that such context-specific effects might be driven by very basic temporal learning processes. In the reported experiment, we manipulated stimulus contrast in place of congruency. In one location, stimulus letters were mostly easy to identify (high stimulus contrast). In the other location, letters were mostly hard to identify (low stimulus contrast). Participants produced a larger contrast effect in the mostly easy context. Along with supplemental analyses investigating the role of context switching and previous trial response times, the results are consistent with the notion that different rhythms of responding are learned for an easy versus hard location context. These results suggest that context-specific proportion congruency effects might result, in whole or in part, from temporal learning. Conflict adaptation may or may not play an additional role.

Keywords: context, temporal learning, proportion congruency, conflict adaptation, cognitive control, attention, contingency learning, contrast

INTRODUCTION

Learning about *when* to respond is arguably as important as learning *what* to respond when interacting with the environment. Whether for determining the causal relation between events or using said causal knowledge to optimally respond to stimuli in the fastest and most accurate manner possible, both contingency and temporal information are critical for successful performance. In the context of psychological experiments, detecting regularities allows for the anticipation of future events on subsequent trials, thus benefiting performance when expectations match reality. For instance, when the series of responses in a task follow a predictable order, responses are sped up relative to a random ordering of responses (Nissen and Bullemer, 1987). Similarly, if a neutral distracting stimulus is predictive of the likely target stimulus, performance is aided when the expected stimulus is presented (e.g., Miller, 1987; Schmidt et al., 2007). Also with timing information, if a cue indicates the likely time at which a stimulus will appear, performance is sped up if the stimulus appears at the expected time (e.g., Hsu et al., 2013).

The learning of contingent and temporal regularity occurs quite easily and has near immediate effects on behavior. One somewhat unfortunate consequence of this fact is that many experiments aimed at investigating something else entirely might end up unintentionally biased by such learning confounds when a regularity exists in the task structure that is learnable by participants. Of particular interest to the present article, much debate has focused on the presence of such learning biases in the cognitive control literature (for a recent review, see Schmidt, 2013a). This paper has two main goals. The more general goal is to investigate the potential role of contextual information in moderating learning, particularly temporal learning. The more specific goal is to discuss how context-specificity in temporal learning relates to an interesting finding in the cognitive control literature, namely, the context-specific proportion congruent (PC) effect. We begin by providing a background for the latter of these two goals, and will then return to the former.

When given the goal to selectively attend to one stimulus while simultaneously ignoring another distracting stimulus, participants are not completely successful at doing so. For instance, in the Stroop task (Stroop, 1935) participants see color words printed in colors. On a *congruent trial*, the word and color match (e.g., the word "blue" printed in blue); whereas on an *incongruent trial*, the word and color mismatch (e.g., "blue" in red). Responses to incongruent trials are slower and less accurate than congruent trials. This congruency effect indicates that the distracting word has partially slipped through the attentional filter, producing conflict.

In the cognitive control literature, it is often assumed that the attentional system adapts to conflict by adjusting the allocation of attentional resources away from the source of conflict (e.g., the distracting word) and/or toward the target stimulus (e.g., the color). This is called the *conflict adaptation account*. One piece of evidence argued to support the conflict adaptation account is the *PC effect*: the greater the proportion of incongruent trials, the smaller the congruency effect (Lowe and Mitterer, 1982). According to the conflict adaptation account (e.g., Botvinick et al., 2001), this occurs because participants detect frequent conflict and adjust attention away from the word.

However, others have argued that the cognitive system might not be reactive to conflict. Instead, findings such as the PC effect might be driven by regularities in the task structure that allow for learning biases (e.g., Schmidt and Besner, 2008; Schmidt, 2013b,c; for a review, see Schmidt, 2013a). For instance, Schmidt and Besner (2008) (see also Mordkoff, 1996; Jacoby et al., 2003; Schmidt, 2013c) argue that such an effect may be largely explained by simple word–color contingency learning. When most of the trials are congruent, then each word is presented most often in its congruent color. The word is therefore a valid cue of the response on congruent trials, and invalid on incongruent trials, thereby increasing the congruency effect. When most of the trials are incongruent, the bias is eliminated or even reversed (depending on the specific manipulation). Thus, the PC effect might be driven by contingency biases, rather than conflict adaptation.

The contingency account often seems to undermine the conflict adaptation account in explaining both behavioral (e.g., Atalay and Misirlisoy, 2012; Schmidt, 2013c) and brain data (Grandjean et al., 2013). However, there are still several findings that may not seem to fit well with the simple learning view. One such finding is the context-specific proportion congruent (CSPC) effect. Corballis and Gratton (2003) used a flanker task in which distracting letters were mostly congruent with the target letter on one side of the screen (e.g., left), but mostly incongruent on the other side of the screen (e.g., right). The congruency effect was larger in the mostly congruent relative to mostly incongruent location. This was initially argued as evidence for hemispheric-specificity in conflict adaptation. However, Crump et al. (2006) replicated this effect in a Stroop-like procedure with *up* and *down* locations, rather than left and right, and argued instead that participants simply adapt to conflict differently in different contextual locations (see also, Wendt et al., 2008). That is, in the location with mostly incongruent trials attention to the word is reduced relative to the location with mostly congruent trials. Similarly, Bugg et al. (2008) presented color–word Stroop stimuli in two fonts. With one font, the word was mostly congruent; and with the other, the word was mostly incongruent. The congruency effect was larger for the mostly congruent font. This again might suggest that participants can dynamically (i.e., on a trial-to-trial basis) adjust their attention to distracting words on the basis of contextual cues, such as fonts or locations.

What is interesting about CSPC effects is that they have been argued to be difficult to explain via a contingency learning account. The distracting word, for instance, is not predictive of which response to make. Indeed, every word is both mostly congruent *and* mostly incongruent, depending on the context. Similarly, the context cue (e.g., location) is not predictive of what response to make. Thus, such data have been taken as strong support for the notion that conflict adaptation does occur. However, the CSPC effect could be explained with a contingency learning mechanism by assuming that multiple irrelevant cues are combined to predict the response. For instance, in the mostly congruent location "blue" predicts a blue response, whereas in the mostly incongruent location "blue" predicts a green response. Thus, it is not unreasonable to assume that participants might combine two distracting cues, such as the word and stimulus location, to jointly predict the response. Indeed, Mordkoff and Halterman (2008) demonstrated exactly this in three flanker experiments. Color and shape combinations were used as flankers to unrelated target stimuli. Just like in a CSPC experiment, each flanker color was predictive of one response (e.g., left key press) when presented with one shape, and the other response (e.g., right) when presented with a different shape. Participants responded faster for the high contingency color–shape conjunctions than the low-contingency color–shape conjunctions. More generally, work on occasion setting suggests that participants can use conjunctive stimulus information quite readily (for a review, see Holland, 1992).

However, even if contingency learning can be context-specific in this way, there are still results that are not explainable by such a mechanism. Of particular interest, Crump and Milliken (2009; see also Heinemann et al., 2009; Reuss et al., 2014) used both context and transfer items in a single CSPC experiment. *Context items* were manipulated for PC across locations. *Transfer items* were not manipulated for PC, having equal congruency proportions in both locations. While combining the word and the location might provide predictive information on context items, this would be impossible for transfer items. A CSPC effect was observed for *both* item types, however. A contingency learning account is therefore unsatisfactory for explaining the effects on transfer items, because such items were frequency-unbiased. Indeed, the fact that an effect occurs for the transfer items at all indicates that behavior is being influenced, at least in part, in a non-item-specific way. That is, transfer items only produce a CSPC effect due to the influence of the intermixed context items. Contingency biases likely do play some role in producing the CSPC effect, but it is clear that they are not the whole story.

However, another learning bias that might explain a component of CSPC effects, particularly for frequency-unbiased transfer items, is temporal learning. In addition to learning what response to make to a stimulus, participants can also learn the timing of responses (Matzel et al., 1988; Taatgen and Van Rijn, 2011). According to the *temporal learning account*, participants learn about *when* to respond based on the rhythm of the task (e.g., Grosjean et al., 2001). This timing information can influence subsequent behavior. For instance, the speed of responding to one task affects the speed of responding to a second, intermixed task (for a review, see Los, 1996). Note that such effects are by definition not (entirely) item-specific: performance on some items affects performance on others. Multiple potential mechanisms for such effects have been proposed. For instance, temporal information might be used to alter response caution (Van Maanen et al., 2011), to balance speed and accuracy (Kinoshita and Mozer, 2006), or to make time-criterion adjustments (Lupker et al., 1997).

There are multiple possible mechanisms for temporal learning. Most can explain components of the PC effect. For instance, Schmidt (2013b) suggested that participants develop expectancies of when they will be able to respond on the basis of previous trials.

When a response is sufficiently active at the expected time, a shortcut in responding can occur. In the mostly congruent condition, the expected time to respond will be relatively early in the trial, due to the preponderance of congruent trials. Subsequent congruent trials will thus benefit from this temporal expectancy, because a response is likely to be active enough at the expected time, allowing a response to be produced even faster than usual (e.g., because the response threshold is temporally decreased at the expected time). In contrast, incongruent trials will not benefit, because there will be insufficient evidence for the correct response at the expected time, meaning that evidence has to continue accruing and the expectancy window is missed. This produces a relatively large congruency effect. In the mostly incongruent condition, the expected time to respond will be relatively later in the trial, due to the preponderance of incongruent trials. Subsequent incongruent trials will thus benefit from this temporal expectancy, because evidence for the correct response will be strong enough at the expected time to benefit from an expectancy-based shortcutting in responding. In contrast, congruent trials will not benefit, because a response will have already been made before the expected time to respond, thus missing out on the added benefit of matching the rhythm of previous trials. This produces a relatively smaller congruency effect.

According to the temporal learning account, the PC effect is not due to conflict *per se*, but merely to the speed of responding in the task. In support of this, large portions of the PC effect are driven by the speed of responding to previous trials (Kinoshita et al., 2011; Schmidt, 2013b). Moreover, Schmidt (2013b) showed that a “pseudo” PC effect can be produced without manipulating conflict and without the presence of a distracting stimulus. Instead, target letters were manipulated for stimulus contrast, with high contrast (easy to see) and low contrast (hard to see) letters. Thus, fast (high contrast) and slow (low contrast) responses are still made, but without a conflict manipulation. To manipulate timing, letters were presented most often in high contrast (mostly easy) for half of the participants and most often in low contrast (mostly hard) for the other half. The contrast effect (i.e., low minus high contrast trials) was larger in the mostly easy relative to the mostly hard condition. This *proportion easy effect* therefore parallels the PC effect. It was further demonstrated by Schmidt (2014) that the proportion easy effect is *not* (at least primarily) item-specific. Some context letters were manipulated for “proportion easy” whereas other intermixed transfer items were not. While there was some (non-significant) hint that the proportion easy effect might have been larger for context items, a reliable proportion easy effect was observed for both item types. This indicates that participants can learn the overall speed of the task, which then produces larger effects (of whatever sort) in the mostly easy context relative to the mostly hard context, even for frequency-unbiased transfer items.

It might then be suggested that participants in the experiment of Crump and Milliken (2009) produced larger congruency effects in the mostly congruent location because of a different temporal expectancy for each location. In other words, temporal learning might be context-specific. The expectancy for a relatively quick response for stimuli presented in the mostly congruent location (i.e., due to a preponderance of fast congruent trials) benefits congruent trials, whereas the expectancy for a relatively slow response for stimuli presented in the mostly incongruent location (i.e., due to a preponderance of slow incongruent trials) benefits incongruent trials. It is, of course, extremely difficult to de-confound temporal learning and conflict biases in an experiment, because proportion congruency and the proportion of fast responses are inherently confounded. Thus, it is difficult to conceive a way to test for conflict adaptation effects independent of temporal biases. It is possible, however, to provide a proof-of-principle that temporal learning biases can produce an interaction that mimics the CSPC effect, even in the absence of conflict in the task. In that vein, the current experiment aims to test the context-specific temporal learning account by manipulating the proportion of easy (high contrast) items across two display locations. If the contrast effect is observed to be larger in the mostly easy location than the mostly hard location, then this would be consistent with the notion that temporal learning can be context-specific.

Such a finding would also provide an important proof-of-principle that the context-specific temporal learning account might provide a viable alternative interpretation of the CSPC effect. Because there is no *conflict* in the task (indeed, there are no distracters at all), the conflict adaptation account would not predict an effect. The temporal learning account, on the other hand, merely assumes that the difference in response time (RT) between easy and hard items is influenced by the proportion of easy and hard items. In other words, it is important that participants respond faster in one context than the other, but it is not important *why* participants respond faster or slower in a given context. Thus, an experiment constructed analogously to a CSPC task but without a manipulation of conflict, should nevertheless produce the same interaction.

In the interest of better understanding context-specificity in temporal learning, the experiment tested two other side questions. First, it seemed possible that the proportion easy effect might be dependent in some way on whether the context of the previous trial matched the context of the current trial. For instance, it might be the case that a context-specific proportion easy effect is only observed on context (i.e., location) repetition trials, where the context remains consistent from one trial to the next. On context alternation trials, it might be the case that there is a cost of shifting contexts that results in the elimination (or reduction) of temporal expectancies. Evidence consistent with this notion is present in CSPC studies (King et al., 2012a,b).

Second, it is known that the RT of the immediately preceding trial has a large influence on performance on the next trial. For instance, not only is previous trial RT highly correlated with current trial RT, but congruency effects increase the faster the previous trial RT (e.g., Kinoshita et al., 2011; Schmidt, 2013b). This is highly consistent with time-based learning accounts. In these accounts, participants learn *when* to respond on the basis of previous performance, so it only stands to reason that the immediately-preceding trial should have some measurable impact on current trial performance. This follows the same logic already discussed when explaining the temporal learning account. For instance, following a fast response to an easy item, another easy item should be able to be responded to particularly fast (just like a fast rhythm of responding should benefit responding more on congruent than incongruent trials). Following a slow response to a hard item, another hard item should be responded to faster than typical (just like a slow rhythm of responding should benefit responding more on incongruent than on congruent trials). Because the temporal learning account is proposed to explain the context-specific proportion easy effect, a few predictions follow. First but least important, previous and current RT should correlate. Second, the contrast effect should be larger the faster the previous RT. Again, this is because easy trials will benefit most following a fast (easy) trial, whereas hard trials will not, resulting in a large contrast effect following fast responses. The exact opposite is true following a slow (hard) trial, resulting in smaller contrast effects following slower RTs. Third, the impact of previous trial RT should be especially large when the context repeats from one trial to the next. When the context alternates, the previous RT does not correspond to the same context, so the effect should be attenuated. We therefore conducted analyses that were specifically designed to assess these predictions. Notably, it is unclear why the conflict adaptation or any other attentional filtering account should make any of these predictions regarding previous RT. First, there is no conflict in the task to adapt to. Second, these accounts merely argue that attention is adapted to the conflict level associated with a given context, not to time information.

MATERIALS AND METHODS

PARTICIPANTS

Participants were 60 Ghent University undergraduates who participated in exchange for €5. The research was approved by the Ethical Committee at Ghent University. Participants provided informed consent before participating.

APPARATUS

Stimulus and response timing were controlled by E-Prime 2 (Psychology Software Tools, Sharpsburg, PA, USA). Participants responded to the letters D, F, J, and K with the D, F, J, and K keys of an AZERTY keyboard of a laptop PC, respectively. The laptop PC had a 15″ monitor.

MATERIALS AND DESIGN

The stimulus letters for the experiment were D, F, J, and K. Letters were presented on a dark gray background (100,100,100). Before the main experiment, there was a 24-trial practice phase, consisting of six presentations of each letter in the center of the screen in white (255, 255, 255). In the main phase of the experiment, letters were presented in either high contrast gray (200, 200, 200) or low contrast gray (110, 110, 110), for a total of eight unique letter–contrast combinations. Letters were presented in bold, 18 pt Courier New font on a 640 × 480 resolution screen setting. On half of the trials, the letter appeared on the top half of the screen (four lines up from the center), and on the other half of the trials on the bottom half of the screen (four lines down from the center). In one location (mostly easy), each letter was presented 70% of the time in high contrast and 30% in low contrast. In the other location (mostly hard), the proportions were reversed. Which location (above or below) served as the mostly easy location was counterbalanced across participants. The contrast effect is defined as the difference between high and low contrast trials. A context-specific proportion easy effect is defined as a larger contrast effect in the mostly easy location context relative to the mostly hard context. For the main part of the experiment, there were a total of 300 trials selected at random with replacement.

PROCEDURE

Participants were instructed to press the key corresponding to the letter on the screen (e.g., press the K key for the letter K). Each trial began with a blank screen for 500 ms, followed by the target letter for 2000 ms or until a response was made. Correct responses were immediately followed by the next trial, whereas incorrect responses and trials on which participants failed to respond in 2000 ms were followed by a centrally located "XXX" in red (255, 0, 0) for 500 ms.

RESULTS

Mean correct RTs and percentage errors were calculated. Analyses were conducted with a linear mixed effect (LME) model in order to assess any overall benefits for stimuli presented in one of the two stimulus locations and/or interactions between stimulus location and the other factors. Note that such an analysis cannot easily be performed with a standard repeated-measures ANOVA[1]. For those unfamiliar with LME models, it is sufficient to know that we performed our analysis in a roughly identical fashion to a typical repeated-measures ANOVA, only with a type of dataset that ANOVA cannot handle (see Footnote 1). We performed all analyses using the MIXED command in SPSS with maximum likelihood estimation. Note that while LME *can* be used for much more advanced analyses, we used LME to produce a simple analysis roughly equivalent to repeated-measures ANOVA. For the initial analyzes, the fixed factors were contrast (high vs. low), context (mostly easy vs. mostly hard), stimulus location (above vs. below), and their interactions. High contrast, mostly easy, and below were coded as 1, and the other levels as 0. The mean RT for each participant in each of the unique factor combinations were used for the analysis. Participants were inserted as the single random effect.

RESPONSE TIMES

The correct RT data are presented in **Table 1**. The data revealed a significant main effect of contrast, $F_{(1,180)} = 476.422$, $p < .001$, $\eta_p^2 = 0.89$, indicating faster responses to high contrast stimuli. There was also a main effect of context, $F_{(1,180)} = 13.628$, $p < .001$,

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[1] In this experiment, half of the participants received mostly easy stimuli above fixation, and mostly hard stimuli below fixation. The other half of the participants had the reverse. Thus, both proportion easy and stimulus location were manipulated within-groups, but the *interaction* between proportion easy and stimulus location was manipulated between-groups. Standard repeated-measures ANOVA has no easy way of handling such data, whereas LME modeling does. Note that the counterbalancing factor could be used in place of location in a repeated-measures ANOVA. As we confirmed, such an analysis produces exactly the same results as the LME, but the location effects are represented by very unintuitive interactions between counterbalancing order and proportion easy.

Table 1 | Experiment response times and errors (SEs in parentheses).

	High		Low		Effect	
	RTs	**Errors**	**RTs**	**Errors**	**RTs**	**Errors**
Above						
Mostly easy	612 (17)	2.8 (0.5)	728 (22)	3.0 (0.5)	116	0.2
Mostly hard	603 (14)	5.6 (0.9)	704 (16)	5.2 (0.6)	101	−0.4
Difference					*15*	*0.6*
Below						
Mostly easy	622 (14)	5.6 (0.5)	824 (19)	5.9 (0.9)	202	0.4
Mostly hard	612 (18)	3.4 (0.6)	770 (20)	3.8 (0.6)	158	0.3
Difference					*44*	*0.0*

$\eta_p^2 = 0.19$, indicating slower overall responses in the mostly easy condition[2]. Critically and as predicted, contrast and context interacted, $F_{(1,180)} = 4.958$, $p = 0.027$, $\eta_p^2 = 0.08$, indicating a smaller contrast effect in the mostly hard condition. There was also a main effect of stimulus location, $F_{(1,180)} = 46.182$, $p < 0.001$, $\eta_p^2 = 0.44$, and an interaction between stimulus location and contrast, $F_{(1,180)} = 29.338$, $p < .001$, $\eta_p^2 = 0.34$. These were due to longer overall RTs and larger contrast effects in the bottom location, respectively. Location did not interact with context, $F_{(1,60)} = 0.112$, $p = 0.739$, $\eta_p^2 < 0.01$. Finally, the three-way interaction between location, contrast, and context was also not significant, $F_{(1,180)} = 1.192$, $p = 0.276$, $\eta_p^2 = 0.02$, showing that the context-specific temporal learning effect was roughly equivalent in both locations.

PERCENTAGE ERRORS

The error data are also presented in **Table 1**. Generally, the error data were much less sensitive. There was no significant contrast effect, $F_{(1,180)} = 0.106$, $p = 0.745$, $\eta_p^2 < 0.01$, no context effect, $F_{(1,180)} = 0.217$, $p = 0.642$, $\eta_p^2 < 0.01$, and no interaction between the two, $F_{(1,180)} = 0.139$, $p = 0.710$, $\eta_p^2 < 0.01$. The main effect of location was also not significant, $F_{(1,180)} = 2.222$, $p = 0.138$, $\eta_p^2 < 0.04$. Location and context did interact, $F_{(1,60)} = 12.723$, $p < 0.001$, $\eta_p^2 < 0.18$. Location and contrast did not interact, $F_{(1,180)} = 0.425$, $p = 0.515$, $\eta_p^2 < 0.01$. The three-way interaction between location, contrast, and context was also not significant, $F_{(1,180)} = 0.176$, $p = 0.676$, $\eta_p^2 < 0.01$. Critically, there was no evidence for a speed–accuracy trade-off.

CONTEXT REPETITIONS

Next, RT data were reassessed for a potential role of repetition versus alternation of the context from one trial to the next. To assess this possibility, we conducted another LME model on correct RTs including the variable context transition (context repetition versus context alternation). Because the previous trial context will be correlated with previous trial contrast (e.g., more high contrast trials if the previous trial was the mostly easy location), we also included the factor of previous trial contrast (high versus low). Thus, we added these two new factors to the LME model, along with their interactions with the other factors. Again, a mean for each unique combination of these factors was computed for each participants for the analysis. In this analysis, all the previously reported results were replicated. Most importantly, the contrast by context interaction remained significant, $F_{(1,898)} = 9.740$, $p = 0.002$, $\eta_p^2 = 0.14$. Interestingly, this context-specific proportion easy effect was not modulated by context transition, $F_{(1,898)} = 0.027$, $p = 0.871$, $\eta_p^2 < 0.01$, or by previous trial contrast, $F_{(1,898)} = 1.667$, $p = 0.197$, $\eta_p^2 = 0.03$. Responses were, however, faster overall if the context repeated, $F_{(1,898)} = 81.449$, $p < 0.001$, $\eta_p^2 < 0.58$, and the contrast effect was smaller on a context repetition, $F_{(1,898)} = 4.459$, $p = 0.035$, $\eta_p^2 < 0.07$. There was no main effect of previous trial contrast, $F_{(1,898)} = 0.021$, $p = 0.886$, $\eta_p^2 < 0.01$, but the contrast effect was larger if the previous trial was high contrast, $F_{(1,898)} = 6.631$, $p = .010$, $\eta_p^2 = 0.10$. Thus, context transition and previous trial contrast did have an impact on current trial contrast, but not on the critical proportion easy effect. All other effects were non-significant.

PREVIOUS RESPONSE TIMES

Next, we assessed the possible role of previous trial RTs on the size of the contrast effect. Of course, every trial had a unique previous RT associated with it. Thus, all trials were inserted into the LME. Trials with an error on the current or previous trial were excluded from analyses, however. Participants were again added as a random factor. The fixed main effect factors included the scale variable of previous RT and the binary factors contrast, context, location, previous trial contrast, and context transition. For the binary factors, low contrast, mostly hard, above location, previous low contrast, and context repetition were coded as zero, with the other level of each of these factors codes as one. Previous RT and the RT dependent measure were inverse transformed (–1000/RT, similar to Kinoshita et al., 2011; Schmidt, 2013b) to correct violations of normality,[3] and were centered on the mean to avoid correlation

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[2] An anonymous reviewer pointed out that the main effect of proportion easy might seem problematic for the temporal learning account. Specifically, this main effect seems to indicate that RTs were slower in the *mostly easy* context than in the mostly hard context. This might seem inconsistent with the temporal learning account, which requires the reverse pattern (i.e., *faster* responses in the mostly easy context). However, note that this main effect is computed by averaging the mean RT for high contrast with the mean RT for low contrast in each context. Thus, such an analysis ignores the relative *frequency* of high and low contrast items in each context. Because there are much more high contrast items in the mostly easy context than the mostly hard context, the average overall RT for the mostly easy location is actually faster. That is, there are many high contrast trials and few low contrast trials in the mostly easy context, meaning that most of the responses in the mostly easy context are fast. In the mostly hard context, it is the reverse, with a large number of (slow) low contrast items. Indeed, if one simply computes the average RT for each context (i.e., ignoring the distinction between high and low contrast items), overall RTs were significantly faster in the mostly easy context (659 ms) than the mostly hard context (693 ms), $F_{(1,16323)} = 87.283$, $p < .001$, $\eta_p^2 = 0.75$. This 34 ms difference therefore shows that the temporal regularity did indeed exist for learning context-specific regularities.

[3] Note that with regard to the statistics that follow the –1000/RT inverse transform is equivalent to a 1/RT inverse transform. The only differences are that the negative sign preserves the more intuitive direction of effects, and the 1000 multiplier reduces the numbers of decimal places in the parameter estimates.

with the intercept. Investigation of the Q–Q plots revealed no need for trimming the tails of the distribution.

Three models were tested. Model A was the fully factorial model, including all interactions between the six factors. This was obviously a very complex model with far too many terms. We therefore tested two simpler models. Model B was the same as Model A with the exclusion of many non-significant and seemingly irrelevant interactions involving previous RT. Only the main effect of previous RT and its two- and three-way interactions with contrast and context transition were retained. Model C was simpler still. In neither Model A nor Model B was any main effect or interaction involving previous contrast significant. Model C was thus identical to Model B with the exclusion of previous contrast.

To select the best of the three models we assessed the Akaike information criterion (AIC) and the Bayesian information criterion (BIC) scores. For those unfamiliar with such information criteria, AIC and BIC are two different ways to assess the amount of variance explained by a set of factors and can be used to assess whether a more complex model does or does not add anything meaningful to a simpler model. For both AIC and BIC, lower scores indicate a better model. Model A produced the worst (highest) scores of the three models (AIC: 18467; BIC: 18975), and was therefore excluded. Model B produced a slightly better (lower) AIC score than Model C (Model B: 18446; Model C: 18452), but a notably worse BIC score (Model B: 18739; Model C: 18621). The difference between these two measures is not so surprising given the harsher penalty BIC gives to added factors. Whether to favor AIC or BIC scores is a matter of heated contention, but we note that none of the factors that Model C excludes were significant in Model B and the results of the key comparisons were qualitatively the same in both models (i.e., same significant and non-significant effects). We therefore decided to present the simplest model (i.e., Model C).

Table 2 presents the parameters and statistical tests for Model C. Note that the RT dependent measure was inversed transformed, so the parameter estimates are difficult to relate back to mean RT. However, in the following we will explain what each of these tests show. We first consider the tests *excluding* previous RT (non-shaded cells in **Table 2**). There was a main effect of contrast, indicating overall slower responses to low contrast items. There was also a main effect of context, indicating overall faster responses to mostly hard items. There was no effect of location. There was a main effect of context transition, indicating faster responses for repeated locations. As before, contrast and context interacted, indicating a proportion easy effect. There was also an interaction between location and context, indicating overall slower responses to mostly easy items in the below location. Contrast, context, and location interacted, indicating a larger proportion easy effect in the below location. No other interactions were significant.

We now consider the effects involving previous RT (shaded cells in **Table 2**). There was a main effect of previous RT, indicating that previous RT and current trial RT were correlated. Critically, we also observed that previous RT and contrast interacted. This indicates that contrast effects were larger the faster the previous RT. Previous RT also interacted with context transition, indicating a higher correlation between previous RT and current trial RT when the context repeated. Particularly interesting, previous RT, contrast, and context transition interacted. This indicates that the effect of previous RT on the contrast effect was larger when the context alternated. Overall, then, the results fit perfectly with the predictions of the temporal learning account.

Table 2 | Mixed modeling results.

Variable	**Estimate**	**SE**	***t***	***p***
Intercept	−0.071283	0.034188	−2.085	0.041
Contrast	0.415671	0.020591	20.187	<0.001
Context	−0.061880	0.020618	−3.001	0.003
Location	−0.027907	0.048361	−0.577	0.566
Context transition (CT)	−0.109343	0.016075	−6.802	<0.001
Contrast*Context	−0.155539	0.029148	−5.336	<0.001
Contrast*Location	−0.162065	0.028590	−5.669	<0.001
Contrast*CT	−0.020604	0.029425	−0.700	0.484
Context*Location	0.065353	0.029129	2.244	0.025
Context*CT	0.045058	0.029501	1.527	0.127
Location*CT	0.005037	0.022659	0.222	0.824
Contrast*Context* Location	0.248084	0.040793	6.081	<0.001
Contrast*Context*CT	−0.037323	0.041547	−0.898	0.369
Contrast*Location*CT	0.021650	0.040916	−0.529	0.597
Context*Location*CT	−0.065708	0.041111	−1.598	0.110
Contrast*Context* Location*CT	−0.003608	0.057877	−0.062	0.950
Previous RT	0.070863	0.013922	5.090	<0.001
Contrast*Previous RT	−0.104218	0.019003	−5.484	<0.001
CT*Previous RT	0.091393	0.019255	4.747	<0.001
Contrast*CT*Previous RT	0.063083	0.027095	2.328	0.020

DISCUSSION

The current paper makes two novel contributions to the literature. First, the results of our experiment are consistent with our suggestion that temporal learning can function in a context-specific manner. Specifically, the contrast effect was found to be larger in the mostly easy location context relative to the mostly hard context. Thus, the magnitude of an effect on the current trial is influenced not only by previous trial RTs, but also by contextual cues. Interestingly, this context-specific proportion easy effect was not modulated by the contrast of the previous trial or by whether or not the context (i.e., location) repeated. The lack of an effect for location repetitions is particularly interesting, because it shows that the appearance of the context-specific proportion easy effect is not solely driven by those trials on which the context repeats. This would seem to suggest that participants have two learned rhythms, one for each location context, that they can flexibly switch between depending on the location in which the stimulus appears. Note that previous contrast did impact the magnitude of the current trial contrast effect, but this effect was eliminated when previous RT was included as a factor. This suggests that previous trial RTs influence the size of the contrast effect, but not previous contrast (which is correlated with previous RT). This is consistent with results from analyses on list-level effects, where the same pattern of results was observed (Schmidt, 2013b).

The mixed model analyses including previous RT were particularly interesting. In line with predictions from the temporal learning account, previous RT not only correlated with current trial RT, but also affected the size of the contrast effect. As predicted, the contrast effect increased as previous RT sped up. Moreover, this effect of previous RT on the contrast effect was found to be larger when the context (i.e., location) repeated. This is consistent with the notion that participants learn a rhythm for each location, because the previous RT on a context repetition belongs to the same context as the current trial, whereas on a context alternation it belongs to a different context.

The second novel contribution of the current work is that our results hint at an alternative explanation of the CSPC effect. While previous accounts might have attributed such findings to hemisphere-specific processing (Corballis and Gratton, 2003) or context-specific adjustments of cognitive control (Crump et al., 2006; Bugg et al., 2008; Wendt et al., 2008; Crump and Milliken, 2009), the present results suggest that such effects might instead be explainable by context-specific temporal learning. Indeed, the context-specific conflict adaptation account argues that the CSPC effect is driven by attentional adjustments to the differing rates of conflict in each location. If the CSPC effect is argued to be solely explainable by such attentional adjustments, then the current observation of a context-specific proportion easy effect should not have been predicted. Given that the present contrast design used no conflicting stimuli (indeed, there were no distracters in the task), an interaction between proportion easy and contrast should not have occurred. Furthermore, it is not clear how any such attentional filtering account could explain the influences of previous RT that we observed in our data, at least not without considerable added assumptions. Of course, this does not mean that conflict adaptation does not play a role in the CSPC effect, but our results might suggest, at minimum, that the contribution of conflict adaptation to such an effect is probably overestimated due to temporal learning biases.

Unlike attentional accounts, the simple temporal learning account has no difficulties with the current findings. The effect of context on the magnitude of difficulty effects (e.g., congruency or contrast) is proposed, according to such an account, to be unrelated to conflict. Thus, context-specific effects should not be eliminated by removing conflict from the task. The temporal learning view only needs to assume that participants can learn different temporal rhythms for two contexts. If they are in a faster rhythm (i.e., have earlier temporal expectancies) in the mostly easy context (i.e., because of the large number of high contrast or congruent trials), then they will have a larger effect relative to the slow-rhythm (i.e., later temporal expectancies) mostly hard context.

Another interesting result that might be seen as consistent with the temporal learning view comes from Wendt and Kiesel (2011). They presented participants with a CSPC task in which *foreperiod* (i.e., the time between fixation and stimulus presentation) was the contextual cue. Specifically, a short foreperiod (200 ms) was associated with mostly congruent stimuli and a long foreperiod (1200 ms) was associated with mostly incongruent stimuli, or vice versa. Congruency effects were larger with the mostly congruent foreperiod. This is an interesting finding, as some form of timing process is necessary to explain such results. While it can certainly be argued that attentional filtering might be modulated over time with contextual cues (though this would require some changes in thinking about conflict adaptation effects), such results could alternatively be argued to be due to participants learning when to respond based on stimulus onset. The idea that participants can learn about differing temporal intervals is already an inherent part of the temporal learning account, meaning that such results fit quite nicely with the temporal learning view without any necessary adjustments to the account. As pointed out by Wendt and Kiesel, this is not the case for extant models of conflict monitoring (e.g., Blais et al., 2007; Verguts and Notebaert, 2008), which would need some retuning to allow for such time-based effects. Indeed, one of such changes would have to be a mechanism to learn about timing, further indicating that temporal learning of one form or another is a highly plausible mechanism for producing CSPC effects.

As discussed in the Introduction, the temporal learning account could potentially also explain the transfer effects observed by Crump and Milliken (2009; see also, Heinemann et al., 2009; Reuss et al., 2014) for contingency-unbiased items. Indeed, the items in the current report were entirely contingency-unbiased. That is, there were no distracting stimuli that could provide a predictive cue for the likely response. Similarly, the location context was completely un-predictive of what response would follow. Of course, some letter–contrast–location combinations were more frequent than others, but the previously discussed results of Schmidt (2014) demonstrate that proportion easy effects with contrast are not (at least primarily) item-specific. Future work might aim at testing context and transfer items in a context-specific proportion easy task to add further credence to this notion.

The present research is not without limitations, however. Note that the present results do not rule out the possibility that conflict adaptation also contributes to the CSPC effect. The conflict adaptation account does not specifically predict that a context-specific proportion easy effect should *not* occur. It merely does not predict such an effect. It could be that both conflict adaptation and temporal learning play a role in the CSPC effect. Of course, this is a less parsimonious account than suggesting that both the context-specific *PC* and context-specific *proportion easy* effects are explainable by the same (e.g., temporal learning) mechanism. Moreover, even if context-specific conflict adaptation *does* occur, the present results would suggest that the CSPC effect is likely to be confounded with temporal learning biases. Thus, the best possible outcome that remains for the conflict adaptation view is probably the conclusion that the CSPC effect overestimates the contribution of conflict adaptation processes. Future research might aim to attempt to dissociate the separate influences of temporal learning and conflict adaptation on the CSPC effect. As already pointed out, this is unfortunately a dissociation that will be difficult if not impossible to produce, given how inherently confounded speed-of-responding and congruency are. It is certainly our hope, however, that the current work might serve to inspire other researchers to find a solution to this predicament.

As another caveat, there were a few differences between the context-specific proportion easy data observed here and data from CSPC experiments. For instance, a main effect of proportion easy was observed, such that responses were overall slower in the mostly easy condition. More specifically, this main effect seemed to be the result of the context-specific proportion easy effect being driven exclusively by hard items. However, in CSPC experiments it is uncertain whether such a main effect is observed. In King et al. (2012a; see also King et al., 2012b) the CSPC effect was driven by seemingly symmetric effects on congruent and incongruent trials. On the other hand, the exact same pattern of interference-driven effects that we observed *was* observed in CSPC error rates by these authors. Crump et al. (2006) do not report tests for main effects, but their Experiment 1 RTs and Experiment 2a error rates appear numerically consistent with our findings. Their Experiment 2a RT data suggest the reverse pattern, however, with the CSPC seemingly driven by congruent items. As pointed out by Schmidt (2014), proportion congruency manipulations of all types, much like proportion easy effects observed in our lab, seem to provide quite inconsistent patterns regarding whether the effect is located in the congruent trials, incongruent trials, or both. It is not clear how any account explains these inconsistencies, and we would suggest that future work might aim to explain not only PC interactions, but also the precise pattern of means.

One possible explanation is the presence of floor or ceiling effects. In some experiments, responses to easy (e.g., congruent) items might be fast enough that no further benefit can be gained from temporal expectancies (or some other mechanism, such as conflict adaptation). In other experiments, responses to hard (e.g., incongruent) items might be slow enough that participants are responding at a maximum slow rate, temporal expectancies or not. For instance, responding might be thresholded for such hard items. Error rates might be informative in such a case. This floor/ceiling argument, however, is admittedly post hoc and would need corroboration from actual data. Another possibility might be overall differences in response caution for differing contexts, though this would seem at first glance to predict the reverse main effect of context observed in the current report. On the other hand, an early temporal learning account by Grice (1968) suggested that in an overall harder context, the threshold for responding might be reduced to expedite processing of difficult items. This notion *is* consistent with the finding of slower responses for both the easy and hard item types in the mostly easy condition. Whatever the explanation, note that the observed main effect does not present an inherent problem for the temporal learning account, as the overall mean RT of responses in mostly easy context was faster than in the mostly hard context.^{2}

As another limitation, it is noteworthy that we did not find a modulation of the context-specific proportion easy effect as a function of whether or not the context repeated from one trial to the next. Such modulations have been observed in CSPC experiments (King et al., 2012a,b). Though not impossible that we simply lacked the statistical power to detect such a modulation, no clear evidence for one was observed. This inconsistency does leave open the possibility that the context-specific proportion easy effect is not driven by the same mechanism as the CSPC effect, which would undermine the temporal learning account of CSPC effects suggested in the current manuscript. On the other hand, we provided an important control on the context transition analysis by including previous contrast in the analysis. This is important, because previous trial contrast is highly confounded with context transition. Unfortunately, the same control was not used in previous research investigating the role of context transition in CSPC experiments. Specifically, previous trial congruency was not coded along with context transition. Thus, the observation of larger CSPC effects following context repetitions relative to context alternations could have been due to a previous trial congruency confound. Indeed, the direction of observed effects is consistent with such a confound. Furthermore, such interactions between previous and current trial congruency are well documented (Gratton et al., 1992) and may be, in full or in part, driven by feature repetition or other learning biases (Mayr et al., 2003; Hommel et al., 2004; Schmidt and De Houwer, 2011; Mordkoff, 2012). Thus, there is strong reason to suspect a confound in the analyses of King and colleagues. As such, future research or reanalysis of existing data to answer this question is well warranted.

As another limitation, while our experiment was designed to test the *a priori* hypothesis that a context-specific proportion easy effect could be observed in the absence of a conflict manipulation due to a temporal learning process, it is alternatively possible that yet another account explains either the context-specific proportion easy effect or *both* the context-specific proportion easy and CSPC effects. One account, intimately related to the temporal learning account presented here, is the response caution account. According the response caution account, context-specific effects could be driven by increases in the response threshold when experiencing unexpected stimulus combinations for a context. For instance, a low contrast item is not expected in the mostly easy context, and this might lead to an increase in the response threshold, thus delaying responding. A similar process would occur for high contrast items in the mostly hard context.

Indeed, evidence for a response threshold account of CSPC effects has been presented by King et al. (2012a). They used a quantitative model to test how well a threshold account fit the data relative to an evidence accrual model (the latter of which is consistent with a conflict adaptation process). The response threshold model was found to provide a much better fit than the evidence accrual model. The authors therefore argued that the CSPC effect is better explained by response caution than conflict adaptation. It should be noted that the response caution predictions match those of the temporal learning perspective, because both accounts predict a relatively lower threshold for expected stimuli (congruent mostly congruent and incongruent mostly incongruent) relative to unexpected stimuli (congruent mostly incongruent and incongruent mostly congruent). Similar time-based accounts, such as the adaptation to the statistics of the environment (ASE) model (e.g., Kinoshita et al., 2011), are also consistent with such results. We therefore think that determining which variant of these response threshold models provides the best fit to proportion easy and PC effects is an important goal for future research to address.

Though inconsistent with the above-mentioned finding that CSPC effects are more in line with a response threshold rather than evidence accrual mechanism, it could nevertheless be proposed that some other form of attentional filtering mechanism explains the context-specific proportion easy effect observed in the current manuscript. While it seems that conflict adaptation can be safely ruled out for the context-specific proportion easy effects observed with the present design, perhaps it could be argued that the cognitive system learns to better extract target information of the most frequent contrast level in each context. That is, high contrast stimuli might be better processed in the mostly easy location, and low contrast stimuli in the mostly hard location via some form of attentional capture (e.g., see Cosman and Vecera, 2014; Thomson et al., 2014) of the most likely contrast level. It is not entirely clear how such an attentional mechanism would work, however. For instance, it seems unlikely that information accrual of low contrast items in the mostly hard context would be improved without similarly improving high contrast items in the same context. The plausibility of this account is further weakened by the fact that the modeling results of King et al. (2012a) seem to argue against an evidence accrual account of context-specific effects. Further still, it is not clear how such an account would explain the effects of previous RT we observed. Still, it should be acknowledged that the manipulation of “easy” and “hard” did involve using stimuli of differing luminance. This is not optimal for comparison with CSPC experiments, where luminance is equated across easy (congruent) and hard (incongruent) items. Future experiments with different manipulations of stimulus ease would thus be a welcome addition.

It should also be noted that the temporal learning view does share some similarities with the conflict adaptation view. In both accounts, it is assumed that contextual information is used to adjust performance. In the conflict adaptation view, attention is adjusted to minimize conflict. In the temporal learning view, expectancies are adjusted to maximize the speed of responding. Both accounts therefore propose an adjustment of performance in order to benefit the task goal, but merely differ in *what* is adapted to (i.e., conflict versus temporal information) and *how* performance is adjusted (i.e., attentional modulations versus temporal expectancies). These are not trivial differences, of course, and we hope that future research will further the investigation of these issues. The current results only provide a proof-of-principle that temporal learning might provide a sufficient explanation of CSPC effects. Further work will be required to draw more definitive conclusions.

At the broader level, the current results suggest that temporal learning occurs in a context-specific fashion and that switching between contexts can occur on a relatively quick, trial-by-trial basis. This is an interesting finding in its own respect that might be investigated further in future research. Rapid context-specificity in temporal learning also need not be viewed as unintuitive. While many rhythmic behaviors may entail producing an action in equally spaced intervals, this is not always the case. For instance, not all notes in a song will be quarter notes. Some notes will come sooner or later, often with deliberate syncopation. Thus, even with the most obvious example of rhythmic behavior (i.e., music), context (in this case, the notes that came before the current one) plays an important role in modifying behavior.

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ISPC effect is not observed when the word comes too late: a time course analysis

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The item-specific proportion congruency (ISPC) effect is demonstrated by a smaller Stroop effect observed for mostly incongruent items compared to mostly congruent items. Currently, there is a continuing debate on whether conflict driven item-specific control processes or stimulus-response contingency learning account for the ISPC effect. In the present study, we conducted two experiments to investigate the time course of the ISPC effect with a stimulus onset asynchrony (SOA) manipulation. Both negative and positive SOAs were used in order to manipulate the contingency learning between the word and the color dimensions. We also combined this SOA manipulation with a set size manipulation (Bugg and Hutchison, 2013) to moderate the contribution of contingency learning and item-specific processes to the observed ISPC effect. We expected that the change in the magnitude of the ISPC effect as a result of SOA would follow different patterns for the 2-item and 4-item set conditions. Results showed that the SOA manipulation influenced the ISPC effect. Specifically, when the word followed the color with a 200 ms delay, the observed ISPC effect was smaller, if at all present, than the ISPC effects in other negative and positive SOA conditions, regardless of set size. In conclusion, our results showed that the ISPC effect was not observed if the word arrived too late. We also conducted additional awareness and RT distribution analyses (delta plots) to further investigate the ISPC effect. These analyses showed that a higher percentage of participants were aware of the ISPC manipulation in the 2-item set condition compared to the 4-item set condition. Delta plots revealed that the ISPC effect was smaller for fastest responses and increased as the responses got slower.

Keywords: ISPC effect, conflict monitoring, contingency learning, stimulus onset asynchrony, Stroop task, cognitive control

INTRODUCTION

Cognitive control is the ability to meet task demands despite distractors and maintain stable performance in the face of changing contexts (Matsumoto and Tanaka, 2004). One of the most commonly used tasks to investigate cognitive control is the Stroop task (Stroop, 1935). In a Stroop task, color words are presented in matching colors (congruent trials) or mismatching colors (incongruent trials) and participants are instructed to name the ink color and ignore the written word. Despite clear instructions, automatic word reading processes interfere with the color naming task, which results in shorter reaction times observed for congruent trials than for incongruent trials. The reaction time difference between the incongruent and the congruent trials is called the Stroop effect. Variations in the magnitude of the Stroop effect have been interpreted as an indication of control over automatic word reading processes. The nature of these control processes have been investigated by observing certain variables and contexts that modulate the Stroop effect.

A widely used example is the list-wide proportion congruency manipulation, in which the magnitude of the Stroop effect is modulated by the proportion of congruent and incongruent trials in a block. Specifically, a larger Stroop effect is observed when the proportion of congruent trials is higher, compared to the condition when the proportion of incongruent trials is higher (Logan and Zbrodoff, 1979; Logan et al., 1984; Tzelgov et al., 1992). These findings were initially attributed to the strategic use of control processes. For instance, the conflict monitoring account presented a mechanistic explanation of how control operations were executed in response to list-wide proportion congruency manipulations (Botvinick et al., 2001; Verguts and Notebaert, 2008). According to this account, an increase in the proportion of incongruent trials in a block resulted in higher levels of conflict, which in turn, increased control over the Stroop effect.

The notion that Stroop effect was controlled by list-wide strategies was challenged by the introduction of the item-specific proportion congruency (ISPC) manipulation by Jacoby et al. (2003). In the ISPC manipulation there were an equal number of congruent and incongruent trials in each block, and the proportion congruency was manipulated at the item level. That is, Jacoby et al. (2003) used two sets of color words (i.e., green and white vs. blue and yellow). The first set of color words were presented mostly in their congruent color (the mostly congruent

[MC] condition); the second set of words were presented mostly in their incongruent color (the mostly incongruent [MI] condition). Any observed changes in the magnitude of the Stroop effect in such an experimental design can no longer be attributed to list-wide control processes, since equal number of congruent and incongruent trials in the experiment prevents the participants from predicting the congruency of incoming trials. Their result showed a smaller Stroop effect for the MI items compared to the MC items, which they referred to as the item specific proportion congruency (ISPC) effect.

Jacoby et al. (2003) proposed an item-level reactive control mechanism to explain the ISPC effect. The proposed mechanism consists of a word reading filter which is rapidly triggered by the stimulus feature that predicts proportion congruence. The filter controls the effects of word reading on color naming performance by decreasing the activation for the irrelevant word dimension (Jacoby et al., 1999). This explanation has challenged the classical dichotomy between automatic and controlled processes (Posner and Synder, 1975), by introducing the possibility of *automatic control*. Owing to the demonstration of the ISPC effect, the relative contributions of proactive and reactive control to list level proportion congruency effects have been extensively investigated (Hutchison, 2011; Bugg et al., 2011a; Bugg and Crump, 2012; Abrahamse et al., 2013). It also led to a continuing debate on whether conflict driven cognitive control processes or stimulus-response learning explained the observed ISPC effect.

According to the *conflict monitoring account*, item-specific control is exerted by registering item-specific conflicts (Blais et al., 2007). The conflict monitoring system calculates and keeps records of conflicts for individual items, and modifies the Stroop effect for each item differentially. Conflict-driven control processes are rapidly set following the onset of each stimulus depending on the proportion congruence of items. The assumption that the irrelevant dimension (the word) determines the conflict monitoring and control processes is difficult to accommodate, since it can determine these only after that specific word is read. (Schmidt and Besner, 2008; see also Verguts and Notebaert, 2008; Levin and Tzelgov, 2014).

Schmidt and Besner (2008) challenged the automatic control explanations by showing that proportion congruency in the ISPC design was fully confounded with stimulus-response contingency. They demonstrated that two independent processes, namely, Stroop interference and contingency learning, accounted for the ISPC effect (Schmidt, 2013a,b). One line of support for this claim comes from within- and between language ISPC studies (Atalay and Misirlisoy, 2012; Atalay et al., 2013). Contingency learning effects observed with non-color words under within- and between-language manipulations were parallel to those observed with color words under within- and between-language ISPC manipulations.

Subsequently, Bugg et al., advanced the cognitive control hypothesis by introducing certain boundary conditions for the involvement of control processes in the ISPC effect (Bugg et al., 2011b; Bugg and Hutchison, 2013, Experiments 1-2). They showed that both contingency learning and control processes played a role in the ISPC effect, and that their level of involvement was determined by the degree of efficiency of access to memory representations by the relevant (color) and irrelevant (word) dimensions of the Stroop stimuli. They demonstrated that item-specific control processes were involved when the relevant dimension signaled proportion congruency and/or when the relevant dimension's access to memory representations was strengthened by the experimental manipulation. Otherwise, contingency learning processes came into play; since word-response contingencies were used more readily by participants.

Bugg and Hutchison (2013, Experiment 3) introduced set size as another factor that moderated the contribution of item-specific control to the ISPC effect. They argued that in the classic 2-item set design, a single high-contingency response existed for both MC and MI item sets (see Schmidt and Besner, 2008), which made it possible and advantageous for the participants to rely on a contingency learning mechanism. In order to test this, they introduced a novel 4-item set design. Eight color words were divided into two 4-item sets. In the MC condition, words were presented in their congruent color 80% of the trials. For the remaining 20% of the trials, they were presented in each of the three incongruent colors equally. In the MI condition, words were presented in their congruent color 20% of the trials; for the remaining 80% of the trials, they were presented in each of the three incongruent colors equally. Therefore, a single high-contingency response did not exist for the MI set. In this case, participants were not able to predict the most likely response with high accuracy, in the incongruent trials. This, in turn, promoted the use of item-specific control instead of contingency learning mechanisms.

Bugg and Hutchison (2013) provided two important pieces of evidence supporting their claim. First, they showed that the pattern of the ISPC effects observed for the 2-item and 4-item sets were different. More specifically, in line with the predictions of the contingency account, proportion-congruence effects observed in the congruent and incongruent trials were similar when 2-item sets were used. For the 4-item sets, however, the proportion-congruence effect observed for the incongruent trials were larger than that of the congruent trials. This result would be predicted by the item-specific control account, but not by the contingency account (see Schmidt, 2014 for an alternative view).

The second piece of evidence was obtained by utilizing transfer items, which were introduced in the final block of the experiment. The transfer items were 50% congruent and 50% incongruent. Incongruent transfer items were obtained by choosing MC and MI words equally from the previous (training) blocks and presenting these words with the transfer colors. For the 2-item set condition, RTs for the MC-incongruent and MI-incongruent transfer items were comparable. However, for the 4-item set condition, RTs for the MI-incongruent transfer items were shorter than RTs for the MC-incongruent transfer items. In summary, an ISPC effect was observed with the transfer items in the 4-item set condition, but not in the 2-item set condition. These results showed that item-specific (reactive) control contributed to the ISPC effect even when the word acted as the ISPC signal.

In the present study, our aim was to investigate the time course of the ISPC effect, by using a separated version of the Stroop task, in which stimulus onset asynchrony (SOA) between the word and the color is manipulated (Glaser and Glaser, 1982; Sugg and McDonald, 1994; Appelbaum et al., 2009, 2012; Roelofs, 2010a,b). A well-replicated result was that larger Stroop effects were observed when the color and the word are presented closer in time, compared to when they are more distant. These findings served an important function in testing the models of the Stroop effect (Cohen et al., 1990). In a similar vein, information on the time course of the ISPC effect is expected to help dissociate contingency learning and item-specific control processes underlying the ISPC effect.

In the present study, we combined the set size manipulation, introduced by Bugg and Hutchison (2013), with an SOA manipulation. The word (the irrelevant dimension) was presented before (−200 ms, −100 ms), simultaneously with (0 ms) or after (+100 ms, +200 ms) the color patch (the relevant dimension, see **Figure 1**), for both 2-item and 4-item set conditions. We predicted that contingency learning and control processes would be differently affected by the SOA manipulation. In other words, we expected the change in the magnitude of the ISPC effect as a result of the SOA manipulation to follow a different pattern for the 2-item and 4-item set conditions. This resulted in several predictions regarding the level of contribution of control processes and contingency learning to the ISPC effect.

The simultaneous presentation condition, in which the color and the word are presented at the same time, is very similar to the classical color-word Stroop task. Therefore, for the simultaneous presentation condition, in keeping with Bugg and Hutchison (2013), we expected to observe different patterns for the ISPC effects observed in the 2-item and 4-item set conditions. That is, we expected to observe equal proportion congruency effects for the congruent and incongruent trials in the 2-item set condition; which indicate contingency learning processes. In the 4-item set condition, however, the proportion congruency observed for the incongruent trials was expected to be larger than that of the congruent trials; which would indicate control processes.

In the negative SOA conditions, the word was presented before the color. For the 2-item set condition, seeing the word before the color is expected to give participants the opportunity to predict the response even before seeing the color, which would make reliance on contingency learning mechanisms more advantageous. For the negative SOA trials in the 4-item set condition, however, one can predict two different results. On the one hand, the absence of a single high contingency response for incongruent trials in the 4-item set design, together with the presentation of the word before the color, may increase the probability of the word acting as an ISPC signal. That is, item specific control operations may be triggered after seeing the word. If this is the case, then the pattern of results observed in the 4-item set design is expected to be different from that observed in the 2-item set design, which indicate control processes. On the other hand, seeing the word before the color could facilitate word reading, and in turn, make it harder to control the effects of word reading on color naming. In this case, the ISPC effect would be smaller, if at all present.

In the positive SOA conditions, the color was presented before the word. In these conditions, while the structure of the 2-item set design allows the participants to rely on a contingency learning mechanism, seeing the color patch before the word reduces the prediction power of the word (cf. Schmidt and De Houwer, 2012, Experiment 3), making a contingency learning strategy less advantageous. Accordingly, in the 2-item set design for positive SOA conditions, the ISPC effect is expected to be smaller, if at all present. For the 4-item set design positive SOA conditions, since the color is presented before the word, participants might initiate

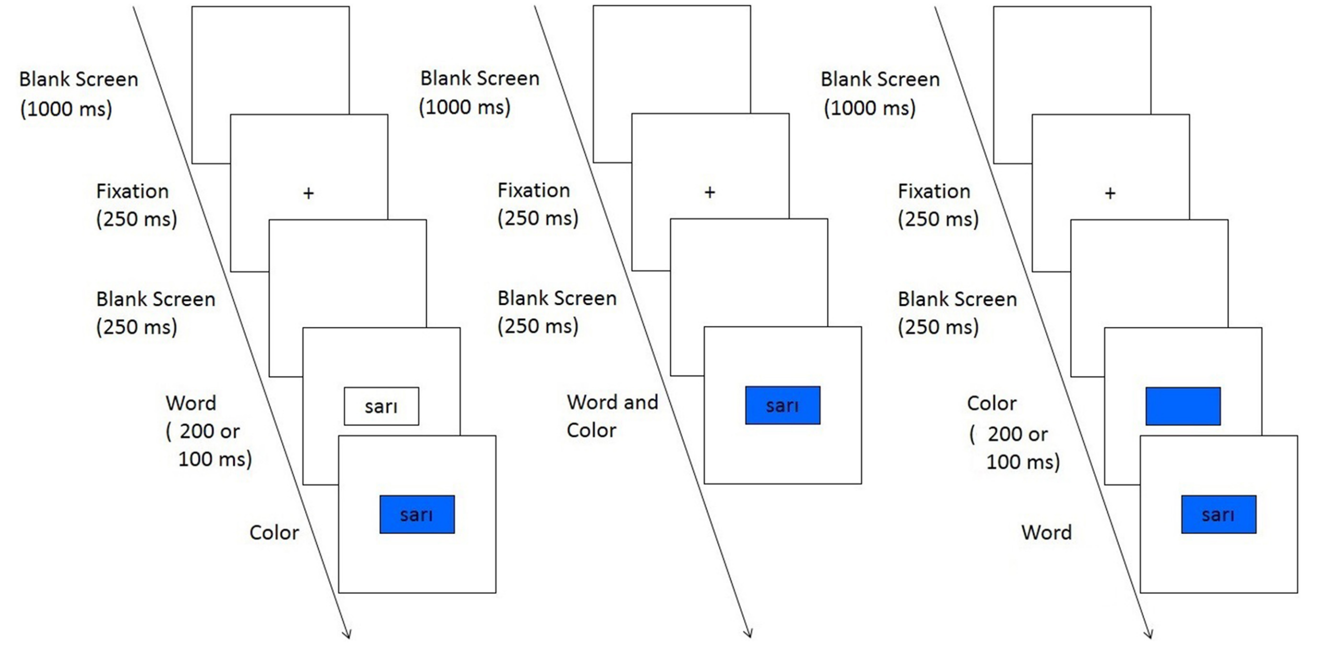

FIGURE 1 | Trial sequence of Experiments 1 and 2. Participants named the color of the rectangle. The word appeared before (left), at the same time (middle) or after the colored rectangle (right). The figure is not drawn in scale. Durations are presented in parentheses.

the response even before the word has a chance to trigger control processes, which would eliminate the ISPC effect. In this case, the ISPC effect is expected to be smaller, if at all present. On the other hand, presenting the color before the word could modulate the imbalance between color naming and word reading processes regarding access to memory, in favor of color naming. Therefore, the color would act as an ISPC signal. Consequently, control processes would dominate in the 4-item set positive SOA conditions, and an ISPC effect would be observed.

We conducted two ISPC experiments to investigate the above predictions. In the first experiment, set size was manipulated as a between-subjects factor and SOA was manipulated as a within-subject factor. There were five SOA blocks, within which SOA was kept constant. In keeping with our predictions regarding set size and SOA, we expected the negative and positive SOA conditions to favor control or contingency learning processes depending on set size. Considering the possibility that participants might switch between control and contingency learning processes across different SOA blocks in Experiment 1, which could possibly obscure the results, a second experiment in which SOA was manipulated as a between-subjects factor was necessary. Accordingly, in Experiment 2, both set size and SOA were manipulated as between-subjects factors. This also increased the number of stimuli for each SOA condition, improving the validity of observations.

MATERIALS AND METHODS

PARTICIPANTS

There were 126 participants in Experiment 1 (mean age = 21.22, 89 females) and 127 participants in Experiment 2 (mean age = 20.96, 105 females). Participants were university students who volunteered for course credit, or monetary compensation (10 TL∼$5). For Experiment 2, sample size was determined with the G*Power 3 software (Faul et al., 2007) by using the effect size measure (eta squared) reported in Bugg and Hutchison (2013). We used the same sample size for the within subjects experiment, since we were interested in the four-way interaction between set size, SOA, proportion congruency and item type.

Participants who were not native speakers of Turkish, who reported colorblindness, a reading or attentional disability, or who did not follow the experimental protocol were excluded from the analyses. In addition, if the microphone was not triggered for more than 10% of the trials, the data for that participant was excluded. Analyses were conducted with 106 participants for Experiment 1, and with 113 participants for Experiment 2.

STIMULI AND DESIGN

The stimuli and procedure were approved by the local human research ethics committee. Stimuli consisted of a color patch (7.1 × 3.3 cm) and a color word in the middle. Eight colors (blue, gray, green, orange, pink, purple, red, yellow) and their corresponding Turkish color words (mavi, gri, yeşil, turuncu, pembe, mor, kırmızı, sarı) were used in both experiments. For each participant, the 2-item or 4-item sets were selected randomly, and each set was randomly assigned to the MC or MI conditions. MC words were presented with a congruent color patch 83% of the trials and with an incongruent color patch for the remaining 17% of the trials. MI words were presented 83% of the trials with an incongruent color patch, and 17% of the trials with a congruent color patch. There were five blocks of 144 trials (720 trials in total). All eight words were presented 18 times in a block (90 times in total). **Table 1** presents a sample of the stimuli arrangement in

Table 1 | A sample arrangement and frequency of stimuli in a single block in the 2-item and 4-item set conditions.

		Word	Color							
			blue	gray	green	orange	pink	purple	red	yellow
2-item Set	MC	blue	15	3						
		gray	3	15						
		green			15	3				
		orange			3	15				
	MI	pink					3	15		
		purple					15	3		
		red							3	15
		yellow							15	3
4-item Set	MC	blue	15	1	1	1				
		gray	1	15	1	1				
		green	1	1	15	1				
		orange	1	1	1	15				
	MI	pink					3	5	5	5
		purple					5	3	5	5
		red					5	5	3	5
		yellow					5	5	5	3

MC, mostly congruent; MI, mostly incongruent. Assignment of the stimuli to the MC and MI conditions were random for each participant.

a single block, for both 2-item and 4-item set conditions. In the 2-item set condition, a single color was used to present items in their incongruent form. In the 4-item set condition, however, three different colors were used to present items in their incongruent form.

A 2 (set size: 2-item vs. 4-item) × 5 (SOA: −200 ms, −100 ms, 0 ms, +100 ms, +200 ms) × 2 (proportion congruency: MC vs. MI) × 2 (item type: congruent vs. incongruent) mixed-design was used in both experiments. In Experiment 1, proportion congruency, item type, and SOA were manipulated within participants; set size was manipulated between participants. SOA was manipulated across blocks. The order of the blocks was counterbalanced across participants with a Latin square design. Transitions between blocks were not obvious to the participants. SOA between the relevant and the irrelevant dimensions were −200, −100, 0, +100 or +200 ms (see **Figure 1**). The minus sign denotes presentation of the word before the color. Experiment 2 was identical to Experiment 1, except for the SOA manipulation. In Experiment 2, SOA was kept constant across blocks and it was used as a between-subjects factor.

PROCEDURE

The procedure was the same for Experiment 1 and Experiment 2. Participants completed the experiment individually in a quiet room within approximately 45 min. Half of the participants were assigned to the 2-item set and the other half to the 4-item set ISPC condition. Before the experiment, participants signed the informed consent form, and filled a questionnaire on color blindness, reading and attentional disability and proficiency in Turkish. Automatic stimulus display and data collection were controlled with a PC running E-Prime 2.0 software. Participants were seated at approximately 60 cm from the monitor. They were given verbal and written instructions to name out loud the ink color of the stimulus, as quickly and as accurately as possible, while ignoring the written word.

Trial sequence as a function of SOA is presented in **Figure 1**. Trials started with a blank screen (1000 ms), followed by a 250-ms fixation display, and another blank screen (250 ms). As depicted in **Figure 1**, a color patch was presented before (−200 ms and −100 ms conditions), simultaneously with (0 ms condition), or after (+200 ms and +100 ms conditions) a color word. The color was visible for 1500 ms after its onset. A microphone connected to a Serial Response Box detected the voice onset times. The stimulus appeared on the screen until the voice key was tripped, or until the 1500 ms response deadline was reached. Feedback was given when the voice key was not tripped until the response deadline. Responses were recorded with a second microphone. Before the experiment, participants completed 30 training trials.

Immediately after the experiment, participants answered two questions assessing their awareness of the ISPC manipulation. First question (the awareness question) asked whether or not specific words and colors were paired more frequently than others in the experiment. Participants were required to explain their answer if they answered "yes." They also gave a confidence judgment for their response by either selecting "certain" or "guessed." For the next question (the matching question), they were given 10 colors and 10 color words presented as two columns and were asked to connect the more frequently paired colors and color words by drawing a line in-between. Eight of the 10 colors and color words were used in the experiment, the remaining two were new. They were encouraged to guess if they were not certain.

After the participants left the laboratory, the experimenter listened to and coded each trial as correct, incorrect or scratch. Trials were coded as scratch if the voice key was not tripped at all or was tripped by noise.

RESULTS

EXPERIMENT 1

Prior to the analyses, scratch trials and trials with RTs 3 *SD*s above (or below) the mean were removed. Trials sharing any variety of stimulus- and response-features with the previous trial were also removed to exclude effects sequential repetition or alternation of color and/or word dimensions (Mayr et al., 2003; Hommel et al., 2004). Analyses were run with the remaining 74.7% of the trials. Correct RT and proportion of error (PE) data were analyzed with separate 2X5X2X2 mixed-design ANOVAs, in which set size (2-item vs. 4-item set) was a between-subjects factor; SOA (−200, −100, 0, +100 or +200), proportion congruency (MC vs. MI), and item type (congruent vs. incongruent) were within-subject factors. The alpha level was set at 0.05 for all analyses and partial eta squared (η_p^2) is reported as the measure of effect size. *F*s are reported with a Greenhouse-Geisser correction. **Tables 2**, **3** present mean RTs and PEs for the conditions of Experiment 1, respectively. Only the results from the RT analyses are reported, since PEs were low ($M = 2.3\%$) and the results of the PE analyses were parallel with that of the RT analyses. Average number (and standard deviation) of correct RTs per cell in Experiment 1 is presented in **Table 4**.

The between-subjects main effect of set size was significant, $F_{(1,\ 104)} = 16.56$, $MSE = 82,862$, $p < 0.001$, $\eta_p^2 = 0.14$. Overall, responses were slower for the 2-item sets (692 ms) compared to the 4-item sets (641 ms). There was a significant main effect of SOA, $F_{(3.48,\ 362.07)} = 69.14$, $MSE = 6436$, $p < 0.001$, $\eta_p^2 = 0.40$. These main effects were also qualified by a significant set size × SOA interaction, $F_{(3.48,\ 362.07)} = 4.61$, $MSE = 6436$, $p = 0.002$, $\eta_p^2 = 0.04$. RTs linearly increased as SOAs become more positive, but there was a decrease in RTs in the +200 condition. This decrease was more pronounced for the 4-item set compared to the 2-item set condition. The Stroop effect (the main effect of item type) was significant, $F_{(1,\ 104)} = 733.25$, $MSE = 5653$, $p < 0.001$, $\eta_p^2 = 0.88$, and it significantly interacted with set size, $F_{(1,\ 104)} = 9.55$, $MSE = 5653$, $p = 0.003$, $\eta_p^2 = 0.08$. Overall the Stroop effect was smaller for the 2-item set (79 ms) compared to the 4-item set (99 ms) condition. Furthermore, the Stroop effect significantly interacted with SOA $F_{(3.45,\ 358.77)} = 22.48$, $MSE = 1882$, $p < 0.001$, $\eta_p^2 = 0.18$. Congruent and incongruent RTs increased steadily as SOAs became more positive, but there was a sharp decrease for the incongruent +200 ms condition (see **Figure 2A**). The three-way set size × SOA × item-type interaction was not significant, $F = 1.46$. The pattern of change in the Stroop effect across SOA blocks was similar for the 2-item, and the 4-item sets (see **Figure 3A**).

Table 2 | Mean correct RTs (ms) for the conditions of Experiment 1.

PC	Item type	2-item set (*N* = 56)					4-item set (*N* = 50)				
		−200 ms	−100 ms	0 ms	+100 ms	+200 ms	−200 ms	−100 ms	0 ms	+100 ms	+200 ms
Mostly congruent	Congruent	594 (9)	606 (10)	646 (9)	664 (10)	680 (10)	549 (10)	557 (11)	584 (9)	612 (10)	614 (11)
	Incongruent	710 (11)	724 (12)	774 (12)	791 (12)	755 (14)	679 (12)	690 (13)	724 (13)	756 (13)	674 (15)
Mostly incongruent	Congruent	621 (11)	641 (12)	684 (11)	691 (11)	701 (13)	565 (12)	589 (13)	609 (11)	629 (11)	609 (14)
	Incongruent	666 (9)	680 (10)	727 (9)	749 (10)	737 (13)	641 (9)	653 (10)	697 (10)	722 (11)	668 (13)
ISPC effect		71	80	85	69	39	54	70	53	52	1
PC effect congruent		27	35	37	27	21	16	33	26	17	−5
PC effect incongruent		44	44	47	42	18	38	37	28	34	6

Standard errors are presented in parenthesis. PC, proportion congruency. ISPC effect = (MC-incongruent—MC-congruent)—(MI-incongruent—MI-congruent). PC effect congruent = MI-congruent—MC-congruent. PC effect incongruent = MC-incongruent—MI-incongruent.

Table 3 | Percentage of errors for the conditions of Experiment 1.

PC	Item Type	2-item set (*N* = 56)					4-item set (*N* = 50)				
		−200 ms	−100 ms	0 ms	+100 ms	+200 ms	−200 ms	−100 ms	0 ms	+100 ms	+200 ms
Mostly congruent	Congruent	0.3 (0.1)	0.5 (0.2)	0.2 (0.1)	0.3 (0.1)	0.5 (0.1)	0.2 (0.1)	0.5 (0.2)	0.4 (0.1)	0.2 (0.1)	0.2 (0.1)
	Incongruent	4.5 (1.4)	8.1 (1.7)	6 (1.3)	6.8 (1.6)	3.9 (1)	3.7 (1.4)	5.4 (1.7)	4.4 (1.3)	4.3 (1.7)	3.7 (1.1)
Mostly incongruent	Congruent	1.1 (0.3)	0.7 (0.6)	2.1 (0.5)	2.3 (0.6)	1.8 (0.5)	0 (0.3)	1.5 (0.6)	0.7 (0.6)	0.2 (0.6)	0.2 (0.5)
	Incongruent	2.1 (0.6)	2.9 (0.7)	1.7 (0.5)	3.7 (0.7)	2.9 (0.6)	1.8 (0.7)	2.7 (0.7)	3.4 (0.6)	4.2 (0.8)	2.7 (0.6)
ISPC effect		3.2	5.4	6.2	5.1	2.3	1.7	3.7	1.3	0.1	1.0
PC effect congruent		0.8	0.2	1.9	2.0	1.3	-0.2	1.0	0.3	0.0	0.0
PC effect incongruent		2.4	5.2	4.3	3.1	1.0	1.9	2.7	1.0	0.1	1.0

Standard errors are presented in parenthesis. PC, proportion congruency. ISPC effect = (MC-incongruent—MC-congruent)—(MI-incongruent—MI-congruent). PC effect congruent = MI-congruent—MC-congruent. PC effect incongruent = MC-incongruent—MI-incongruent.

Table 4 | Average number of correct RTs per cell in Experiment 1, after the exclusion of scratch trials, trials with RTs 3 SDs above (or below) the mean, errors, and trials sharing any variety of stimulus- and response-features with the previous trial.

PC	Item type	2-item set					4-item set				
		−200 ms	−100 ms	0 ms	+100 ms	+200 ms	−200 ms	−100 ms	0 ms	+100 ms	+200 ms
Mostly congruent	Congruent	49.8 (3.3)	49.8 (3.4)	49.5 (3.4)	50.1 (2.6)	49.4 (4.7)	50.7 (2.9)	49.8 (3.8)	49.7 (3.3)	49.7 (3.8)	49.9 (2.8)
	Incongruent	8.3 (1.8)	7.7 (1.7)	7.7 (1.7)	7.5 (1.9)	7.6 (1.7)	7.9 (1.8)	7.7 (1.9)	8.1 (1.9)	7.2 (1.9)	7.8 (1.4)
Mostly incongruent	Congruent	8.6 (1.4)	8.8 (1.5)	8.9 (1.6)	8.6 (1.8)	8.3 (1.8)	9.4 (1.3)	8.8 (1.6)	8.9 (1.8)	8.9 (1.7)	9.1 (1.5)
	Incongruent	42.4 (3.9)	41.9 (4.6)	41.9 (4.1)	40.9 (4)	40.6 (4.9)	35.5 (3.6)	34 (3.5)	33.6 (3.9)	31.9 (4.3)	33.3 (3.6)

Standard deviations are presented in parenthesis. Numbers are calculated after the exclusion of scratch trials, trials with RTs 3 SDs above (or below) the mean, errors, and trials sharing any variety of stimulus- and response-features with the previous trial. There were a total of 60 trials in the mostly congruent-congruent and mostly incongruent-incongruent conditions. There were a total of 12 trials in the mostly congruent-incongruent and mostly incongruent-congruent conditions.

The ISPC effect, that is, the two-way interaction between proportion congruency and item type was significant, $F_{(1,\ 104)} = 161.07$, $MSE = 2698$, $p < 0.001$, $\eta^2_p = 0.61$. The three-way interaction between set size, proportion congruency, and item type was also significant, $F_{(1,\ 104)} = 6.33$, $MSE = 2698$, $p = 0.013$, $\eta^2_p = 0.06$. The ISPC effect was larger for the 2-item set (69 ms) compared to the 4-item set condition (46 ms) (see **Figure 4A**). Important to our study, the ISPC effect changed across SOA conditions, which was indicated by the significant three-way interaction between SOA, proportion congruency, and item type, $F_{(3.79,\ 394.37)} = 10.60$, $MSE = 1214$, $p < 0.001$, $\eta^2_p = 0.09$ (see **Figure 5A**). The observed ISPC effect was 63, 75, 69, and 60 ms, and 20 ms for the for the −200, −100, 0 +100, and +200 SOA conditions, respectively. Critically, the four-way interaction between set size, SOA, proportion congruency, and item type was not significant, $F < 1$ (see **Figure 6A**). The relationship between the ISPC effect and SOA was similar for 2-item set and 4-item set conditions.

We ran an additional, 2X4X2X2 mixed-design ANOVA, removing the +200 ms SOA condition, in order to investigate

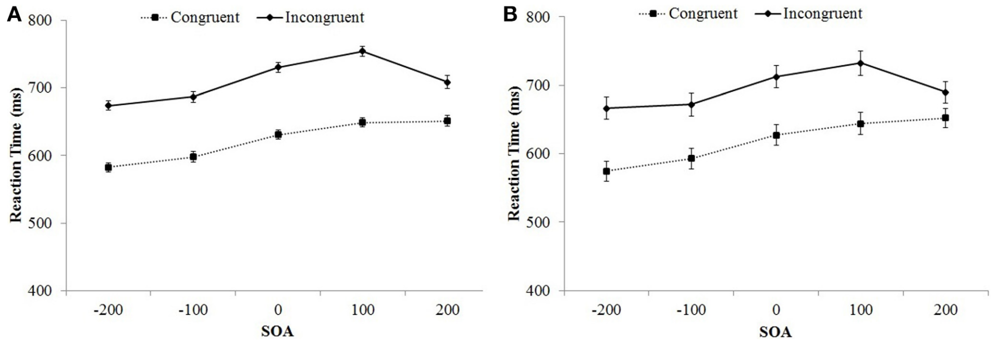

FIGURE 2 | Mean reaction time as a function of SOA and trial type in Experiment 1 (A) and Experiment 2 (B). Bars show standard errors.

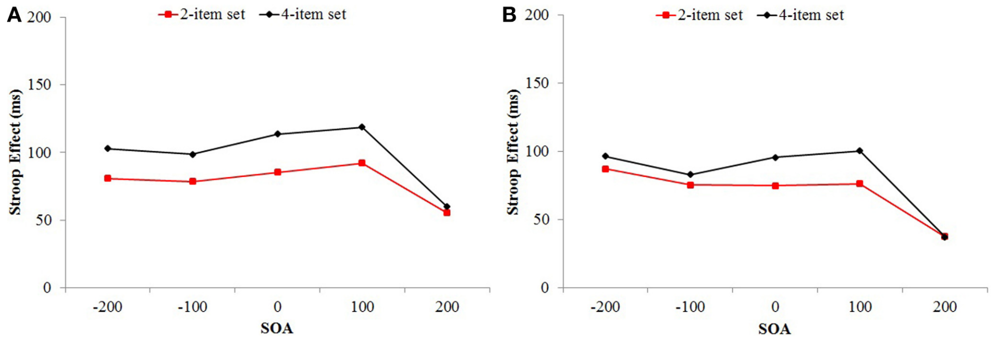

FIGURE 3 | Mean Stroop effect as a function of set size and SOA in Experiment 1 (A) and Experiment 2 (B).

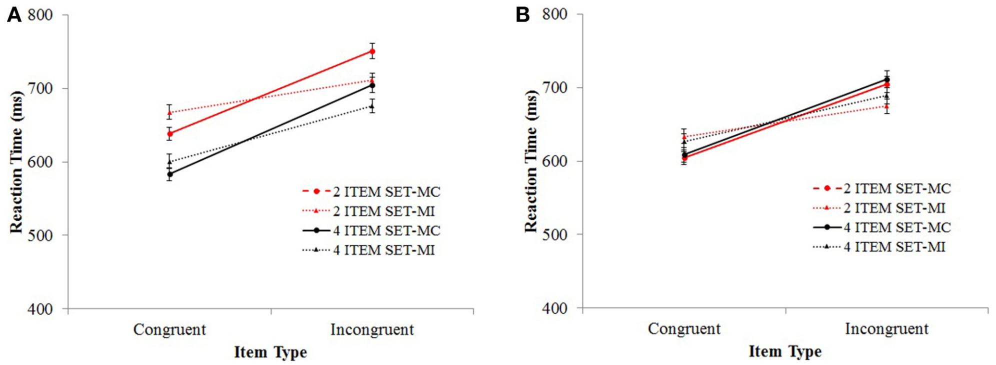

FIGURE 4 | Mean reaction time a function of set size, proportion congruency and item type in Experiment 1 (A) and Experiment 2 (B). Bars show standard errors.

whether interactions involving SOA would remain significant without the +200 ms condition. Therefore, we only report changes involving SOA. First, the two-way interaction between set size and SOA was no longer significant, $F_{(2.68,\ 278.60)} = 1.05$, $p = 0.37$. Second, the three-way interaction between SOA, proportion congruency, and item type was no longer significant, $F_{(2.89,\ 300.79)} = 1.06$, $p = 0.37$[1].

We also compared proportion congruence (PC) effects for incongruent and congruent items across the SOA conditions (including the +200). PC effect for congruent items was calculated as MI-congruent minus MC-congruent. PC effect for incongruent items was calculated as MC-incongruent minus MI-incongruent. PC effects were analyzed with a 2X5X2 mixed-design ANOVA, in which set size (2-item vs. 4-item set) was a between-subjects factor; SOA (−200, −100, 0, +100 or +200), and item type (congruent vs. incongruent) were within-subject

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[1] We would like to thank our reviewer for this suggestion.

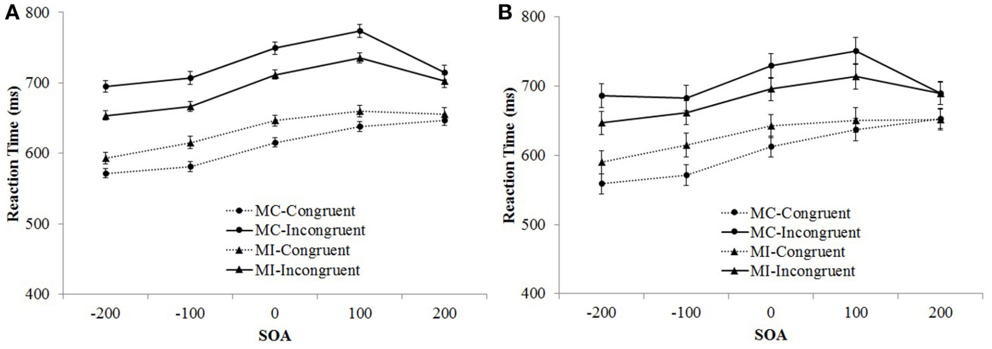

FIGURE 5 | Mean reaction time a function of SOA, proportion congruency and item type in Experiment 1 (A) and Experiment 2 (B). Bars show standard errors.

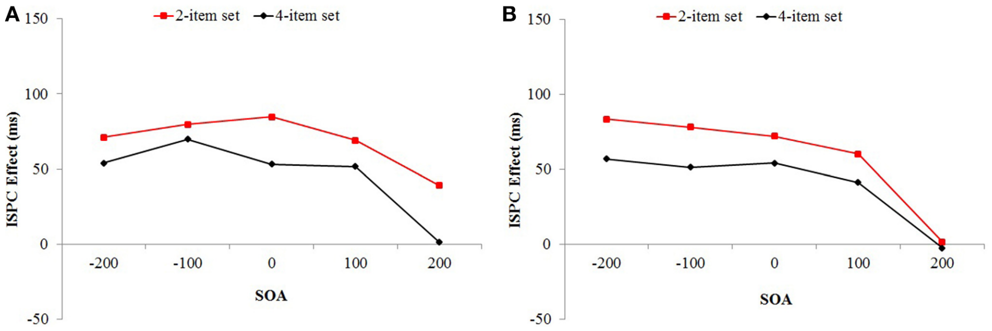

FIGURE 6 | Mean ISPC effect a function of set size and SOA in Experiment 1 (A) and Experiment 2 (B).

factors. There was a main effect of set size, $F_{(1,\ 104)} = 6.33$, $MSE = 5396.86$, $p < 0.05$, $\eta_p^2 = 0.06$. There was also a main effect of SOA, $F_{(3.79,\ 394.37)} = 10.59$, $MSE = 2428.22$, $p < 0.001$, $\eta_p^2 = 0.09$. Critically, neither the two-way interaction between item-type and set size, $F_{(1,\ 104)} = 0.11$, $p = 0.92$, nor the two-way interaction between item-type and SOA, $F_{(3.72,\ 386.62)} = 1.25$, $p = 0.29$, nor the three-way interaction between SOA, item-type and set size, $F_{(3.72,\ 386.62)} = 0.45$, $p = 0.76$, were significant[2].

EXPERIMENT 2

Similar to Experiment 1, scratch trials and trials with RTs 3 *SDs* above (or below) the mean were removed. Trials sharing any variety of stimulus- and response-features with the previous trial were also removed to exclude effects sequential repetition or alternation of color and/or word dimensions (Mayr et al., 2003; Hommel et al., 2004). Analyses were run with the remaining 76% of the trials. Correct RT and PE data were analyzed with separate 2X5X2X2 mixed-design ANOVAs, in which set size (2-item vs. 4-item set) and SOA (−200, −100, 0, +100 or +200) were between-subjects factors; proportion congruency (MC vs. MI), and item type (congruent vs. incongruent) were within-subject factors. The alpha level was set at 0.05 for all analyses and partial eta squared (η_p^2) is reported as the measure of effect size. **Tables 5**, **6** present mean RTs and PEs for the conditions of Experiment 2, respectively. Only the results from the RT analyses are reported, since PEs were low ($M = 1.9\%$) and the results of the PE analyses were parallel with that of the RT analyses.

The between-subjects main effect of SOA was significant, $F_{(4,\ 103)} = 3.31$, $MSE = 21,641$, $p = 0.014$, $\eta_p^2 = 0.11$. RTs linearly increased as SOAs became more positive. The Stroop effect (the main effect of item type) was significant, $F_{(1,\ 103)} = 615.75$, $MSE = 1059$, $p < 0.001$, $\eta_p^2 = 0.86$, and it's interaction with set size was also significant, $F_{(1,\ 103)} = 3.95$, $MSE = 1059$, $p = 0.049$, $\eta_p^2 = 0.04$. Overall the Stroop effect was smaller for the 2-item set (70 ms) compared to the 4-item set (83 ms) condition. Furthermore, the interaction between the Stroop effect and SOA was significant $F_{(4,\ 103)} = 11.36$, $MSE = 1059$, $p < 0.001$, $\eta_p^2 = 0.31$. Congruent and incongruent RTs increased steadily as SOAs became more positive, but there was a sharp decrease for the incongruent +200 ms condition (see **Figure 2B**). The three-way set size × SOA × item-type interaction was not significant, $F < 1$. The pattern of change in the Stroop effect across SOA blocks was similar for the 2-item, and the 4-item set conditions (see **Figure 3B**).

The two-way interaction between proportion congruency and item type (the ISPC effect) was significant, $F_{(1,\ 103)} = 162.54$, $MSE = 423$, $p < 0.001$, $\eta_p^2 = 0.61$. ISPC effect and set size

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[2] We would like to thank James R. Schmidt for suggesting this analysis.

Table 5 | Mean correct RTs (ms) for the conditions of Experiment 2.

PC	Item type	2-item set					4-item set				
		−200 ms (*N* = 12)	−100 ms (*N* = 12)	0 ms (*N* = 13)	+100 ms (*N* = 11)	+200 ms (*N* = 13)	−200 ms (*N* = 11)	−100 ms (*N* = 10)	0 ms (*N* = 10)	+100 ms (*N* = 9)	+200 ms (*N* = 12)
Mostly congruent	Congruent	563 (20)	557 (20)	600 (19)	655 (21)	646 (19)	555 (21)	585 (22)	625 (22)	619 (23)	659 (20)
	Incongruent	692 (24)	672 (24)	711 (23)	762 (25)	684 (23)	680 (25)	694 (27)	747 (27)	740 (28)	695 (24)
Mostly incongruent	Congruent	598 (23)	606 (23)	628 (22)	693 (24)	643 (22)	582 (24)	624 (25)	656 (25)	608 (27)	661 (23)
	Incongruent	644 (23)	642 (23)	667 (22)	739 (24)	679 (22)	650 (24)	681 (25)	725 (25)	688 (26)	700 (23)
ISPC effect		83	78	72	60	2	57	51	54	41	−3
PC effect congruent		35	49	28	38	−3	27	39	32	−11	2
PC effect incongruent		48	29	44	22	5	30	13	22	52	−5

Standard errors are presented in parenthesis. PC, proportion congruency. ISPC effect = (MC-incongruent—MC-congruent)—(MI-incongruent—MI-congruent). PC effect congruent = MI-congruent—MC-congruent. PC effect incongruent = MC-incongruent—MI-incongruent.

Table 6 | Percentage of errors for the conditions of Experiment 2.

PC	Item type	2-item set					4-item set				
		−200 ms (*N* = 12)	−100 ms (*N* = 12)	0 ms (*N* = 13)	+100 ms (*N* = 11)	+200 ms (*N* = 13)	−200 ms (*N* = 11)	−100 ms (*N* = 10)	0 ms (*N* = 10)	+100 ms (*N* = 9)	+200 ms (*N* = 12)
Mostly congruent	Congruent	0 (0.1)	0.3 (0.1)	0.1 (0.1)	0.1 (0.1)	0.5 (0.1)	0.2 (0.1)	0.2 (0.2)	0.3 (0.2)	0.1 (0.2)	0.4 (0.1)
	Incongruent	3.6 (1.5)	5.8 (1.5)	5.5 (1.4)	4.5 (1.6)	4.1 (1.4)	1.5 (1.6)	2.9 (1.6)	4.8 (1.6)	6.3 (1.7)	3.2 (1.5)
Mostly incongruent	Congruent	0.2 (0.4)	0.8 (0.4)	0.6 (0.4)	0.2 (0.4)	1.4 (0.4)	0.2 (0.4)	0.7 (0.4)	0.3 (0.4)	0.2 (0.4)	1 (0.4)
	Incongruent	1.3 (0.9)	3.1 (0.9)	2.5 (0.8)	2 (0.9)	3.2 (0.8)	0.5 (0.9)	1.6 (0.9)	4.3 (0.9)	5.4 (1)	2.2 (0.9)
ISPC effect		2.4	3.2	3.5	2.5	1.8	1.0	1.7	0.4	1.0	1.6
PC effect congruent		0.2	0.5	0.5	0.1	0.8	0.0	0.5	-0.1	0.1	0.5
PC effect incongruent		2.3	2.7	3.0	2.5	0.9	1.0	1.2	0.5	0.9	1.1

Standard errors are presented in parenthesis. PC, proportion congruency. ISPC effect = (MC-incongruent—MC-congruent)—(MI-incongruent—MI-congruent). PC effect congruent = MI-congruent—MC-congruent. PC effect incongruent = MC-incongruent—MI-incongruent.

Table 7 | Average number of correct RTs per cell in Experiment 2.

PC	Item type	2-item set					4-item set				
		−200 ms	−100 ms	0 ms	+100 ms	+200 ms	−200 ms	−100 ms	0 ms	+100 ms	+200 ms
Mostly congruent	Congruent	250 (14.5)	251.2 (8.8)	249.8 (6.7)	252.5 (9.8)	248.8 (6.9)	252.6 (7.9)	248.3 (6.3)	253.5 (3.8)	252.1 (7)	251.8 (7.8)
	Incongruent	41.1 (4.4)	41 (5.1)	40.2 (4.2)	39.6 (6.1)	41.8 (4.5)	41.4 (3.5)	41.1 (4.4)	40.3 (6.1)	38.2 (3)	40.2 (7.3)
Mostly incongruent	Congruent	42.3 (3.1)	45.8 (3)	45.8 (4.5)	44 (6)	44.6 (3.7)	45.9 (3)	46.2 (2.8)	45 (4.3)	44.9 (4)	44.6 (3)
	Incongruent	213.5 (8.6)	211 (11.9)	215.6 (11.1)	209.3 (14.3)	213.1 (10.8)	178.9 (10.5)	171.8 (10.4)	165.8 (12.8)	163 (14.3)	172.5 (11.1)

Standard deviations are presented in parenthesis. Numbers are calculated after the exclusion of scratch trials, trials with RTs 3 SDs above (or below) the mean, errors, and trials sharing any variety of stimulus- and response-features with the previous trial. There were a total of 300 trials in the mostly congruent-congruent and mostly incongruent-incongruent conditions. There were a total of 60 trials in the mostly congruent-incongruent and mostly incongruent-congruent conditions.

interaction was also significant, $F_{(1,\ 103)} = 5.86$, $MSE = 423$, $p = 0.017$, $\eta^2_p = 0.05$. The ISPC effect was larger for the 2-item set (59 ms) compared to the 4-item set (40 ms) condition (see **Figure 4B**). There was a significant three-way interaction between SOA, proportion congruency, and item type, $F_{(4,\ 103)} = 12.04$, $MSE = 423$, $p < 0.001$, $\eta^2_p = 0.32$ (see **Figure 5B**). The observed ISPC effect was72, 69, 63, and 56 ms, and −1 ms for the −200, −100, 0 +100, and +200 SOA conditions, respectively. Important for the study, the four-way interaction between set size, SOA, proportion congruency, and item type was not significant, $F < 1$ (see **Figure 6B**). The relationship between the ISPC effect and SOA was similar for the 2-item set and 4-item set conditions. Pair-wise comparisons of ISPC effects across SOAs showed that only the ISPC effect observed in the +200 ms condition was significantly different from all other SOA conditions. Average number (and standard deviation) of correct RTs per cell in Experiment 2 is presented in **Table 7**.

We ran an additional, 2X4X2X2 mixed-design ANOVA, removing the +200 ms SOA condition, in order to investigate whether interactions involving SOA would remain significant without the +200 ms condition. Therefore, we only report changes involving SOA. First, the two-way interaction between item type and SOA was no longer significant, $F_{(3,\ 80)} = 0.57$, $p = 0.64$. Second, the three-way interaction between SOA, proportion congruency, and item type was no longer significant, $F_{(3,\ 80)} = 0.72, p = 0.55$.

We also compared proportion congruence (PC) effects for incongruent and congruent items across the SOA conditions (including the +200). PC effect for congruent items was calculated as MI-congruent minus MC-congruent. PC effect for incongruent items was calculated as MC-incongruent minus MI-incongruent. PC effects were analyzed with a 2X5X2 mixed-design ANOVA, in which set size (2-item vs. 4-item set) was a between-subjects factor; SOA (−200, −100, 0, +100 or +200), and item type (congruent vs. incongruent) were within-subject factors. There was a main effect of set size, $F_{(1,\ 103)} = 5.86$, $MSE = 846.35$, $p < 0.05$, $\eta_p^2 = 0.05$. There was a main effect of SOA, $F_{(4,\ 103)} = 12.04$, $MSE = 846.35$, $p < 0.001$, $\eta_p^2 = 0.32$. Critically, neither the two-way interaction between item-type and set size, $F_{(1,\ 103)} = 0.85, p = 0.77$, nor the two-way interaction between item-type and SOA, $F_{(4,\ 103)} = 0.99, p = 0.42$, nor the three-way interaction between SOA, item-type and set size, $F_{(4,\ 103)} = 1.56, p = 0.19$, were significant.

AWARENESS DATA

Immediately after the experiments, participants answered a series of questions assessing their awareness of the ISPC manipulation (see **Figure 7**). Five participants' (3 participants in Experiment 1, 2 participants in Experiment 2) awareness data were not recorded. Regarding the first (awareness) question, in Experiment 1, 76 out of 103 (74%) participants reported noticing specific words and colors being paired more frequently. In other words, they noticed the ISPC manipulation. The number of participants who noticed the ISPC manipulation was higher for the 2-item set condition (44 out of 53, 83%) compared to the 4-item set condition (32 out of 50, 64%), $\chi^2_{(1)} = 4.81, p < 0.05$. Regarding the confidence judgments, 67 out of 102 (66%) participants reported that they were certain about their answers. This was higher for the 2-item set condition (43 out of 53, 81%) compared to the 4-item set condition (24 out of 49, 49%), $\chi^2_{(1)} = 11.68, p < 0.001$. The results for Experiment 2 regarding the awareness question were parallel to those of Experiment 1. Seventy-nine out of 111 (71%) participants reported noticing the ISPC manipulation. The number of participants who noticed the ISPC manipulation was higher for the 2-item set condition (48 out of 58, 83%) compared to the 4-item set condition (31 out of 53, 58%), $\chi^2_{(1)} = 7.95, p < 0.005$. Regarding the confidence judgments, 72 out of 111 (65%) participants reported that they were certain about their answer. This was higher for the 2-item set condition (46 out of 59, 78%) compared to the 4-item set condition (26 out of 52, 50%), $\chi^2_{(1)} = 9.49$, $p < 0.005$. To sum up, the number of participants who were aware of the ISPC manipulation, and who were certain about their responses, was higher for the 2-item-set condition compared to the 4-item-set condition (**Figure 7**)[3].

For the next (matching) question, participants were given 10 colors and 10 color words presented as two columns and were asked to connect the more frequently paired colors and color words by drawing a line in-between. Eight of the 10 colors and color words were used in the experiment, the remaining two were new. They were encouraged to guess if they were not certain, nevertheless, none of the participants paired the new colors and color words. For each participant, the *proportion of correct pairs* was calculated separately for the MC-congruent, MI-congruent, MC-incongruent, and MI-incongruent conditions (see **Figure 8**). Proportions were analyzed with separate 2X2 mixed-design ANOVAs for congruent and incongruent trials, with set size (2-item vs. 4-item) as the between-subjects factor, and proportion congruency (MC vs. MI) as the within-subject factor.

In Experiment 1, for the congruent pairs, the main effect of proportion congruency was significant, $F_{(1,\ 101)} = 49.70$, $MSE = 0.041$, $p < 0.001$, $\eta_p^2 = 0.33$. The proportion of correct MC-congruent pairs (0.29) was higher than the MI-congruent pairs (0.09). None of the other main effects or interactions were significant, Fs < 2. For the incongruent pairs, there was a significant main effect of proportion congruency, $F_{(1,\ 101)} = 49.89$, $MSE = 0.043$, $p < 0.001$, $\eta_p^2 = 0.33$. The proportion of correct MI-incongruent pairs (0.38) was higher than the MC-incongruent pairs (0.18). There was a significant between-subjects main effect of set size, $F_{(1,\ 101)} = 74.96$, $MSE = 0.098$, $p < 0.001$, $\eta_p^2 = 0.43$. The proportion of correct incongruent pairs was higher for the 2-item set condition (0.47) compared to the 4-item set condition (0.09). The two-way interaction between set size and proportion congruency was also significant, $F_{(1,\ 101)} = 20.06$, $MSE = 0.043$, $p < 0.001$, $\eta_p^2 = 0.17$. The difference between the proportion of correct MI-incongruent and MC-incongruent pairs was more pronounced in the 2-item set condition compared to the 4-item set condition (2-item set MC = 0.30, 2-item set MI = 0.64, 4-item set MC = 0.06, 4-item set MI = 0.13).

Experiment 2 yielded results parallel to Experiment 1. For the congruent pairings, the main effect of proportion congruency was significant, $F_{(1,\ 110)} = 60.57$, $MSE = 0.053$, $p < 0.001$, $\eta_p^2 = 0.36$. The proportion of correct MC-congruent pairs (0.38) was higher than the MI-congruent pairs (0.14). The main effect of set size was not significant, $F < 1$. The two-way interaction between set size and proportion congruency was significant, $F_{(1,\ 110)} = 4.04$, $MSE = 0.053$, $p < 0.05$, $\eta_p^2 = 0.04$. The difference between the proportion of correct MC-congruent and MI-congruent pairs was more pronounced in the 4-item set condition compared to the 2-item set condition (2-item set MC = 0.33, 2-item set MI = 0.15, 4-item set MC = 0.44, 4-item set MI = 0.14). For the incongruent pairs, there was a significant main effect of proportion congruency, $F_{(1,\ 110)} = 29.41$, $MSE = 0.048$, $p < 0.001$, $\eta_p^2 = 0.21$. The proportion of correct MI-incongruent pairs (0.41) was higher than the MC-incongruent pairs (0.25). The between-subjects main effect of set size was significant, $F_{(1,\ 110)} = 98.16$, $MSE = 0.109$, $p < 0.001$, $\eta_p^2 = 0.47$. The proportion of correct incongruent pairs was higher for the 2-item set condition (0.55) compared to the 4-item set condition (0.11). The two-way interaction between set size and proportion congruency was also

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[3] In order to investigate whether subjects who were aware of the manipulation differed from subjects who weren't, we compared these two groups regarding the observed ISPC effects with non-parametric tests. Analyses did not yield significant results (ps > 0.05).

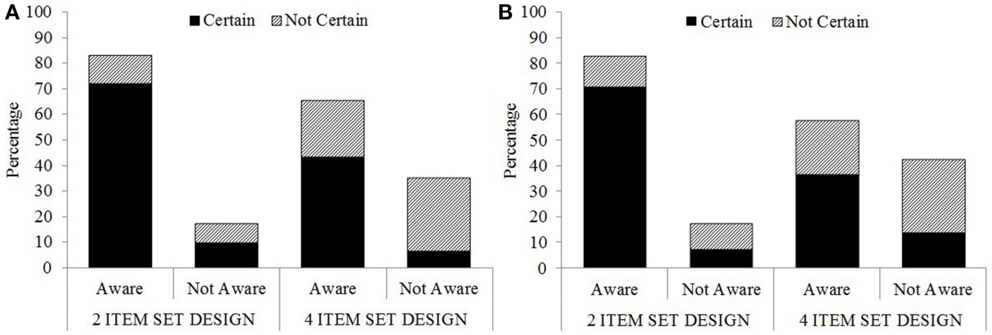

FIGURE 7 | Percentage of participants who were aware of the manipulation, and their confidence judgments in Experiment 1 (A) and Experiment 2 (B).

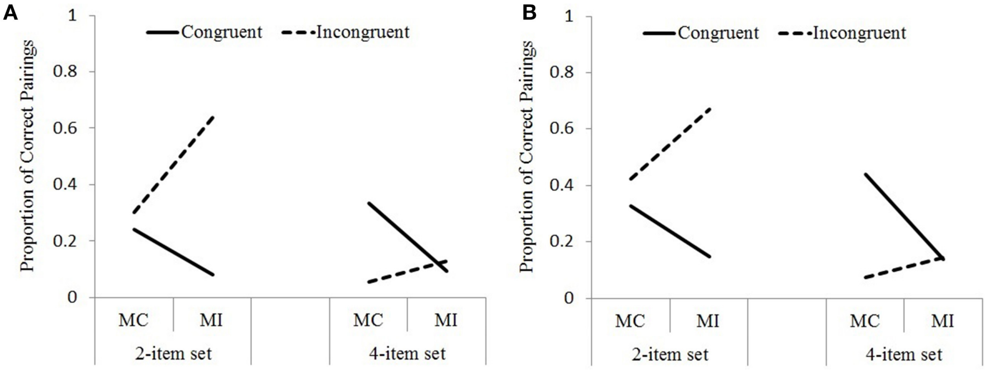

FIGURE 8 | Proportion of correct pairings for the matching question in Experiment 1 (A) and Experiment 2 (B).

significant, $F_{(1,\ 110)} = 9$, $MSE = 0.048$, $p < 0.005$, $\eta_p^2 = 0.08$. The difference between the proportion of correct MI-incongruent and MC-incongruent pairs was more pronounced in the 2-item set condition compared to the 4-item set condition (2-item set MC = 0.42, 2-item set MI = 0.67, 4-item set MC = 0.07, 4-item set MI = 0.14).

In summary, the results for the matching question revealed that participants became aware of the congruent pairs more in the MC condition, and incongruent pairs more in the MI condition. Additionally, these differences between the MI-incongruent and MC-incongruent conditions were more pronounced in the 2-item set condition than the 4-item set condition (**Figure 8**).

DELTA PLOTS

We compared the time course of ISPC effects for the 2-item and 4-item set conditions in Experiment 2 using delta plots (De Jong et al., 1994). Delta plots provide information on the ISPC effect across the RT distribution. In other words, they demonstrate how the ISPC effect changes as the responses slow down. Our previous average response latency analyses showed that the relationship between the ISPC effect and SOA was similar for the 2-item and 4-item set conditions. Parallel to this, we expected the time-course of the ISPC effect to be similar for the 2-item and 4-item conditions, as well.

First the 10th, 20th… 80th, 90th percentiles of the correct RT data for each condition were calculated for each participant, and then averaged across participants. Then, ISPC effects were calculated for each SOA and each set size condition using these averaged percentiles, and are displayed on the vertical axis of the delta plots. Lastly, means of averaged percentiles were calculated, which are displayed on the horizontal axis of the plots (**Figure 9**).

Overall, the ISPC effect was smallest for the fastest responses and increased as the responses got slower. Delta plots for the 2-item and 4-item set conditions were more similar for the positive SOA conditions compared to the 0 SOA or negative SOA conditions. In the +200 SOA condition, the ISPC effect was absent in both the 2-item and 4-item set conditions, except for the slowest responses. In the +100 SOA condition, the time-course of the ISPC effect was similar for the 2-item and 4-item set conditions. In the 0 SOA and −100 SOA conditions, the ISPC effect for the 2-item and 4-item set conditions were similar for the fastest responses. However, for the slower responses, the ISPC effect in the 2-item set condition increased faster, compared to the 4-item set condition. In the -200 SOA condition, this pattern was reversed: for the fastest responses the ISPC effect for the 2-item set condition was larger compared to the 4-item set condition. However, they became more similar as the responses got slower.

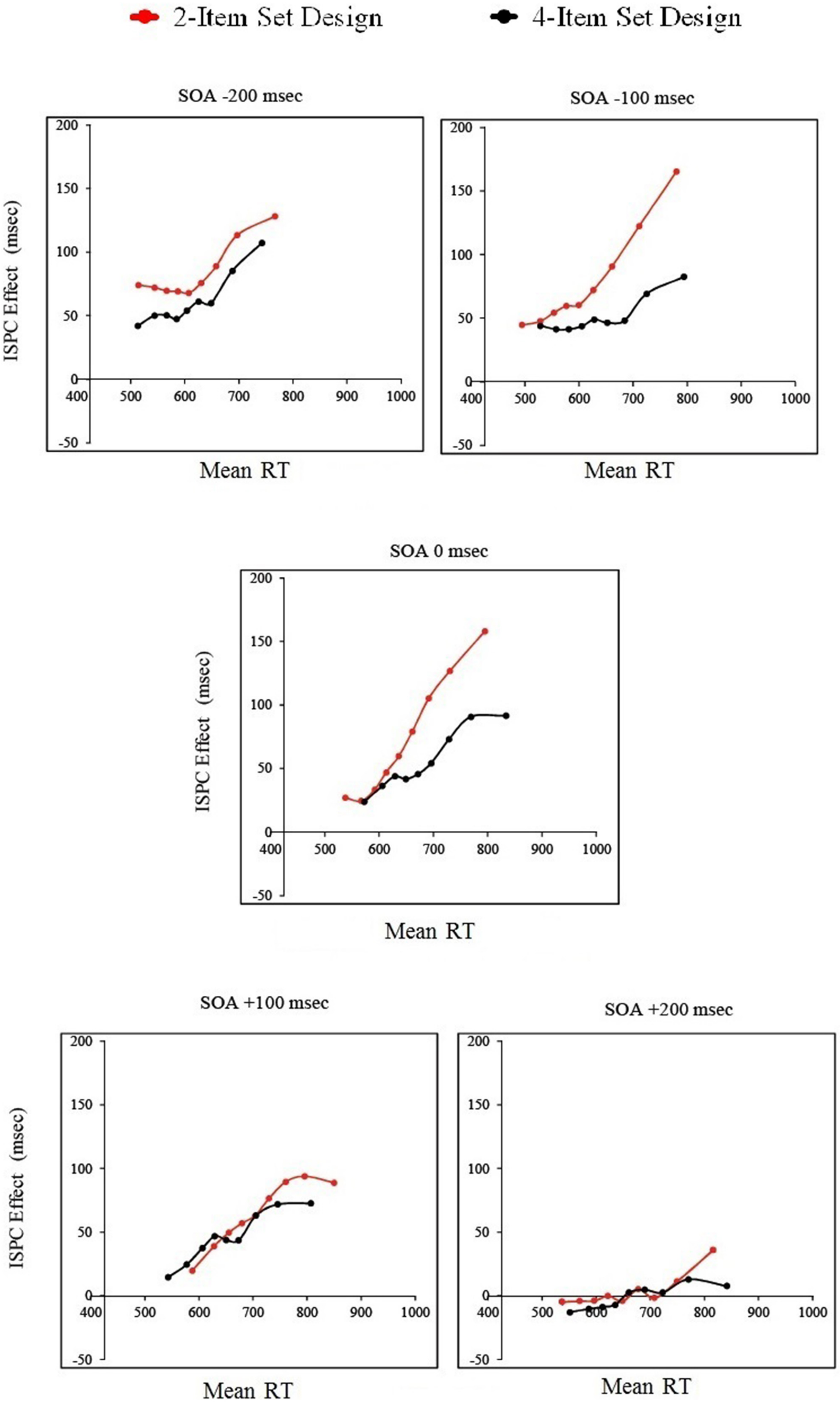

FIGURE 9 | Delta plots of the ISPC effects as a function of set size and SOA for Experiment 2.

When the delta plots for the different SOA conditions are compared within each set size condition, one can observe that while the time course of the ISPC effect across different SOA conditions are comparable for the 4-item set condition, they show more variation for the 2-item set condition. Indeed, the observed differences between the 2-item and 4-item set conditions across SOA conditions were driven primarily by the change in the delta plots for the 2-item set condition.

In summary, the delta plot analysis revealed that the ISPC effect was smaller for fastest responses and increased as the responses got slower. The ISPC effect followed different time courses for the 2-item and 4-item set conditions, with more stable time courses for the 4-item set condition compared to the 2-item set condition.

DISCUSSION

In the current study, we investigated the time course of the ISPC effect with two experiments by combining SOA and set size (Bugg and Hutchison, 2013) manipulations. More specifically, we manipulated the SOA between the relevant (color) and irrelevant (word) Stroop dimensions, and compared the change in the ISPC effect as a function of SOA for 2-item and 4-item set size conditions. In the first experiment, set size was manipulated as a between-subjects factor and SOA as a within-subject factor. In the second experiment, considering the possibility that participants might switch between control and contingency learning processes across different SOA blocks, SOA was manipulated as between-subjects factors. This also increased the number of stimuli for each SOA condition, improving the validity of observations. In addition to the conventional RT analyses, time course of the ISPC effect was investigated using delta plots. Furthermore, awareness data regarding the ISPC manipulations were collected and analyzed.

Overall, Experiment 1 and Experiment 2 did not yield different findings. The results indicated that the SOA manipulation was working as expected. In other words, the Stroop effect changed as a function of different SOAs (Glaser and Glaser, 1982). In addition to this, the SOA manipulation interacted with the ISPC effect. More specifically, the ISPC effects observed for different SOA conditions were comparable, except for the +200 SOA condition, in which the ISPC effect was very small, if present. Of specific interest to our study, the effect of the SOA manipulation on the ISPC effect did not change as a function of set size, as indicated by the lack of interaction between the ISPC effect, set size, and SOA.

Regarding the 2-item set condition, the results supported our predictions. The ISPC effect was observed, when participants were able to use the word to predict the correct response, yet it disappeared when the word was presented too late to help prediction, i.e., the +200 condition. The effects of word reading on color naming processes, nevertheless, persisted even in the +200 SOA condition, as indicated by a significant Stroop effect. This observation supports the notion that Stroop interference and contingency learning processes are independent in the 2-item set condition (Schmidt and Besner, 2008). Alternatively, it could also be argued that the word is presented too late to cause enough interference to call for control processes. This would also result in a reduced Stroop and ISPC effects, as observed[4].

Results for the 4-item set condition were parallel to that of the 2-item set condition. If we attribute the observed ISPC effect in the 4-item set condition to item-specific control processes, then our results favor an explanation in which the word acts as the ISPC signal. The lack of an ISPC effect in the +200 SOA condition supports this explanation. Participants in this condition apparently initiate a response before the word has a chance to trigger item-specific control processes. Still, this explanation is somewhat difficult to accommodate, especially in the negative SOA conditions, since it requires controlling word reading processes *after* the word is read. Consequently, the observed ISPC effect in the 4-item set condition cannot be easily explained by reactive control processes.

Alternatively, the RT difference between the MC-incongruent and MI-incongruent trials may be a result of the differences in the frequency of the incongruent items, while the difference between the MC-congruent and MI-congruent trials may stem from differences in S-R contingency learning. Replicating these results in future studies using transfer stimuli is essential. Evidently, processes underlying the ISPC effect in the 4-item set condition need to be investigated in more detail to gain a better understanding of their exact nature.

Most ISPC explanations are based on computational models of the Stroop effect that assume spreading activation through color naming and word reading pathways in associative memory. These models erroneously predict that a larger Stroop effect would be observed when the word precedes the color (Cohen et al., 1990). The changes in the Stroop effect as a function of SOA have alternatively been explained with strategies that are independent of the Stroop effect itself (Glaser and Glaser, 1982; Cohen et al., 1990). According to these explanations; in separated versions of the Stroop task, participants rely on their knowledge of the relative timing of the relevant (color) and irrelevant (word) Stroop dimensions to modulate visual attention and/or response selection processes (Appelbaum et al., 2009, 2012). The participants in our study may have implemented a similar temporal attentional control strategy in addition to the S-R learning and/or item-specific control processes that they have been already using. Consequently, to account for the observed ISPC effect and SOA interaction, one has to either assume SOA related attentional control strategies or to adopt a different model of the Stroop effect (see Roelofs, 2010b).

A recent study argued that temporal learning might explain proportion congruency effects in the Stroop task (Schmidt, 2013c, 2014). More specifically Schmidt (2013c, 2014) argued that an explanation for the proportion congruency effects might be that the participants are learning, and in turn predicting, when to respond. Even though effects of temporal learning on the ISPC effect were not conclusive, there was a trend in the data of Schmidt (2014). Separated versions of the Stroop task provide the participants with more informative cues regarding when to respond. Therefore, in our experiment, a temporal learning mechanism might have played a role in the

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[4] We would like to thank our reviewer for this suggestion.

observed change in the magnitude of the ISPC effect as a function of SOA.

Additional to the conventional RT analyses, we investigated the time course of the ISPC effect using delta plots (De Jong et al., 1994). Overall, delta plot analyses showed that the ISPC effect was smaller for faster responses and increased as the responses got slower. The slopes of the delta plots for the 2-item set condition were steeper than those of the 4-item set condition, especially in the negative and 0 SOAs. On the one hand, it is reasonable to assume that S-R learning (or temporal learning) processes affect shorter RTs more than longer RTs since faster responses are more likely to be modified by S-R learning or temporal prediction processes. Attentional control processes, on the other hand, should affect longer RTs more than shorter RTs, since attentional effects are more likely to be cumulative (Schmidt, 2014). However, for both faster (as observed in the −200 SOA condition) and slower (as observed in −100 and 0 SOA conditions) responses, our analyses showed that whenever there was a difference between the 2-item and 4-item set conditions, the ISPC effect was larger for the 2-item set condition compared to the 4-item set condition. Therefore, the results do not fit well with the notion that S-R learning is dominant in the 2-item set condition and control processes are dominant in the 4-item set condition. This dissociation between the negative and positive SOA conditions regarding the change in the magnitude of the ISPC effect as a function of response latency, calls for further investigation.

To the knowledge of the authors, this study is the first in which, additional data were collected after an ISPC experiment to assess whether or not participants were aware of the ISPC manipulation. According to the results participants in the 2-item set condition were aware of the ISPC manipulation more than the participants in the 4-item set condition. They came up with a higher proportion of correct pairs in the experiment, as well. These results suggest that awareness might be playing a role in the observed differences between the 2-item and 4-item set ISPC effects. Previously, Crump et al. (2008) investigated the effects of awareness on the CSPC effect by explicitly telling the participants about the CSPC manipulation. Their results showed that awareness did not influence the CSPC effect. In a recent study, however, Blais et al. (2012) observed that awareness had little role in the list-wide PC effect. In the current study, we did not observe an effect of awareness on the magnitude of the ISPC effect, indicated by the results of our *post-hoc* analysis (see Footnote 3). Nevertheless, possible effects of awareness of the ISPC manipulation on the magnitude of the ISPC effect need to be investigated in more detail in order to gain a better understanding of the underlying mechanisms.

In conclusion, our results showed that manipulating the SOA between the relevant and irrelevant dimensions changed the ISPC effect. The ISPC effect observed in the +200 condition was smaller, if at all present, than the ISPC effects in other SOA conditions. Moreover, this pattern was observed in both the 2-item and 4-item set conditions, that is, regardless of whether reactive control or contingency learning processes were dominant. Furthermore, a higher percentage of participants were aware of the ISPC manipulation in the 2-item set condition compared to the 4-item set condition. In addition, RT distribution analyses (delta plots) revealed that the ISPC effect was smaller for fastest responses and increased as the responses got slower. The SOA manipulation proves promising to further the understanding of the mechanisms underlying the ISPC effect.

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Proportion congruency effects: instructions may be enough

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Learning takes time, namely, one needs to be exposed to contingency relations between stimulus dimensions in order to learn, whereas intentional control can be recruited through task demands. Therefore showing that control can be recruited as a function of experimental instructions alone, that is, adapting the processing according to the instructions before the exposure to the task, can be taken as evidence for existence of control recruitment in the absence of learning. This was done by manipulating the information given at the outset of the experiment. In the first experiment, we manipulated list-level congruency proportion. Half of the participants were informed that most of the stimuli would be congruent, whereas the other half were informed that most of the stimuli would be incongruent. This held true for the stimuli in the second part of each experiment. In the first part, however, the proportion of the two stimulus types was equal. A proportion congruent (PC) effect was found in both parts of the experiment, but it was larger in the second part. In our second experiment, we manipulated the proportion of the stimuli within participants by applying an item-specific design. This was done by presenting some color words most often in their congruent color, and other color words in incongruent colors. Participants were informed about the exact word-color pairings in advance. Similar to Experiment 1, this held true only for the second experimental part. In contrast to our first experiment, informing participants in advance did not result in an item-specific proportion effect, which was observed only in the second part. Thus our results support the hypothesis that instructions may be enough to trigger list-level control, yet learning does contribute to the PC effect under such conditions. The item-level proportion effect is apparently caused by learning or at least it is moderated by it.

Keywords: Stroop, proportion congruent, item-specific congruency, conflict adaptation, control, learning

INTRODUCTION

The Stroop paradigm (Stroop, 1935; MacLeod, 1991) has been extensively used to investigate control of attention. In this paradigm, participants are asked to name the color of the ink of a color word and ignore the meaning of the stimulus word. Usually, participants respond slower when the word and the ink color are incongruent (e.g., GREEN written in red) compared to when the word is congruent with the ink color (e.g., RED written in red). This effect is known as *the Stroop effect* and it demonstrates effects of prepotent word reading processes on color naming performance (Cohen et al., 1990; MacLeod, 1991; Tzelgov et al., 1992).

The proportion congruent (PC) effect—an increase of the Stroop effect when the proportion of congruent stimuli increases—is frequently taken as a marker of conflict adaptation in the Stroop task (i.e., participants are able to adapt to conflict encountered in the task by adjusting attention away from the source of conflict; Logan and Zbrodoff, 1979; Logan, 1980; Lowe and Mitterer, 1982; Cheesman and Merikle, 1986; Lindsay and Jacoby, 1994).

Botvinick et al. (2001) modeled the control of the Stroop effect by extending Cohen et al.'s (1990) work, thereby providing a possible explanation for the PC effect[1]. According to the conflict monitoring model of Botvinick et al. (2001), control is triggered by a module responsible for detecting conflicts in information processing; namely, the conflict monitoring unit (assumed to be located at the ACC). This unit calculates the amount of conflict at the response layer and accordingly increases the input from the relevant task demand (color naming) units when the level of conflict is high. This mechanism measures the level of conflict on each trial and then the cognitive system uses this conflict information to adjust attention (i.e., *conflict adaptation*).

In the Stroop task, incongruent items generate more conflict than congruent items. Namely, higher proportions of incongruent items give rise to higher levels of conflict, which in turn result in increased cognitive control via the activation of the relevant task demands (color naming), leading to a decreased Stroop effect.

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[1] It should be clear that in the absence of neutral stimuli (as in the designs of Logan and Zbrodoff, 1979; Logan, 1980; Lowe and Mitterer, 1982; Lindsay and Jacoby, 1994) the PC effect means that the proportion of incongruent stimuli out of the total number of stimuli in a given block decreases as the proportion of congruent stimuli increases. Botvinick et al. (2001) modeled the control of the Stroop effect by simulating the results of Tzelgov et al. (1992). Note that these researchers employed neutrals in their design and their manipulation was based on changing the proportion of color word vs. neutrals while holding the ratio of congruent to incongruent stimuli constant. This implies that in this case conflict adaptation was led by the proportion of the conflicting (incongruent) stimuli out of the total number of stimuli in each experimental block.

However, recent findings have challenged this widely accepted model and the conflict adaptation theory overall. This challenge started with the finding that the PC effect can be item-specific. The *item-specific proportion congruent (ISPC) effect*—larger congruency effect for color words presented mostly congruently than for those presented mostly incongruently (Jacoby et al., 2003; Schmidt et al., 2007)—pointed to possible involvement of learning processes in the proportion effect. Jacoby et al. (2003) introduced the ISPC task by manipulating PC between items instead of between participants or between blocks. That is, some words (e.g., BLUE and RED) were presented most often in their congruent color (high PC), while others (e.g., GREEN and YELLOW) were presented most often in an incongruent color (low PC). A larger congruency effect was observed for high, relative to low, PC items. The conflict adaptation account suggests that PC effects are due to modulation of attention to the word as a reaction to the general conflict level in the task as a whole, but given that high and low PC trials are intermixed in the item-specific task, one cannot know in advance whether one needs to attend or not attend to the word. Therefore, it cannot explain item-specific effects. Learning accounts conversely propose that the cognitive system learns how to respond to a specific condition, thus explaining item-specific proportion effects. Therefore, conflict adaptation does not have to be assumed.

Given this problem, Blais et al. (2007; see also Verguts and Notebaert, 2008, 2009; Blais and Verguts, 2012) proposed that learning processes may contribute to conflict adaptation. In particular, Blais et al. (2007) modified Botvinick et al.'s (2001) model by allowing the modulation to be condition-specific, that is, by modulating the connections between a specific condition [e.g., the stimulus color (Bugg et al., 2008) or location[2] (Crump et al., 2006)] and the required response according to their co-occurrence. Such a model allows for simulating ISPC effects. Verguts and Notebaert (2008, 2009) noted that the conflict monitoring model (Botvinick et al., 2001) and the Blais et al. (2007) version of it clearly specify when extra control should be exerted, but not where. In particular, response conflict warns the cognitive system that it should be attentive, thus specifying when it should be activated. The conflict monitoring model further postulates that this is done by increasing activation of the currently relevant task representation (task demand unit). However, how the control system knows which stimuli are more conflicting than others [e.g., how the systems knows that red and blue are mostly congruent (MC), while yellow and green are mostly incongruent (MI), i.e., where to intervene] is left unspecified. As a solution to this problem, Verguts and Notebaert (2009) proposed a model in which Hebbian learning provides the mechanism for binding specific stimulus–response combinations, suggesting that the modulation of cognitive control might be the result of interactions between arousal and online learning processes.

While flexible conflict adaptation models (Blais et al., 2007; Verguts and Notebaert, 2009) as presented above can provide an explanation for ISPC effects by showing that control adaptation can be applied under specific conditions, such effects can be also explained by a pure stimulus–response (S–R) contingency-learning mechanism (Schmidt et al., 2007, 2010; Schmidt and Besner, 2008; see also Mordkoff, 1996). According to the contingency-learning account, participants learn the associations between certain words and certain responses and thus frequently appearing conditions are responded to much quicker. In tests of this account, as in an ISPC experiment, one set of words is presented mostly in their congruent color (e.g., GREEN written in green), and another set is presented mostly in their incongruent color (e.g., BLUE written in yellow), and thus participants are able to associate certain words with certain responses (e.g., the word green with the response green, the word blue with the response yellow).

Schmidt and Besner (2008) claimed that the standard PC experiments confound item-specific and list-level[3] PC effects. Because all stimuli are presented most often in their congruent color in the MC condition, and most often in their incongruent color in the MI condition, these contingency biases are capable of producing a PC effect on their own. Bugg and Chanani (2011; see also Bugg et al., 2011), however, proposed that both contingency learning and conflict adaptation may contribute to the proportion effect in the Stroop task. They demonstrated that list-wide proportion effects cannot be fully explained by item-specific mechanisms (cf. Hutchison, 2011). Bugg and Chanani suggested that participants may not implement list-wide control when associative learning provides a reliable and efficient means for responding (e.g., Bugg et al., 2008; Blais and Bunge, 2010). In their word-picture Stroop experiment, they increased the set of possible responses on incongruent trials, that is, they generated more contingent response options, which made associative learning less effective. The researchers found a list-level proportion congruency effect for 50% congruent items, showing that list-level proportion congruency effects could be observed independently of ISPC effects. Bugg (2014) demonstrated similar results in a color-word Stroop task. In her study, participants showed no evidence of increased control in high relative to low conflict context when they were able to rely on item-specific S–R associations to respond to the majority of trials (Experiments 1B and 2A). By contrast, when this was not a reliable approach, due to there being multiple possible responses on incongruent trials (i.e., a four-item biased set), increased use of control was observed in the high relative to the low conflict context (PC effect in Experiments 1A and 2B).

Recently, Schmidt (2013a) proposed a temporal learning-based explanation of list-wide PC effects. According to his proposal, participants may learn **when** to respond in specific conditions rather than what to respond (i.e., contingency learning). Such *temporal learning* results in biasing response retrieval times in specific conditions. Namely, in the high PC conditions, congruent trials are responded to faster, so the high frequency of quick responses leads participants into a rapid pace of responding to congruent trials, with a penalty to the infrequent incongruent trials. In contrast, in

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[2] Blais et al. (2007) did not simulate the context-specific proportion congruent (CSPC) effect, but we believe this could probably be done in their model.

[3] The term list-level PC (in contrast to item-specific PC) refers to a situation in which the congruent to incongruent ratio is held constant for all color words included in a given block.

the low PC conditions, most previous responses are slow, leading to a slower expectancy. Incongruent trials benefit from the slower expectancy, thus leading to a smaller Stroop effect.

THE PRESENT STUDY

The associative learning accounts that propose learning-based modulation of conflict adaptation (i.e., Blais et al., 2007; Verguts and Notebaert, 2008, 2009; Blais and Verguts, 2012), as well as those proposing that (contingency) learning alone can account for the PC and the ISPC effects (Schmidt et al., 2007, 2010; Schmidt and Besner, 2008; Schmidt, 2013b), suggest that gaining experience with the stimuli (S–R) relations is crucial in order to learn. As described earlier, the ISPC effect was examined in order to reveal the contribution of learning. We examined if gaining experience is a necessary condition for the PC effect. The conflict monitoring model (Botvinick et al., 2001), as well as the computational model of Blais et al. (2007), does not specify where extra control has to be exerted. It does suggest, however, that once the system knows there is a conflict, it recruits control. This allows us to assume that "knowing" may be enough and the actual exposure to the task (i.e., experiencing the S–R relations) is not always necessary to generate control. Therefore, showing that PC effects can be observed as a function of experimental instructions alone, when participants receive no practice, is evidence of the existence for control recruitment in the absence of learning.

The idea that instructions alone are not enough in order to learn is supported by Schmidt and De Houwer's (2012) findings. These researchers aimed to reveal whether contingency awareness resulting from instructions can aid performance in an implicit learning task, such as the color-word contingency learning task. In their second experiment, three **color-unrelated** words were presented most often in a particular color (e.g., "plate" most often in green, "month" most often in red, and "under" in yellow). In addition, they manipulated the experimental instructions. Half of the participants were told the word-color contingencies in advance and half were not. The researchers showed that when the contingency instructions were given, but no contingencies were actually present, no contingency effect was found. By contrast, Meiran et al. (2012) proposed recently that application of novel plans that have never been executed before is not only possible but may actually represent the typical scenario of control adaptation. Similarly, Verbruggen et al. (2014) proposed that participants are able to derive action rules from instructions and immediately perform a task that they have never done before as a prepared or intention- based reflex.

Based on these findings, the aim of the present study was to distinguish between the effects of control recruitment by instructions (henceforth *control by instructions*) and those of learning.

EXPERIMENT 1

We aimed to differentiate between control by instructions and learning by manipulating the information given at the outset of the experiment. Half of the participants were informed that most of the stimuli would be congruent, whereas the other half were informed that most of the stimuli would be incongruent. This held true for the stimuli in the second part of the experiment, however, in the first part the proportion of the two stimulus types was equal; therefore it was impossible to learn the proportions during the first part. If control by instructions does exist, we would expect to find a significant PC effect from the very beginning of the experiment. If the PC effect also reflects learning, the effect should be larger in the second part of the experiment. Finally, if PC is caused exclusively by learning, it should appear only in the second half of the experiment.

METHOD

Participants

Twenty-eight students at Ben-Gurion University of the Negev, who were native speakers of Hebrew, participated in the experiment. All had normal or corrected-to-normal eyesight. Participation in the experiment was in partial fulfillment of course requirements. All participants gave written informed consent. The experiment was approved by the ethics committee of the Psychology Department at Ben-Gurion University of the Negev.

Stimuli

We used four colors in the experiment: red, green, blue, and yellow. The name of each of these colors in Hebrew consists of four letters. We generated the congruent stimuli by printing each of the four color names in its own color. We generated the incongruent stimuli by printing each color name in ink colors of the three other colors. The stimuli were presented on a 17″ widescreen CRT monitor with a resolution 1024 × 768, in bold-faced 18-point Courier New font. Data collection and stimuli presentation were controlled by E-Prime software (Psychology Software Tools, Pittsburgh, PA, USA) on a Dell computer with an Intel Pentium 4 central processor. The two types of stimuli (congruent or incongruent) were randomly ordered.

Design and procedure

We created two experimental conditions. Half of the participants were informed that most of the stimuli would be congruent, whereas the other half were told that most of the stimuli would be incongruent. This held true for the stimuli in the second part of the experiment [congruent to incongruent ratio (C/I) = 80/20 or 20/80 in accordance with the instructions given] but in the first part the proportion of the two stimulus types was equal; therefore it was impossible to learn the proportions during the first part (C/I ratio = 50/50). Fourteen participants were randomly assigned to each of the experimental conditions. Participants were tested individually. At the beginning of the experiment, the task was explained to the participants, who were asked to ignore the written word and name the ink color as fast as possible without making errors. Depending on the experimental group they were allocated to, participants were informed what the distribution of the stimuli to be presented would be: (English translation) *In this experiment, you will see congruent (the word and the ink color are congruent, e.g., RED printed in red) and incongruent (the word and the ink color are incongruent) stimuli. Note that most of the stimuli will appear as congruent (/incongruent) stimuli.*

There were no practice trials and no breaks between the two experimental parts. Each experimental part consisted of 120 trials.

Table 1 | Experimental design.

Experimental group	Instructions	Part	Congruent to incongruent ratio	Number of trials	Total
1	Most stimuli congruent	1	50/50	120	240
		2	80/20	120	
2	Most stimuli incongruent	1	50/50	120	240
		2	20/80	120	

The participants sat opposite to the display screen. The stimuli were presented in the center of the screen, at ~80 cm from the participant's eyes. Each trial began with a fixation point presented for 500 ms (a white plus sign at the center of a black screen). After that, the stimulus appeared and remained in view until the participant's response into a microphone, which stopped the timer and removed the stimulus from the screen. Reaction time (RT) in milliseconds was measured by the computer from the stimulus onset until the participant's response. A keypress by the experimenter initiated the next trial. Responses were scored as errors if the initial consonant sound indicated a color other than that of the current trial. The experimenter typed in the vocal response of the participant on one of four keys so that the computer could evaluate errors.

The instructions given to participants ("most of the stimuli would be congruent/ incongruent") were manipulated between participants. Stimulus type (congruent or incongruent) and the part of the experiment (first or second) were manipulated within participants (see **Table 1** for details). RT was the main dependent variable in the experiment.

RESULTS AND DISCUSSION

For each participant, mean RTs of correct responses and of the percentage of errors (PEs) in each experimental condition were calculated. RTs of error trials were omitted (less than 2% of all responses) as were RTs slower than 2,500 ms and faster than 250 ms. All effects were tested at a significance level (α) of 0.05.

A three-way ANOVA (analysis of variance) mixed-factor model with stimulus type and part of the experiment as within-participant factors, and type of instructions as a between-participant factor, revealed a significant main effect for stimulus type, $F(1,26) = 104.2$, MSE = 3,107, $\eta_p^2 = 0.8$. The two-way interaction between stimulus type and instructions was significant, $F(1,26) = 24.65$, MSE = 3,107, $\eta_p^2 = 0.49$, as was the three-way interaction between the stimulus type, part and the instructions, $F(1,26) = 12.36$, MSE = 1,250, $\eta_p^2 = 0.32$ (see **Figure 1**).

Further analysis revealed significant simple interactions between instructions and stimulus type, both in part 1 where the proportions of the color words were equal, $F(1,26) = 11.16$, MSE = 1,039.87, $\eta_p^2 = 0.3$, and in part 2 where we changed the proportions in accordance with the instructions, $F(1,26) = 24.24$, MSE = 3,317.04, $\eta_p^2 = 0.48$, revealing smaller Stroop effects in the C/I = 20/80 conditions (142 ms for MC vs. 84 ms for MI in part 1, and 178 ms for MC vs. 26 ms for MI conditions in part 2).

In the current design, the part of the experiment and the proportions that the participants were exposed to were confounded; the first and second experimental parts differed not only in proportions but also in length of exposure to experimental stimuli and in the amount of fatigue experienced by the participant. To test the hypothesis that the time location in the experiment *per se* influenced behavior, were analyzed the first part of the experiment after adding location within the first part (i.e., first vs. second half) to the design. Splitting the first part did not moderate the effects of congruency, instructions or their interactions ($F < 1$). This finding supports the claim that the increase in the PC effect (by 94 ms) in the second part of the experiment is due to learning. As one can see in **Figure 1**, responses for congruent stimuli were 46 ms faster when most of the stimuli were congruent, $F(1,26) = 13.3$, MSE = 922.34, $\eta_p^2 = 0.34$, and incongruent stimuli were 106 ms faster when most of the stimuli appeared as incongruent, $F(1,26) = 6.47$, MSE = 12,102, $\eta_p^2 = 0.20$.

The error rate was very low, averaging 1.24%. A three-way repeated measures ANOVA with stimulus type and part as within-participant factors, and type of instructions as a

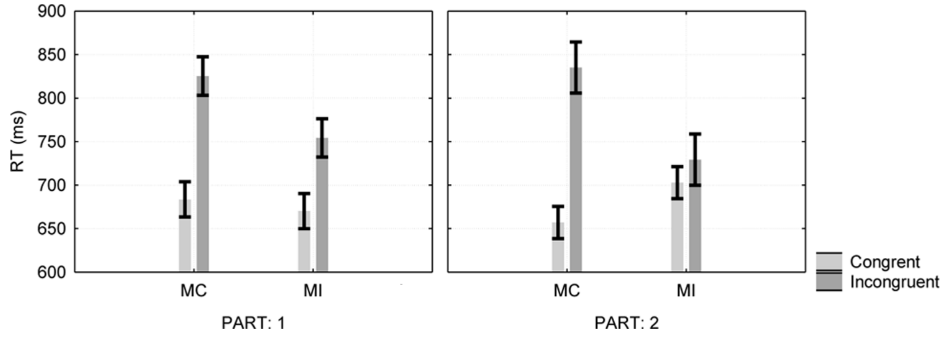

FIGURE 1 | Proportion congruent effect as a function of instruction type and experimental part in Experiment 1. Error bars are one standard error of the mean. MC, mostly congruent; MI, mostly incongruent.

between-participant factor, revealed a single significant main effect for stimulus type, $F(1,26) = 8.45$, MSE $= 2.97$, $\eta_p^2 = 0.24$, showing more errors for the incongruent stimuli.

These results, as those proposed by Bugg (2014; see also Bugg and Chanani, 2011), suggest that both learning and control adaptation via mere instructions may contribute to the proportion effect in the Stroop task. Namely, our results show that in addition to stimulus–response associative learning, control contributes to the proportion effect.

EXPERIMENT 2

Experiment 1 demonstrated that pro-active control can be activated by instructions in the absence of learning. In the first experiment we manipulated the list-level proportions (i.e., most of the stimuli appeared as congruent or incongruent trials), but we did not change the item-specific proportions. Therefore, the purpose of our second experiment was to discover whether the ISPC effect could be observed as a function of experimental instructions alone, thus indicating item-level control recruitment.

We manipulated the item-specific proportions, generating two color sets–color words presented mostly in their congruent color, and color words presented mostly in their incongruent color. Participants were informed about the exact word-color contingencies in advance. Similar to Experiment 1, this held true only for the second part of the experiment, while in the first experimental part the item-specific proportion was equal. Therefore it was impossible to learn the proportions during the first part. The list proportion congruency was held constant. If item-level control by instructions does exist, we would expect to find a significant ISPC effect from the very beginning of the experiment. If the ISPC effect also reflects learning, the effect should be larger in the second part of the experiment. Finally, if the ISPC effect is caused exclusively by learning, it should appear only in the second half of the experiment.

METHOD

Participants

Thirty-four students at Ben-Gurion University of the Negev, who were native speakers of Hebrew, participated in the experiment. All had normal or corrected-to-normal eyesight and had not participated in Experiment 1. Participation in the experiment was in partial fulfillment of course requirements. All participants gave written informed consent. The experiment was approved by the ethics committee of the Psychology Department at Ben-Gurion University of the Negev.

Stimuli and procedure

We used two sets of color words (i.e., red and blue vs. yellow and green). In the first part of the experiment, each color word was presented 30 times and the item-specific proportion was equal for the two sets (e.g., red and blue were presented half of the time in their congruent color (i.e., in 15 trials each) and the other half in their incongruent color). In the second part, we varied the item-specific proportions: for the first set, each color word was presented in its congruent color in 24 trials (80%) and in the other color from that set in 6 trials (20%) to produce the MC condition. For the second set, these rates were reversed to produce the MI condition (i.e., each color word was presented as an incongruent stimulus in 24 trials and as a congruent stimulus in 6 trials). Thus, overall, in the experiment there were 120 congruent trials and 120 incongruent trials (60 congruent and 60 incongruent stimuli in each block), with each color and color-word appearing equally often, while the item-specific proportion was changed only in the second experimental part. Assignment of color sets to the MC and MI conditions was counterbalanced across participants.

At the beginning of the experiment, the task was explained to the participants, who were asked to ignore the written word and name the ink color as fast as possible without making errors. In addition, participants were informed what the distribution of the stimuli to be presented would be. As in Experiment 1, they received instructions telling them of the word-color contingencies involved in the task. One group was instructed that red and blue would appear mostly as congruent stimuli, while yellow and green would appear mostly as incongruent stimuli: (English translation) *In this experiment, you will see color words printed in colors. Note that RED and BLUE will appear mostly as congruent stimuli (the word and the ink color are congruent), while YELLOW and GREEN will appear mostly as incongruent (the word and the ink color are incongruent) stimuli.* The other group was instructed exactly the opposite, that is, that yellow and green would appear mostly as congruent stimuli while red and blue would appear mostly as incongruent stimuli. There were no practice trials and no break between the two experimental parts. Each experimental part consisted of 120 trials.

Three independent variables—condition (MC or MI), stimulus type (congruent or incongruent), and experimental part (1 or 2)—were manipulated between participants (see **Table 1** for details). RT was the main dependent variable in the experiment.

RESULTS AND DISCUSSION

For each participant, mean RTs of correct responses and of the PE in each experimental condition were calculated. RTs of error trials were omitted (less than 1% of all responses) as were RTs slower than 2,500 ms and faster than 250 ms. All effects were tested at a significance level (α) of 0.05.

A three-way repeated measures ANOVA with condition, stimulus type, and part as within-participant factors revealed a significant main effect for stimulus type, $F(1,33) = 151.2$, MSE $= 4{,}707$, $\eta_p^2 = 0.82$. The two-way interaction between condition and stimulus type was significant, $F(1,33) = 29.53$, MSE $= 1{,}516$, $\eta_p^2 = 0.47$, as was the three-way interaction between the condition, stimulus type and part, $F(1,33) = 28.9$, MSE $= 1{,}412$, $\eta_p^2 = 0.47$ (see **Figure 2**).

The first part of the experiment revealed no difference between the congruency effects in the two proportion conditions, $F < 1$. This finding implies that informing the participants about the word-color contingencies without giving them the opportunity to learn the S–R relations was not enough to produce the ISPC effect. In order to reassure this additive pattern, we also computed the Bayesian posterior probabilities (see Wagenmakers, 2007; Campbell and Thompson, 2012).

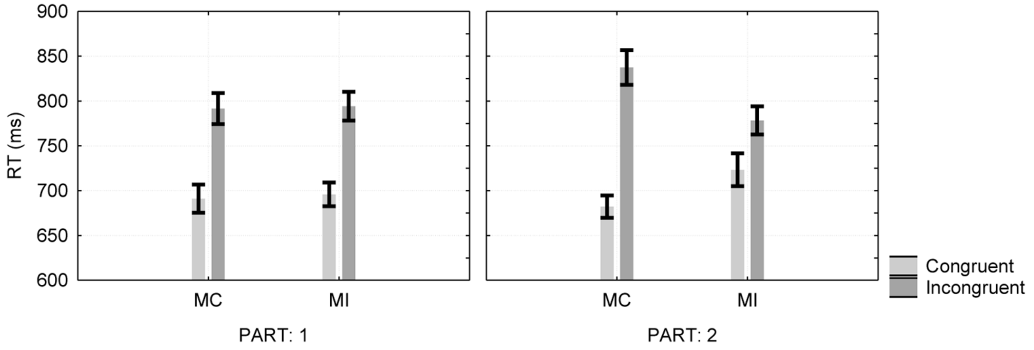

FIGURE 2 | Item-specific proportion congruent effect as a function of instruction type and experimental part in Experiment 2. Error bars are one standard error of the mean. MC, mostly congruent; MI, mostly incongruent.

We estimated the posterior probabilities of p (H_0 | D; i.e., the posterior probability of null effect of instructions) and of p (H_1 | D; i.e., the posterior probability that instructions were enough to modulate control) as 0.85 and 0.15 for H_0 and H_1 respectively, leading to dBIC of 3.5, which according to Campbell and Thompson (2012), is substantial evidence for H_0. Thus, it is apparently not enough to provide information that processing the meaning of stimuli in specific colors is harmful to performance.

In contrast to the first part of the experiment, we observed a significant simple interaction between condition and stimulus type in the second part, where we changed the specific item proportions in accordance with the instructions, revealing a significant ISPC effect, $F(1,33) = 38.14$, MSE = 2,242.52, $\eta_p^2 = 0.54$. Further analysis revealed faster responses (by 41 ms(for the congruent stimuli in the MC condition, $F(1,33) = 9.04$, MSE = 3,178.98, $\eta_p^2 = 0.22$, and faster responses (by 59 ms) for the incongruent stimuli in the MI condition, $F(1,33) = 36.04$, MSE = 1,653.08, $\eta_p^2 = 0.52$.

The error rate was very low, averaging 1%. A three-way repeated measures ANOVA with item type, stimulus type, and part as within-participant factors revealed faster responses for congruent stimuli, $F(1,33) = 33.65$, MSE = 4.24, $\eta_p^2 = 0.5$. The two-way interaction between item type and stimulus type was significant, $F(1,33) = 6.4$, MSE = 3.96, $\eta_p^2 = 0.16$, revealing more errors for incongruent stimuli in the MC condition rather than in the MI condition, $F(1,33) = 33.65$, MSE = 4.73, $\eta_p^2 = 0.13$. Less errors were also observed for congruent trials in the MC condition than in the MI condition, however, the difference was not significant, $F(1,33) = 1.64$, MSE = 2.6, $\eta_p^2 = 0.05$.

Observing a significant ISPC effect when item-specific proportions are varied, resulting in color-word contingency, implies that this effect reflects associative learning. Therefore our results support the notion that learning processes are important (e.g., Schmidt and Besner, 2008; Verguts and Notebaert, 2008, 2009), showing that in contrast to the PC effect, the ISPC effect is not observed in the absence of learning, that is, experiencing S–R contingencies is crucial in order to be able to adapt performance.

GENERAL DISCUSSION

In our first experiment, we informed half of the participants that most of the stimuli would be congruent, whereas the other half were told that most of the stimuli would be incongruent. This held true for the stimuli in the second part of the experiment, but in the first part the proportion of the two stimulus types was equal. A significant PC effect was found in both parts of the experiment, but it was larger in the second experimental part. In our second experiment, we manipulated the item-specific proportions, while the list-level proportion was held constant. Participants were told word-color contingencies in advance but the information, similar to in Experiment 1, was valid only for the second experimental part. In contrast to the list-wide PC effect in Experiment 1, in Experiment 2 the ISPC effect was found only in the second experimental part, where we varied the item-specific proportions.

Our results showed that instructions may be enough to trigger list-level control, thus supporting Cohen-Kdoshay and Meiran (2009) who showed that instructions can be implemented with a high degree of accuracy even on the very first trial. This raises an important question: *How do instructions influence behavior?* Recently, Ramamoorthy and Verguts (2012; see also Doll et al., 2009) suggested a possible computational model of applying instructions. In their model they distinguish between instructions leading to rule-based "learning" and actual exposure to the task stimuli. According to the model, instructions are acquired [apparently by the prefrontal cortex (PFC)] before the actual exposure to the task. Upon repeated application, the basal ganglia (which learn more slowly but execute more quickly) pick up the appropriate stimulus–response mapping by Hebbian learning.

It is important to note that in contrast to our first experiment, in Experiment 2 the ISPC effect was found only in the second experimental part, showing that this effect cannot be produced solely by prior information about the word-color contingencies. We assume that a possible reason for this difference can lay in the difficulty of the instructions. Learning all the item-specific pairings in the ISPC task is much harder than learning "most of the stimuli will be presented in their congruent/incongruent color," therefore, in our first experiment adapting performance via instructions was much easier than in our second experiment and also in Schmidt and De Houwer's (2012) study. This explanation is line with Meiran et al.'s (2012) claim that automatic applications of novel (never executed before) plans are possible only if the task instructions are not too complicated.

To conclude, our results shed new light on the relations between control and associative learning, showing that both processes can take part in the modification of the Stroop phenomenon. We show that under specific (relatively simple) conditions, practice is not necessary for the emergence of the PC effect, from which it follows that control adaptation may lead to such effects. While the conflict monitoring model (Botvinick et al., 2001) and all its later versions (e.g., Blais et al., 2007) do not show how conflict adaptation can be activated by instructions alone, such models could be easily extended to include adaptation on the basis of instructions as shown by Cole et al. (2010) and Ramamoorthy and Verguts (2012). Furthermore, it could also be argued that the increase in the PC effect in the second part of Experiment 1 can also be explained by conflict adaptation, assuming that changes in the color-word contingencies change the actual experienced level of conflict, thereby increasing the PC effect. This, however, cannot explain the results of Experiment 2 without assuming a learning mechanism that directs attention to high conflict conditions (e.g., Hebbian learning as suggested by Verguts and Notebaert, 2008). Thus, it seems that several mechanisms are involved in the PC effect.

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The role of visual awareness for conflict adaptation in the masked priming task: comparing block-wise adaptation with trial-by-trial adaptation

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This study investigated the role of participants' visual awareness in the block-wise and the trial-by-trial adaptations. We employed a subliminal response compatibility task in which a prime arrow was briefly presented before the target arrow, and the participants were requested to indicate the direction of the target arrow. The direction of the prime and direction of the target were either the same (compatible trial) or different (incompatible trial). To examine block-wise adaptation, two blocks were conducted, i.e., the Neutral block (50% compatible and 50% incompatible trials) and the Incompatible block (10% compatible and 90% incompatible trials). The results showed the existence of the block-wise adaptation without participants' visual awareness. The compatibility effect on both response time and error rate (ER) was smaller in the Incompatible block than in the Neutral block. Moreover, a separate data analysis based on the preceding trial type revealed that the trial-by-trial adaptation of cognitive control was observed only in the ER. These results suggest the different role of visual awareness in the block-wise and trial-by-trial adaptations.

Keywords: cognitive control, subliminal priming, conflict adaptation, proportion congruency, contingency learning

INTRODUCTION

Cognitive control is one of the most important cognitive functions humans have for environmental adaptation. By employing response compatibility tasks in a laboratory setting, we are able to examine the process of selecting an appropriate stimulus and guiding participants to an optimized behavior. For example, in the flanker task (Eriksen and Eriksen, 1974), response time (RT) is generally faster when the central target is surrounded by compatible flankers (e.g.,<<<<<) as opposed to incompatible flankers (e.g.,>><>>). The difference between RTs (RT in the incompatible trials minus RT in the compatible trials) is called the "compatibility effect" and regarded as an index of efficiency of conflict solving (Nieuwenhuis et al., 2006; Verguts and Notebaert, 2009).

Amazingly, the compatibility effect is known to be modulated by the task context. Earlier studies reported a block-wise context effect in the response compatibility task. In a block with a larger number of incompatible trials (e.g., 90% incompatible and 10% compatible), the compatibility effect becomes very small and sometimes changes its direction to yield a reverse compatibility effect (Logan and Zbrodoff, 1979; Lindsay and Jacoby, 1994). Furthermore, a trial-by-trial sequential analysis revealed that the compatibility effect is smaller when the preceding trial type is incompatible vs. compatible (Gratton et al., 1992; Stürmer et al., 2002), while the compatibility effect is clearly observed for all the data in a block.

To completely understand these adaptation mechanisms, recent studies have highlighted two issues: (1) differences between block-wise and trial-by-trial adaptations (Braver, 2012) and (2) the role of the participant's awareness in these adaptations (Desender and Van den Bussche, 2012; Schmidt and de Houwer, 2012; Desender et al., 2013). In regard to the first issue, our recent study, which investigated the false alarm response in no-go trials in the Simon task, demonstrated the difference between these adaptations in the process of task-irrelevant information (Hasegawa and Takahashi, 2013). In the experiments, a red, green, or gray disk was presented on either the left or right side of a monitor. Participants were requested to respond to a red or green disk by pressing assigned keys while ignoring its location (normal Simon trials) and to refrain from responding when a gray disk was presented (no-go trial). When the trial-by-trial context was examined, the overall rate of the false alarm response (key-pressing for the gray disk) was lower when the no-go trial was immediately preceded by the incompatible trial compared to the compatible trial, suggesting the enhancement of the inhibition of the task-irrelevant process. This result is well explained by the conflict monitoring theory (Botvinick et al., 2001, 2004), which emphasizes that experience of conflict in one trial boosts inhibition of the task-irrelevant response activation in the next trial. More specifically, the trial-by-trial adaptation is explained as a feedback-loop of the conflict detection and the top-down control demand, and when the conflict is detected in the current trial, the top–down control demand in the next trial is assumed to be strengthened.

Conversely, when the block-wise context was manipulated, a utilization of the task-irrelevant information rather than its inhibition was suggested. This was shown by the fact that when a block contained a larger number of incompatible trials, the opposite-side false alarm (i.e., to respond with the right hand to a gray disk presented on the left side and vice versa) occurred more frequently than the same-side false alarm, whereas the overall false alarm rate was not changed by the manipulation of the block-wise context. These incompatible results for the trial-by-trial and the block-wise contexts suggest that the block-wise adaptation is not a simple accumulation of the trial-by-trial adaptation, contrary to a presumption of the conflict monitoring theory. We have argued that the contingency learning model (Schmidt, 2013) would better explain our results of the block-wise adaptation; participants would learn contingency between the correct response and the task-irrelevant location information to make reactive bias of responding with the hand opposite to the stimulus location (Hasegawa and Takahashi, 2013).

Next, as for the second issue, there is no direct evidence supporting the role of awareness for cognitive control in the response conflict task. Though the effect of block-wise adaptation has been often argued to be a conscious control, our abovementioned study suggested that block-wise adaptation is achieved in an unconscious manner (Hasegawa and Takahashi, 2013). It was shown that our participants were not aware of the utilization of the task irrelevant information, and furthermore, they were not aware of the proportion of the trial types (compatible vs. incompatible) that was manipulated in two blocks.

In relation to the trial-by-trial adaptation, there have been some investigations of the role of awareness, but the results are incompatible with each other. For example, Kunde (2003) showed an absence of trial-by-trial adaptation in the unconscious response conflict task. In his study, a task-irrelevant priming arrow was presented for 14 ms before presenting the target arrow, which ensured that the pointing direction of the priming arrow was not discriminable. The results showed that in the incompatible trials (i.e., the priming arrow pointed in the opposite direction of the target arrow), RT was longer and error rate (ER) was larger than in the compatible trials (both arrows pointed to the same direction). Moreover, this compatibility effect was not influenced by a trial sequence, thereby suggesting the necessity of the awareness of conflict for the trial-by-trial adaptation. However, more recent studies using the same task did show the trial-by-trial adaptation of ER (van Gaal et al., 2010; Francken et al., 2011) and RT (van Gaal et al., 2010). These conflicting results (**Table 1**) demonstrate that the role of awareness in trial-by-trial adaptation remains unclear.

Table 1 | Summary of unconscious trial-by-trial adaptation effects in previous studies.

	RT adaptation		ER adaptation	
Kunde (2003)	–2 ms	n.s.	1.3 %	Not analyzed
van Gaal et al. (2010)	9 ms	**	1.7 %	*
Francken et al. (2011)	1 ms	+	4.1 %	****

$^+p < 0.10$, $^*p < 0.05$, $^{**}p < 0.01$, $^{****}p < 0.001$.

This study aims to reveal the role of visual awareness in the block-wise and trial-by-trial adaptations. Therefore, we employed the subliminal response conflict task (Kunde, 2003), and compared the compatibility effects between the Neutral block (including 50% incompatible and 50% compatible trials) and the Incompatible block (including 90% incompatible and 10% compatible trials). In addition, data were analyzed separately for trials immediately followed the compatible trial and trials immediately followed the incompatible trial to examine the effect of the trial-by-trial adaptation.

MATERIALS AND METHODS

PARTICIPANTS

Twenty volunteers (10 females and 10 males, 19–28 years of age, $M = 22.0$) participated. All reported having normal or corrected-to-normal vision. All participants provided written informed consent. They provided permission for their data to be used in the analysis.

APPARATUS AND STIMULI

The stimuli were displayed on a CRT monitor (Sony GDM-F520) controlled by a computer (Apple MB324J/A) and Psychophysics Toolbox (Brainard, 1997). The monitor refresh rate was 100 Hz.

In the masked priming task, a left-pointing or right-pointing arrow-like figure was used as the prime and the target. The prime was $0.7° \times 1$, and the target was $1.7° \times 2.4$ of visual angle. The prime fitted exactly into the space in the middle of the target (**Figure 1**). The mask used in the prime discrimination task was depicted as an overlapped figure of the left-pointing and right-pointing targets.

PROCEDURE

In the masked priming task, the prime was presented for 10 ms, and then, following a blank interval of 20 ms, the target was presented for 120 ms. Participants were requested to press, as quickly and accurately as possible, the "F" key on a keyboard with the left index finger for the left-pointing target, and a "J" key with the right index finger for the right-pointing target. The next trial was started after a response was made or 2000 ms passed without a response. The length of the inter-trial-interval (ITI) was varied randomly within a range of 1400–1700 ms (**Figure 1**).

Participants performed two separate blocks (Neutral and Incompatible), which each consisted of 320 experimental trials. The Neutral block had 160 compatible trials (the left-pointing target followed the left-pointing prime or the right-pointing target followed the right-pointing prime) and 160 incompatible trials (the left-pointing target followed the right-pointing prime or the right-pointing target followed the left-pointing prime). The Incompatible block had 32 compatible trials and 288 incompatible trials. Participants were not provided with any information about the presentation of the prime and the proportion of compatible/incompatible trials in each block. The block order was fixed for all participants. The Neutral block was performed first. This was because the possible biased effect of the Incompatible block should not be carried over into the Neutral block. The trial order in each block was randomized among participants. After completing the second block, they were

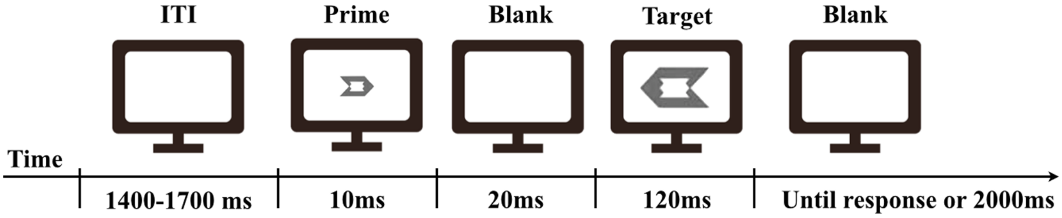

FIGURE 1 | Timeline of the experimental trial.

questioned whether they noticed any difference between the two blocks.

Next, the prime discrimination task was conducted. Before beginning the task, the participants were informed that a prime was briefly presented before the target in each trial of the masked priming task they had just completed. Then, in the prime discrimination task, the prime was presented for 10 ms, and, following a blank interval of 20 ms, the mask was presented for 120 ms. Participants were requested to answer the pointing direction of the prime (a two-alternative forced choice between left and right). Forty trials (20 left and 20 right) were provided in a random order to each participant.

RESULTS

PRIME DISCRIMINATION TASK

The mean correct response rate in the prime discrimination task was 51.75%, which was not significantly different from the chance level (50%), $t(19) = 1.017$, $p = 0.322$, $d = 0.346$. This ensures that the pointing direction of the prime was not discriminable and the employed masked priming task worked properly as a subliminal conflict task. Furthermore, this is also supported by the result of the post-task interview concerning the participants' noticing of any difference between the two blocks; that is, none of them pointed out the difference between the blocks.

RESPONSE TIME AND ERROR RATE IN THE MASKED PRIMING TASK

In the analysis of RT data, trials that elicited an incorrect response were excluded. In addition, the criterion for the outliers was set at ± 2.5 SD of mean RT in each participant; however, actually, there was no outlier in the whole data.

Mean RT in the masked priming task (**Table 2**) was analyzed by a three-way repeated measures analysis of variance (ANOVA) with the block type (Neutral and Incompatible), the preceding trial type (compatible and incompatible), and the current trial type (compatible and incompatible). There were a significant main effect of the current trial type, $F(1,19) = 189.06$, $p < 0.001$, $\eta_p^2 = 0.909$, and a significant interaction between the block type and the current trial type, $F(1,19) = 4.88$, $p = 0.040$, $\eta_p^2 = 0.204$. Any other main effects or interactions were not significant. *Post hoc* analysis indicated that RT on the incompatible current trial was longer than RT on the compatible current trial in each block, $p < 0.001$ in the Neutral block; $p < 0.001$ in the Incompatible block. In addition, RT on the incompatible current trial was significantly shorter in the Incompatible block than in the Neutral block, $p = 0.012$, whereas RT on the compatible current trial was not different between blocks, $p = 0.645$.

Mean ER in the masked priming task (**Table 2**) was analyzed by the same three-way ANOVA as in the case of RT. As a result, a three-way interaction was significant, $F(1,19) = 4.801$, $p = 0.041$, $\eta_p^2 = 0.202$. *Post hoc* analyses indicated that ER on the incompatible current trial in the Neutral block was lower when the preceding trial was incompatible than compatible ($p < 0.001$). ER on the incompatible current trial that followed compatible preceding trial was lower in the Incompatible block than in the Neutral block ($p < 0.001$). Finally, ER was lower on the compatible current trial than on the incompatible current trial in all cases of the block type × the preceding trial type; Neutral block × Compatible preceding trial, $p < 0.001$; Neutral block × Incompatible preceding trial, $p < 0.001$; Incompatible block × Compatible preceding trial, $p = 0.003$; Incompatible block × Incompatible preceding trial, $p = 0.005$.

BLOCK-WISE ADAPTATION EFFECT

The compatibility effects in RT (RT on the incompatible trials minus RT on the compatible trials) and ER (ER on the incompatible trials minus ER on the compatible trials) in the Neutral and the Incompatible blocks are shown in **Figure 2**. There was a significant difference in the compatibility effects, both in RT and ER, between the Neutral and the Incompatible blocks, $t(19) = 5.482$, $p < 0.001$, $d = 1.772$ (**Figure 2A**) and $t(19) = 3.139$, $p = 0.005$, $d = 0.629$ (**Figure 2B**), respectively. An adaptation index, which is calculated by subtracting the compatibility effect in the Incompatible block from that in the Neutral block, was 15.00 ms in RT and 4.51% in ER.

TRIAL-BY-TRIAL ADAPTATION EFFECT

Next, to examine the trial-by-trial adaptation, the compatibility effect in RT and ER was calculated separately for trials immediately preceded by the compatible trial and for trials immediately preceded by the incompatible trial in the Neutral block. **Figure 3** shows the effect of the preceding trial type (compatible or incompatible) on the compatibility effects in the Neutral block. The compatibility effect in RT was larger with marginal significance when the preceding trial type was incompatible compared to compatible, $t(19) = 1.860$, $p = 0.079$, $d = 0.299$ (**Figure 3A**). Contrarily, the compatibility effect in ER was significantly smaller when the preceding trial type was incompatible than when it was compatible, $t(19) = 3.253$, $p = 0.004$, $d = 0.402$ (**Figure 3B**). RT adaptation was -5.28 ms, and ER adaptation was 3.74%.

Table 2 | Mean response time (RT) and error rate (ER) in the masked priming task.

		Current Trial			
		Compatible		Incompatible	
		RT (SD)	ER (SD)	RT (SD)	ER (SD)
Neutral Block (50%-Incompatible)					
Preceding trial	Compatible	273.5 (37.4)	1.2 (0.0)	335.4 (37.0)	12.7 (0.1)
	Incompatible	269.7 (35.8)	0.8 (0.0)	336.8 (37.1)	8.6 (0.1)
Incompatible Block (90%-Compatible)					
Preceding trial	Compatible	266.2 (63.5)	0.0 (0.0)	321.7 (37.9)	5.6 (0.1)
	Incompatible	271.9 (29.6)	0.8 (0.0)	321.5 (30.7)	6.0 (0.1)

RT, mean reaction time (ms); ER, error rate (%).

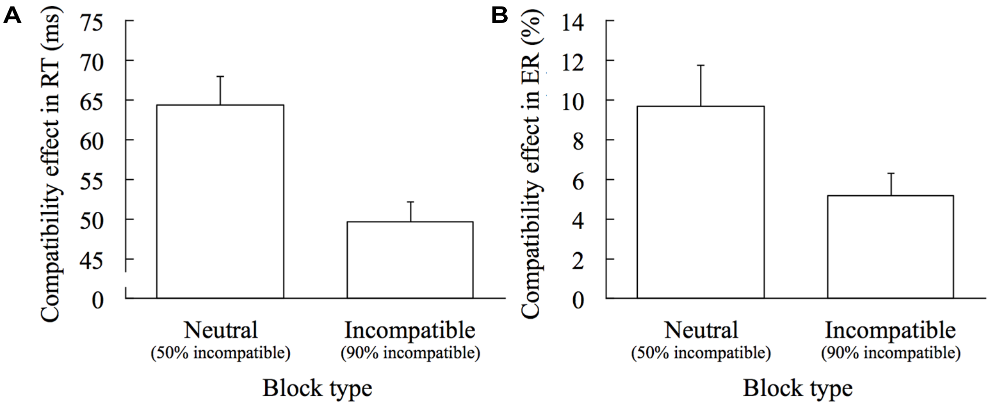

FIGURE 2 | Block-wise adaptation effect. Error bars show 1 SEM. **(A)** Comparing the compatibility effects in RT (RT on incompatible trials minus RT on compatible trials) between the Neutral and the Incompatible blocks. **(B)** Comparing the compatibility effects in ER (ER on incompatible trials minus ER on compatible trials) between the Neutral and the Incompatible blocks.

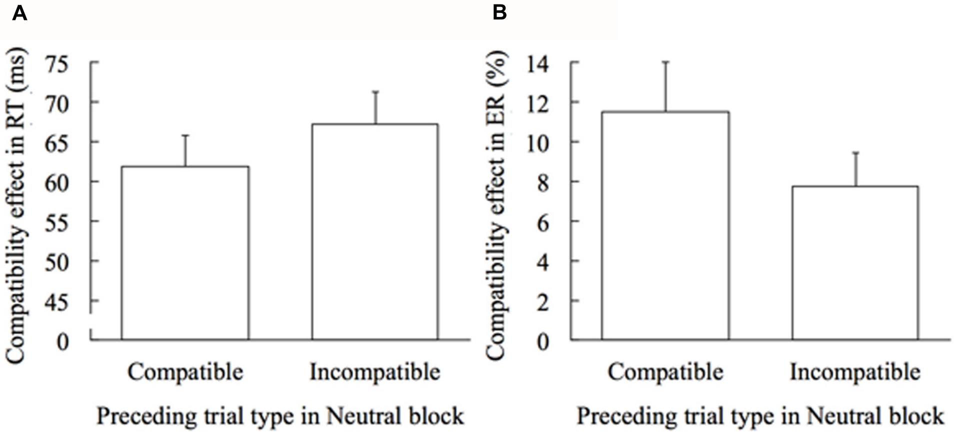

FIGURE 3 | Trial-by-trial adaptation effect in the Neutral block. Error bars show 1 SEM. **(A)** Effect of the preceding trial type on the compatibility effect in RT. **(B)** Effect of the preceding trial type on the compatibility effect in ER.

DISCUSSION

In the present study, we examined the role of visual awareness for the block-wise and trial-by-trial adaptations. Block-wise adaptation was clearly observed; the compatibility effect was smaller in the Incompatible block than in the Neutral block, in both the RT and the ER measures. In addition, the result of the prime discrimination task showed that participants could not discriminate the pointing direction of the prime and the post-task interview revealed that no participants noticed any difference between the blocks, ensuring that the effect of block-wise adaptation on the task performance was derived unconsciously.

These results may give an empirical support to the contingency learning model for the block-wise adaptation, because it has been shown that the contingency learning of proportion congruency can be achieved without awareness (Schmidt et al., 2007). Furthermore, our result is consistent with the evidence of sequence-specific learning effect, which shows that compatibility effect decreases gradually when the order of stimulus-presentation is determined with sequential regularity, though participants did not notice such a regularity (Deroost et al., 2012). These findings, together with our result, suggest that adaptation for the long-term conflict context should progress in an implicit manner without participant's awareness of the conflict

Looking from another angle, the present result would be explained by the Adaptation to the Statistics of the Environment (ASE) model (Kinoshita et al., 2008, 2011), which argues that both the trial-by-trial sequential effect and the proportion effect are driven by the history of trial difficulty. Recent study showed that the conflict awareness could be developed even when visual awareness is absent, and this conflict awareness triggered the conflict adaptation in the masked priming task (Desender et al., 2014). In the present experiment, participants might have felt stronger difficulty in the Incompatible block than in the Neutral block, thereby leading to the conflict awareness. However, although the ASE model is proposed to account for both the trial-by-trial and block-wise effects, the trial-by-trial RT adaptation was not observed in the present results. This suggests that the awareness of stimulus incompatibility between the task-relevant information and the task-irrelevant information is necessary to generate trial-by-trial RT adaptation.

On the other hand, ER data showed that the compatibility effect was changed not only by the block-wise but also the trial-by-trial context. Although some researchers denied the trial-by-trial adaptation in the masked priming task (Kunde, 2003; Ansorge et al., 2010), the present result showed that the trial-by-trial context caused certain change. Nevertheless, because the compatibility effect is likely to be manifested in both RT and ER data, it may be the case that the speed/accuracy trade-off (e.g., post-error slowing) is distinguished from the conflict adaptation (Notebaert and Verguts, 2011). As previously noted, Francken et al. (2011) demonstrated the unconscious trial-by-trial effect only on the accuracy measure (**Table 1**). Furthermore, the results of accuracy reported by Kunde (2003) appear to show a difference between conditions with compatible and incompatible preceding trials, but only through visual inspection of the graph, as the statistical analysis of the data was not provided. Taken together, these results may suggest that the trial-by-trial effect is limited to ER adaptation and does not cause RT adaptation; rather, it may cause an increase of the compatibility effect in RT as in the case of the present result (**Figure 3A**), probably due to speed/accuracy trade-off. Note that the results of other masked priming studies support this view, that is, the trial-by-trial effect on responders' cautiousness has been shown in the masked Go/No-Go task (van Gaal et al., 2008) and in the stop-signal task (van Gaal et al., 2009). These tasks do not require response selection; thus, speed/accuracy trade-off would be sufficient to improve the performance. However, this interpretation needs to be validated by more evidences in the future research.

In summary, the present study investigated the role of awareness in the trial-by-trial and block-wise adaptation to the response conflict. A partial trial-by-trial adaptation (speed/accuracy trade-off) and complete block-wise adaptation (enhancement in both speed and accuracy) were found in the masked priming task. Therefore, we can conclude that the stimulus awareness is not necessary for the block-wise adaptation. The sustained conflict context boosts the conflict resolution even unconsciously. On the other hand, when a response conflict was experienced unconsciously in the preceding trial, the process of conflict resolution would not be completely facilitated, triggering only speed/accuracy trade-off.

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Contingencies and attentional capture: the importance of matching stimulus informativeness in the item-specific proportion congruent task

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The proportion congruent effect is the observation that congruency effects are smaller when the proportion of incongruent stimuli is higher. The conflict adaptation account argues that this effect is due to a shift of attention away from the source of conflict. In contrast, the contingency account proposes that participants learn to predict the likely response on the basis of the distracter, and this produces a proportion congruent effect incidentally. However, some have argued that conflict adaptation can be observed in the restricted scenarios in which the mostly incongruent stimuli are not strongly predictive of the correct response. This opinion article argues that comparing predictive to non-predictive stimuli might be problematic. Some learning research would suggest that attention to the distracter should vary, but for an entirely different reason than that proposed by conflict adaptation theory: contingent stimuli attract attention.

In the Stroop paradigm (Stroop, 1935), participants are tasked with the goal to ignore a distracting color word and to respond to the print color it is presented in. Only partially successful at doing so, participants respond slower and less accurately to incongruent stimuli (e.g., the word "blue" printed in red) than to congruent stimuli (e.g., "blue" in blue). Similar congruency effects are observed in the Simon (Simon and Rudell, 1967), flanker (Eriksen and Eriksen, 1974), picture-word (Rosinski et al., 1975), and various other comparable tasks. Such congruency effects are reduced if the relative number of incongruent trials is increased (Lowe and Mitterer, 1982). This *proportion congruent (PC) effect* is most commonly interpreted as evidence for the *conflict adaptation account* (e.g., Botvinick et al., 2001). This account argues that detection of conflict results in a decrease of attention to the source of conflict (e.g., the word in a Stroop task). Because conflict is more frequent in a mostly incongruent condition, attention to the word is particularly low. The word therefore has little impact on performance, and the congruency effect is resultantly small.

Though seemingly intuitive, the conflict adaptation account does face some important challenges. For instance, consider the item-specific PC effect (Jacoby et al., 2003). In this variant of the PC task, some words are mostly congruent and others are mostly incongruent. These two item types are intermixed into one procedure, but there is nevertheless a smaller effect for mostly incongruent items. This might be described in terms of item-specific adaptations of attention (e.g., Blais et al., 2007), though this requires the unintuitive assumption that attention to the word is determined by the identity of the word (which must, of course, first be identified; but see Verguts and Notebaert, 2008). Alternatively, the *contingency account* proposes that the entire item-specific PC effect is explained by the learning of contingent relationships between distracting words and responses (Schmidt and Besner, 2008; Schmidt, 2013b; for a review, see Schmidt, 2013a). For mostly congruent stimuli, each word is presented most often with the congruent color. This means that, for instance, the word "blue" is predictive of a blue response. Congruent trials thus benefit from this prediction, and the congruency effect increases. For mostly incongruent stimuli, the reverse is true: each word is presented most often with a specific incongruent response. Thus, "red" might be predictive of a yellow response. Incongruent trials thus benefit, and the congruency effect decreases. Some evidence argues compellingly in favor of the contingency account (Schmidt and Besner, 2008; Atalay and Misirlisoy, 2012; Grandjean et al., 2013; Schmidt, 2013b). For instance, Schmidt (2013b) presents a dissociation procedure in which contingency learning and conflict adaptation could be separately assessed. Specifically, it was possible to compare sets of incongruent trials that were: (a) equivalent in PC (mostly incongruent) but that varied in contingency (high vs. low), or (b) equivalent in contingency (low contingency) but that varied in PC (mostly congruent vs. mostly incongruent). Thus, the former set allows an assessment of contingency learning in the absence of conflict adaptation, and the latter set allows an assessment of conflict adaptation in the absence of a contingency bias. These comparisons revealed a very strong contingency effect, with no evidence for conflict adaptation. The (item-specific) PC effect thus might have nothing to do with conflict adaptation at all. Some neuropsychological data even argues that the area claimed to be involved in (item-specific) conflict adaptation (viz., the anterior cingulate cortex; see Blais and

Bunge, 2010) might instead be involved in contingency learning (Grandjean et al., 2013).

On the other hand, many argue that conflict adaptation can be observed independently of contingency biases (e.g., Crump and Milliken, 2009; Blais and Bunge, 2010; Bugg et al., 2011; Abrahamse et al., 2013; Bugg and Hutchison, 2013). One specific claim is that, while contingency learning might dominate performance in some scenarios, conflict adaptation might still be observable in others (Bugg et al., 2011; Bugg and Hutchison, 2013). For instance, Bugg and colleagues argue that if the target is easier to process than the distracter, use of contingencies associated with the distracter might be impaired. The target then might serve as a cue to PC. Of interest for the current discussion, Bugg and Hutchison further argue that in designs where mostly congruent and mostly incongruent stimuli are equally informative, contingencies dominate processing. However, when mostly incongruent stimuli are uninformative this is no longer the case. For instance, if a color word is presented equally often in four colors, then the word is mostly incongruent (i.e., 75% incongruent), but it is not predictive of what to respond (i.e., each of the four responses are equiprobable). According to those authors, weakening the predictiveness of the distracters in this way impairs the contingency mechanism and allows conflict adaptation to play a role. In other words, they argue that conflict adaptation can occur, but only when contingency learning does not "steal the show." In support of this, they found that when mostly incongruent stimuli were as predictive as mostly congruent stimuli, the data fit the predictions of the contingency account. However, when mostly incongruent stimuli were unpredictive and mostly congruent stimuli were (still) predictive, they found a different pattern of results. Specifically, they found mostly interference-driven effects in this scenario, with large impairments of incongruent items in the mostly congruent relative to mostly incongruent condition. In contrast, little differences were observed for congruent items. This is not what the contingency account should predict, especially since the only contingencies present in the task were for *congruent* items. Such a pattern is seemingly more consistent with the conflict adaptation account.

Results such as those in Bugg and Hutchison (2013) are thus quite interesting, because they suggest that conflict adaptation might indeed exist independent of contingency learning biases. However, the key critique of the current article is that the modified task configuration used in such experiments adds an additional complexity to the task. The contingency account was originally proposed to explain the simple scenario in which some words were mostly correlated with the congruent response, whereas other words were mostly correlated with an incongruent response (Schmidt and Besner, 2008). The word is equally informative in both scenarios. Generally speaking, when these same criteria were met in subsequent experiments, evidence for an exclusively contingency-driven account of the data remained compelling (e.g., Bugg and Hutchison, 2013; Schmidt, 2013b). Comparing a *contingent* mostly congruent condition to a *non-contingent* mostly incongruent condition might produce results that seem harder to interpret from a contingency learning perspective, but this might also be like comparing apples to oranges. It is known that a contingency-laden dimension must be attended in order to learn the correlation (e.g., Chun and Jiang, 2001). More importantly, it is also know that when a contingency is detected for a given distracting stimulus, attention is *attracted* to this stimulus (e.g., Chun and Jiang, 1998; Cosman and Vecera, 2014). For instance, Cosman and Vecera presented participants with a red or green cue on the left or right of the screen, followed by two letters in the two possible cue locations. One of the letters was a target, which participants identified, and the other not. Each letter was presented in either red or green. Neither the cue location nor the cue color predicted the color, location, or identity of the target. However, targets were presented most often in one color (e.g., red). As typically observed, responses were faster when the cue location matched the target location, indicating attentional capture of the cue. Most importantly, this attentional capture effect was larger when the cue was the color that the targets were typically presented in. This indicates that a contingent stimulus (e.g., red color) captures attention.

The notion that contingent stimuli attract attention only stands to reason: predictive stimuli in our environment are attended because they can help guide our behavior (see also, Hutcheon and Spieler, 2014). Thus, the suggestion here is that a correlated mostly congruent distracting word will attract more attention than an uncorrelated mostly incongruent one in experiments such as those of Bugg and Hutchison (2013). This is because the distracter is informative of the response in the former condition (i.e., contingent), but not in the latter condition. Thus, changes in attention across these two conditions will indeed lead to larger congruency effects in the mostly congruent condition. Specifically, attending to a mostly congruent word will have a large impairment on incongruent trials, due to an increase in interference. This explains the large impairments of incongruent trials in the (predictive) mostly congruent condition relative to the (non-predictive) mostly incongruent condition observed by Bugg and Hutchison (2013). Indeed, the expected results are the same as those for the conflict adaptation account, because both accounts predict that the congruency effect is modulated by attentional differences. Of course, the difference is what drives those attentional differences: contingencies or conflict.

Note again that differences in informativeness between mostly congruent and mostly incongruent stimuli are not always present in PC experiments (e.g., Jacoby et al., 2003). If words in the mostly incongruent condition are just as predictive of what response to make as in the mostly congruent condition, then informativeness is equated. Thus, attention would not vary across conditions. When Bugg and Hutchison (2013) removed this equality in informativeness between mostly congruent and mostly incongruent stimuli, however, the prior work on contingency learning discussed above suggests that attentional differences should become relevant. Note that this proposed attentional variation has nothing to do with an adaptation to conflict, however. The proposal is not that attention is pulled away from conflicting stimuli. Instead, the suggestion is that attention is *attracted* to stimuli that provide predictive information. Indeed, in the particular case of a PC task, a contingent word is also a viable cue for responding. Thus, attention may indeed be relevant, but conflict might not be.

Future work on this topic will need to focus not only on contingency confounds, simply speaking, but also on the overall informativeness of each stimulus. These considerations alone probably do not explain the entire range of data in the field (for a review of several others, see Schmidt, 2013a), but task regularities that allow the possibility for learning conflict-unrelated information do muddy the interpretation of any observed effects. The present analysis does not, however, only serve to question the interpretability of previously-published results. It is also hoped that this article might inspire future research on the potential role of attentional capture of contingent information in various conflict tasks. For instance, future research might attempt to assess stimulus informativeness biases independent of conflict. Informative stimuli might thus be shown to produce larger congruency effects, even if dissociated from PC. Complimentarily, one might attempt to test the notion of Bugg and Hutchison (2013) that conflict adaptation is observable when contingencies are absent or weak by constructing a situation in which mostly congruent and mostly incongruent stimuli are equally (un)informative. Disentangling these two accounts will probably be challenging, but would be fruitful if possible.

AUTHOR CONTRIBUTIONS

James R. Schmidt is the sole author and is a postdoctoral researcher of the Research Foundation—Flanders (FWO—Vlaanderen).

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Conflict components of the Stroop effect and their "control"

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A GENUINE MARKER OF THE AUTOMATICITY OF READING IN THE STROOP TASK

For the past four decades or so, an idea contrasting early definitions of automaticity and claiming that automatic processes can be controlled, has dominated the literature (Logan, 1980, 1985; Norman and Shallice, 1986; Tzelgov et al., 1990). The interference effect found in the Stroop task is usually considered to be a marker of automaticity of reading, while the modulation of its magnitude is referred to as a marker of control. In the present article, we emphasize the frequently overlooked notion that what we refer to as a marker of automaticity has in fact multiple origins. MacLeod and MacDonald (2000) and Goldfarb and Henik (2007) suggested that two types of conflict—the task and informational conflicts—contribute to Stroop interference. The *informational conflict* (henceforth IC) represents competition between two color concepts: one that is activated through color naming and the second that is activated by the reading process (e.g., the concepts red and blue respectively, when the stimulus is the word BLUE written in red ink). However, according to the same authors, some amount of interference is obtained even with color-unrelated words, since all words automatically activate the irrelevant reading task, setting in motion the competition between two possible tasks (henceforth *task conflict*; TC) (see Kalanthroff et al., 2013a; Entel et al., submitted, for behavioral evidence, and Bench et al., 1993; Carter et al., 1995, for neuroimaging evidence of the TC). Even non-word stimuli containing lexical information (e.g., letter strings) can interfere because they are readable (Klein, 1964; Sharma and McKenna, 1998). The more word-like the stimulus, the more interference it produces (Monsell et al., 2001). Thus, as evident from this distinction, the genuine marker of automaticity is the TC whereas the IC amplifies the interference from the irrelevant task. That is, in order to argue that the automatic reading process can be controlled one should actually show that what is controlled is the TC.

LIST-WIDE PROPORTION CONGRUENT EFFECT AND THE CONFLICT ADAPTATION ACCOUNT

The more frequent the incongruent trials are in an experiment, the smaller the Stroop effect (Logan and Zbrodoff, 1979; Logan, 1980; Tzelgov et al., 1992). This is known as the *list-wide proportion congruent* effect because the proportions are manipulated at the list level. The list-wide proportion congruent effect is considered to be a marker of control since it demonstrates the modulation of the magnitude of the Stroop effect, and as such, is interpreted in terms of conflict adaptation. According to the conflict-monitoring framework (Botvinick et al., 2001; De Pisapia and Braver, 2006; Braver, 2012), an increased proportion of incongruent trials results in higher conflict at the response level, which triggers the control system. The control process includes two stages: conflict detection and control exertion. Referring to our previous discussion, in order to claim the automatic reading process can be controlled, the TC should be the target of both stages of the control process. However, according to our analysis, this is not the case. In fact, the TC is only a target of the control exertion stage. According to the models within the conflict-monitoring framework, conflict reduction is achieved through adjusting the weights of the two tasks, thereby minimizing the TC. However, the conflict detection stage is centered on response competition, which requires the TC to be amplified by the IC. When there is no IC, that is, no competing color-concept activation by reading, no competing color-response can be activated. Focusing on response competition (and thereby on IC) by Botvinick et al. and later models (De Pisapia and Braver, 2006; Blais et al., 2007; see also Verguts and Notebaert, 2008, for a model integrating cognitive control and reinforcement learning) leads to the conclusion that the detection of conflict, and therefore triggering of the entire control process, requires the IC being present (see Kalanthroff et al., 2013b, for evidence inconsistent with this assumption). There is no "path" in these architectures allowing TC to be monitored without the presence of the IC (**Figure 1**). That is, the theory behind these architectures in their current state does not allow an unequivocal claim that reading, as an automatic process, can be controlled.

ITEM-SPECIFIC PROPORTION CONGRUENT EFFECT: CONFLICT ADAPTATION VS. LEARNING ACCOUNT

The conflict adaptation account has been challenged by manipulating the proportions of incongruent trials at the item level,

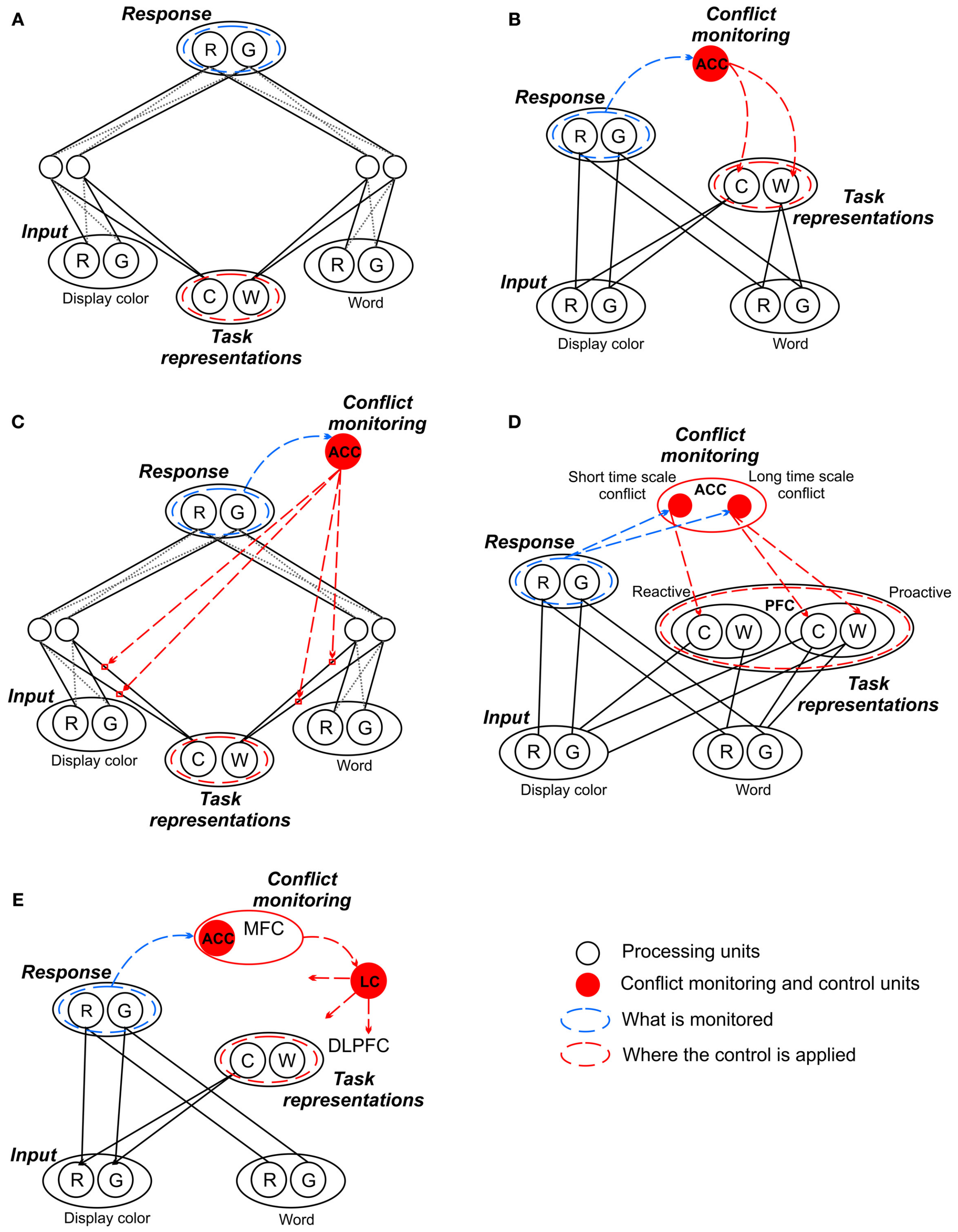

FIGURE 1 | The proposed architectures within the conflict-monitoring framework. (A) All models share the same core architecture introduced by Cohen et al. (1990) in their explanation of Stroop effect performance. This includes the definition of conflict as *response competition*, implying an aggregated contribution of task conflict and informational conflict. The assumption that conflict is controlled solely by adjustment of *task representation* weights implies that only the task conflict can be directly controlled. **(B)** Botvinick et al.'s (2001, 2004) model added a conflict-monitoring unit thereby generating a control loop for adjusting the task representation weights, while **(C)** Blais et al. (2007) proposed that task weights can be adjusted differentially for specific items. **(D)** De Pisapia and Braver's (2006) architecture captures the distinction between reactive and proactive control. **(E)** Verguts and Notebaert's (2008, 2009) model suggests control is modulated through conflict-based Hebbian learning. Note the models are depicted in a very schematic way, with no reference to the nature and direction of the existed connections, their specific weights, etc. The detailed information can be found in the original articles (see references). R, red; G, green; C, color; W, word; ACC, anterior cingulate cortex; LC, locus coeruleus; MFC, medial frontal cortex; PFC, prefrontal cortex; DLPFC, dorsolateral prefrontal cortex.

revealing an *item-specific proportion congruent* effect (Jacoby et al., 1999, 2003). In the item-specific paradigm, list-wide proportion congruence is held at 50%, and specific words are paired in most of the trials with a specific color, creating mostly congruent (i.e., not associated with conflict) or mostly incongruent (i.e., strongly associated with conflict) stimuli. The item-specific proportion congruent effect refers to a smaller interference for mostly incongruent items than for mostly congruent items.

In order to determine whether a word causes conflict, the word should be read, which contradicts the main assumption of the models in the conflict-monitoring literature, including those specifically adapted to explain the item-specific findings (Blais et al., 2007), that control operates proactively to prevent the initiation of the reading process. Consequently, it has been proposed (Bugg et al., 2008, 2011), and supported by empirical data (Bugg and Hutchison, 2012; Abrahamse et al., 2013), that item-specific control may be based on reactive control. This idea, however, is inconsistent with the assumption that reading, as an automatic process, is ballistic (Bargh, 1989), that is, difficult to stop once started. Stopping a ballistic reading process seems especially unlikely given that it is completed extremely quickly (Sereno et al., 1998; Cohen et al., 2000; Pulvermuller et al., 2001). More important, as the previous section illustrates, the conflict adaptation account can only explain the pattern obtained for incongruent (i.e., producing IC) items in mostly incongruent and mostly congruent conditions. However, manipulating the proportions at the item level seems also to affect the congruent (i.e., producing no IC) items, as evidenced (in our view) by the results of Jacoby et al. (2003). In that study, a 50/50 condition in which the number of presentations of words in each color was equal for congruent or incongruent stimuli was included in addition to the mostly congruent and mostly incongruent conditions. The analysis carried out by the authors showed that the larger the proportion of incongruent items was (from mostly congruent to 50/50 to mostly incongruent), the lesser the Stroop effect obtained. However, the 50/50 condition can be defined not just as a condition including more incongruent items than the mostly congruent condition, but also as a *neutral* condition where the conflict cannot be predicted by reading. Jacoby et al.'s data reveal that in comparison to the "neutral" (50/50) condition, incongruent items in the mostly incongruent condition were 32 ms faster. Similarly, and surprisingly, congruent items in the mostly congruent condition also showed a 21 ms reaction time (RT) reduction. Identical information regarding the conflict is provided by reading congruent words in the 50/50 and mostly congruent conditions, and yet RT in the latter condition is faster. This pattern contradicts the conflict adaptation account since congruent items do not produce IC, which according to our analysis, is the basis for control modulation. Schmidt et al. (2007; Schmidt and Besner, 2008) proposed a *contingency learning* account to explain Jacoby et al.'s (2003) finding without assuming conflict adaptation. It postulates that pairing a word with a specific color creates an association between that word and a specific response. The mechanism of contingency learning functions by lowering the threshold of the most frequently encountered response to the word, and does not lower the thresholds of other possible responses. Since according to the contingency learning account it does not matter if the word is paired mostly with congruent or incongruent colors, the facilitative effect of learning predicted by the contingency learning account is consistent with the results of Jacoby et al. (2003).

To prove the independence of the contingency learning mechanism of conflict, Schmidt and Besner (2008) demonstrated that the effects of contingency learning and congruency (i.e., IC) are additive by reanalyzing Jacoby et al.'s (2003) data. This evidence, however, is somewhat problematic because the rearrangement of the cells in the design manipulating proportion congruency still has the (congruency) confound, and the effect of contingency learning cannot be validly evaluated in such an analysis. In order to test directly whether contingency learning depends on the presence of conflict, Schmidt et al. (2007) (also Schmidt and Besner, 2008) conducted an experiment in which they eliminated IC by using neutral (i.e., color-unrelated) words only as stimuli in a color naming task. Their results demonstrated that the contingency learning effect does not require a stimulus to be a color-related (i.e., conflicting) word, supporting the idea that contingency learning is independent of the presence of IC. However, as suggested by MacLeod and MacDonald (2000), even neutral words are conflicting with respect to TC. Thus, the contingency learning effect might be independent of IC, but not of TC. Although such dependency would not weaken the ability of the contingency learning to explain the item-specific proportion congruent effect, it would suggest that this account might actually represent another control-like adaptive mechanism activated by (task) conflict. In fact, such evidence would dissipate the core controversy (i.e., control vs. learning) around the interpretation of the conflict adaptation effect, by incorporating the contingency learning into the category of control mechanisms.

Another potential problem with the contingency learning account is that it assumes that the association learned refers to a particular response in the sense of the button that should be pushed, but not in the sense of the correct color. This claim, supported by the results of their Experiment 4, is explicitly stated by Schmidt et al. (2007): "… pairings of stimuli do not simply form semantic connections… but instead directly cause changes in our behavior …" It is also evident in the architecture of the proposed parallel episodic processing (PEP) model (Schmidt, 2013) where the response generation layer consists of representations of the buttons the responses are mapped to, but not of the response set colors. If so, then it posits the questions of what would happen, and how contingency learning would express itself when instead of pushing the keys on a keyboard, responses are made vocally. When the response requires naming the color, there is no other way contingency learning can proceed but through linking the word with a specific color-concept because the latter is necessary for making a verbal response. That is, with vocal responses, contingency learning is predicted to affect the informational and not the response level of representations. However, if the words already have a strong semantic association with the color concept (i.e., congruent condition) then the contribution of the contingency learning process should be minimal, if at all. Therefore, with respect to the current discussion, the congruency of the item, or in other words, informational conflict or its absence, in some situations, might matter even for the contingency learning process.

SUMMARY

We do not pretend that the distinction between task and information conflict can solve the ongoing argument regarding the mechanism behind the "flexibility" of the Stroop effect, as reflected by the proportion effect. We do believe that the awareness of the fact that only one of two components contributing to the Stroop effect is a genuine marker of the automaticity of reading, would undoubtedly help in further developing existing control models, and probably new ones, that would be able to answer the question regarding controllability of reading. Distinguishing between two types of conflict can also be valuable with respect to the "control vs. learning" debate. For now, the proposed learning mechanism (i.e., contingency learning), as an alternative explanation for some of the proportion congruent effects, has only been proven to be independent from the IC. However, as mentioned, the TC is what really matters. Hence, in order to be considered as an independently standing mechanism that is not part of the control system, the contingency learning should also be evident when no TC is produced by stimuli.

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