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Is Conflict Adaptation due to Active Regulation or Passive Carry-Over? Evidence from Eye Movements

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Abstract

Conflict-adaptation effects (i.e., reduced response-time costs on high-conflict trials following high-conflict trials) supposedly represent our cognitive system's ability to regulate itself according to current processing demands. However, currently it is not clear whether these effects reflect conflict-triggered, active regulation, or passive carry-over of previous-trial control settings. We used eye movements to examine whether the degree of *experienced* conflict modulates conflict-adaptation effects, as the conflict-triggered regulation view predicts. Across two experiments in which participants had to identify a target stimulus based on an endogenous cue while—on conflict trials—having to resist a sudden-onset distractor, we found a clear indication of conflict adaptation. This adaptation effect disappeared however, when participants inadvertently fixated the sudden-onset distractor on the previous trial—that is, when they experienced a high degree of conflict. This pattern of results suggests that conflict adaptation can be explained parsimoniously in terms of a broader memory process that retains recently adopted control settings across trials.

How is cognitive control controlled? How does our cognitive system “know” when to tighten cognitive control and when to ease off? A potential answer to such questions comes from the observation that after high-conflict trials (e.g., a trial $n-1$ incongruent Stroop stimulus) people show reduced response-time (RT) conflict effects (e.g., trial n incongruent Stroop vs. congruent Stroop RTs). This pattern is typically referred to as the conflict-adaptation effect (Gratton, Coles, & Donchin, 1992) and it suggests that the experience of conflict may be a causal factor that drives subsequent increases in control. For more than a decade there has been a vigorous debate about the mechanistic explanations behind these types of effects (e.g., Botvinick, Braver, Barch, Carter, & Cohen, 2001; Braem, Abrahamse, Duthoo, & Notebaert, 2014; Egner, 2007, 2014).

Complicating matters, the standard conflict-adaptation effect, which is typically assessed in paradigms that induce response-selection conflict, is open to alternative explanations in terms of low-level stimulus-response priming or contingency-learning effects. (see Mayr, Awh, & Laurey, 2003; Mordkoff, 2012). In contrast, in the current work we examine adaptation to conflict that is induced during attentional selection. We do this for two reasons: First, within attentional conflict paradigms it is relatively straightforward to rule out the role of priming or contingency learning effects (for details, see section *Repetition and Contingency Learning Effects*). Second, and even more important, we can track the degree of experienced conflict on a trial-by-trial level by measuring eye movements. As elaborated in the next section, this allows us to test contrasting predictions from two competing explanations of conflict adaptation, one relying on active regulation, the other on passive carry-over of control settings. In the General Discussion we will then also return to the important question to what degree the conclusions from the attentional domain can be generalized to the more traditional adaptation effects found with response-selection paradigms.

Active Regulation versus Passive Carry-Over

According to the prominent conflict-monitoring theory (Botvinick et al., 2001), conflict adaptation reflects a feedback process where trial $n-1$ conflict is registered in the anterior cingulate cortex (ACC) and triggers an active process of updating the current goal representation in lateral prefrontal cortex (LPFC), thereby reducing sensitivity to conflict on trial n . Other variants of this general category of models suggest that instead of a conflict monitoring process, the ACC supports a more general system that learns and responds to cues signaling upcoming processing difficulty (Brown & Braver, 2005; Holroyd & Coles, 2002; Shenhav, Botvinick, & Cohen, 2013). Experienced conflict may be one such cue and therefore trigger necessary control processes.

All “conflict-triggered regulation” explanations have in common that the greater the experienced conflict on a given trial, the more probable the resulting control attempt and consequently the conflict-adaptation effect should be (e.g., see Yeung, Botvinick, & Cohen, 2011). For example, in the conflict-monitoring model, greater conflict leads to a greater likelihood of meeting the threshold for regulatory activity.

An alternative view of conflict adaptation is that it reflects more general memory processes that allow carry-over of previous control settings into subsequent trials. For example, according to network models of cognitive control, abstract task goals combined with trial-specific demands mold attentional control settings in a manner that enables successful performance (Cohen, Dunbar, & McClelland, 1990). Conflict on trial $n-1$ would promote a conflict-resistant attentional setting that then carries over into the next trial, where it again will protect from conflict. No assumptions about a dedicated conflict-triggered regulatory process are necessary here (e.g., Gilbert & Shallice, 2002; Mayr & Awh, 2008; Mayr, Kuhns, & Rieter, 2013; Scherbaum, Dshemuchadse, Ruge, & Goschke, 2012). Furthermore, greater experienced conflict on trial $n-1$ would indicate that only a suboptimal control setting has been established, which, when carried over into the next trial, should again provide insufficient protection from conflict. Thus, how the conflict-adaptation pattern responds to

the degree of experienced trial $n-1$ conflict allows us to distinguish between the conflict-triggered-regulation and the carry-over account.

To test these opposing predictions it would be important to measure experienced conflict in a trial-by-trial manner. Unfortunately, the RT conflict effect itself does not serve this purpose because it can only be computed as the difference score between conflict and no-conflict conditions, not on the level of individual trials. In order to index experienced conflict on a trial-by-trial manner, Mayr et al., (2013) recently used eye-tracking to assess fixations to goal-inappropriate information in the context of a task-switching paradigm.

In the current study, we used the same general approach, but applied it to an attentional capture paradigm. Participants made speeded responses to a letter (discriminating L versus R) that appeared within one of six stimulus circles indicated by a central cue (see Figure 1; see Theeuwes, Kramer, Hahn, & Irwin, 1998). Conflict was generated on 50% of trials by presenting not only the relevant cue/stimulus (e.g., the central cue), but also the irrelevant stimulus (e.g., the sudden onset). To maximize the conflict, subjects also received experience with the “opposite”, exogenous attention task (i.e., attending to the sudden-onset stimulus) in separate blocks of trials (interleaved with the theoretically critical, endogenous-attention blocks in Experiment 1 or in terms of two initial blocks in Experiment 2). While performing the endogenous attention task, fixations to the sudden onset stimulus served as indicator of high experienced conflict and therefore allowed us to look at conflict adaptation effects following conflict trials without distractor fixations (low experienced conflict) versus conflict trials with distractor fixations (high experienced conflict).

Repetition and Contingency Learning Effects

As mentioned, in the standard conflict paradigm it is difficult to rule out an alternative explanation of the conflict-adaptation effect in terms of stimulus repetitions (e.g., Mayr et al., 2003; Hommel, Proctor, & Vu, 2004). Trial-to-trial transitions for which conflict-adaptation models predict performance benefits (i.e., noconflict[N]->noconflict[N] or conflict[C]->conflict[C]), also typically contain exact stimulus repetitions (50% when 2-choice tasks are used). Such repetitions never occur in the two conditions for which worse performance is expected (i.e., N->C or C->N). Exact stimulus repetitions often go along with strong priming benefits and therefore induce a confound that can tilt the average RTs in C->C and N->N conditions in the direction of a conflict-adaptation pattern.

Unfortunately, attempts to reduce the impact of repetitions by using large stimulus sets (i.e., a Stroop task with six different color words) lead to new problems. On the one hand, keeping conflict probability at a rate of $p=.5$ while increasing the stimulus set, introduces contingencies between distractor and target information that make processing of distractor information potentially useful (e.g., Mordkoff, 2012). On the other hand, a random presentation of target and distractor stimuli leads to low probability of conflict (i.e., only $p=.25$ with 4 different stimuli), which leads to a particularly low probability of the theoretically most interesting condition in which conflict trials follow conflict trials ($p=.06$), and also is likely to change the nature of conflict effects (e.g., Mayr & Awh, 2008; Tzelgov, Henik, & Berger, 1992).

By introducing conflict through sudden-onset stimuli that do not overlap with the set of response-relevant stimuli we escape these potential pitfalls in our paradigm. With six different target and six different distractor locations the critical, complete stimulus repetitions are so rare that they are unlikely to affect overall results, or can be easily eliminated from data analysis. At the same time, we can use an unbiased conflict probability of $p=.5$ without introducing contingencies between distractor and target stimuli.

Method

Sample Size and Participants

In total 62 students of the University of Oregon participated in exchange for course credit, 32 in Experiment 1 and 30 in Experiment 2. One subject from Experiment 1 had incomplete data and was replaced by an additional subject.

Overview

In Experiment 1, subjects alternated between pure endogenous-attention and exogenous-attention blocks. We included the exogenous condition in our design even though our main focus was on the endogenous task because pilot work using only the endogenous task had produced relatively small conflict and conflict-adaptation effects. In Experiment 2, we aimed to replicate the basic pattern of effects using a more standard procedure where, except for two initial blocks with the exogenous task, subjects performed exclusively the endogenous task.

Stimuli

Figure 1 presents the basic stimulus setup. Participants were seated approximately 65 cm from the computer display. Six circular stimulus frames (diameter of each circle = 21 mm = 1.9 degrees) were presented on a virtual circle (radius = 95 mm = 7.6 degrees) around the screen's center. These circles were always presented in white on a black background. Within each circle the "&" symbol or the letters L, R, P, or K could be presented in white, size 14 Helvetica font. An additional, sudden-onset circle of the same size could appear between two of the regular circle positions along a slightly larger virtual circle (radius = 110 mm = 8.8 degrees). This sudden-onset circle was always presented in red and could also contain the letters L, R, P, or K in white, size 14 Helvetica font. At the center of the screen there were six smaller "cue circles" (diameter of each circle = 7 mm = 0.6 degrees). These were arranged in a way that mirrored the larger set of stimulus circles (radius of the central cue circle = 12 mm = 1.1 degrees), such that for each position in the larger stimulus circle, there was a corresponding, smaller cue circle. The smaller cue circles could be presented either in white or red.

Tasks

Participants were asked to perform single-task blocks of either the endogenous or exogenous task (named "center" and "surround", respectively). Each trial began with a 1000 ms (ITI) in which all of the large peripheral circles contained the "&" symbol and the central cue circles were all filled in red. In the endogenous task, all central cue circles turned white except for a single one that remained red (see Figure 1). This red circle indicated which of the

corresponding peripheral circles contained the response-relevant stimulus (L or R). In the exogenous task, the response-relevant stimulus was embedded in the single red sudden-onset circle that appeared between the peripheral circles. Conflict was manipulated by presenting stimuli for the irrelevant task on 50% of trials. Thus, for the endogenous task, on conflict trials a red sudden-onset stimulus appeared. For the exogenous task, on conflict trials a central cue was presented (i.e., one of the central cue circles remained red) while on the no-conflict trials, all cue circles turned white. Only the target circle contained a response letter and the remaining circles contained either a P or K. Stimuli remained on the screen until a response was made.

Procedure

In Experiment 1, participants performed the two tasks in 32 pure-task blocks of 20 trials each, alternating tasks in either an ABBA or a BAAB sequence (counterbalanced across participants). The first two blocks were considered practice. In Experiment 2, participants first performed two short practice blocks of 20 trials each (one endogenous, one exogenous) followed by two blocks of the exogenous task (80 trials each), then 10 blocks of the endogenous task (also 80 trials each).

Eye-Tracking

Eye movements were measured using the SR Research desk-mounted Eyelink 1000 Plus, controlled by the Eyelink Toolbox in MATLAB (Cornelissen, Peters, & Palmer, 2002) at a sampling rate of 1000 Hz. Fixations were recorded when neither a blink nor a saccade was present, and saccades were defined for each pair of successive data samples for which the velocity of eyes exceeded $30^\circ/\text{s}$ or the acceleration surpassed $8,000^\circ/\text{s}^2$. Calibration for eye position registration occurred after the end of the practice blocks and repeated every 4 blocks.

Analysis

For our analyses, we used an ANOVA approach or simple t-tests, when examining standard conflict or conflict-adaptation effects (i.e., without considering distractor fixations). Distractor fixations could not be analyzed within a balanced design. Therefore, we used multilevel regression when analyzing the effect of distractor fixations. In these analyses, experimental factors, the presence of distractor fixations, and the relevant interactions were coded as binary, fixed effects. In addition, all main effects were also included as random effects. Note, that there is no agreed-upon method to compute exact p values for coefficients from multilevel models (Baayen, Davidson, & Bates, 2008, Footnote 1). Therefore, in addition to reporting the fixed-effect coefficients, we report the t values, as well as confidence intervals around the coefficients (which were derived by multiplying the estimated standard error by 2).

Results and Discussion

Experiment 1

We excluded error and post-error trials (6.6% of all trials) as well as trials slower than 2000 ms (0.4% of trials). For the endogenous task, the RT conflict effect was 100 ms ($CI = \pm 16$),

no-conflict=843 ms ($SD=66$), conflict=943 ms ($SD=86$), $t(31)=12.87$, $p<.001$, and the error conflict effect was .70%. $CI=\pm 1.10$, no-conflict=3.0% ($SD=2.55$), conflict=3.7% ($SD=4.60$), $t(31)=1.23$, $p=.228$. Distractor fixations were determined by specifying an elliptical region centered on the position of the sudden-onset distractor that extended towards the center of the screen and outside of the array by 20 pixels on either side. Any fixation that occurred after stimulus onset and that fell within this region was coded as a distractor fixation (1). Any fixation on conflict trials that did not fall within this region was coded as 0. The elliptical region ensured that we were not capturing fixations to adjacent items on the screen, but also did not miss fixations clearly directed towards the distractor but under- or over-shooting it. Participants fixated the sudden-onset distractor on 16.24% ($SD=10.47$) of conflict trials. To validate the idea that fixations to the distractor indicate high-experienced conflict trials, it is important to show that RTs are in fact increased on such trials. Using a multilevel regression model predicting trial-by-trial RTs in the endogenous task from conflict (coded 0/1) and the occurrence of distractor fixations (coded 0/1), we found that on conflict trials with distractor fixations RTs were larger (174 ms, $CI=\pm 42$) than on conflict trials without distractor fixations (78 ms, $CI=\pm 12$). Thus, attentional capture that is expressed in overt eye movements does in fact go along with considerably increased RT conflict effects. At the same time, even in the absence of fixations, the presence of distractors slows down performance.

As is to be expected for the exogenous task, where subjects could rely on automatic pull of attention from the sudden onset stimuli, the conflict effect from the endogenous cue was relatively small, both for RTs, no-conflict=655 ms ($SD=46$), conflict=672 ms ($SD=50$), $t(31)=4.47$, $p<.001$ (conflict effect=17, $CI=8$), and for errors, no-conflict=3.03% ($SD=2.93$), conflict=3.79 ($SD=3.07$), $t(31)=1.827$, $p>=.077$ (conflict effect=.8%, $CI=.8$). Similarly as for the endogenous task, for the exogenous task, distractor fixations were determined using an elliptical region centered on the circle indicated by the endogenous task cue. Only on 1.59% ($SD=1.77$) of trials did participants fixate the distractor stimulus indicated by the endogenous cue. Given the small conflict effect and the absent conflict-adaptation effect, we focused only on the endogenous-task results for the remaining analyses.

The main focus of our analysis was on how (a) the conflict effect was modulated through previous-trial conflict (i.e., the standard conflict-adaptation effect) and (b) how the standard conflict-adaptation effect was affected when the conflicting information was fixated on the previous trial (as an indication of particularly strong experienced conflict). We first turn to the overall conflict adaptation effect, ignoring the potential influence of previous-trial eye movements. We examined these effects using a repeated-measures ANOVA with current trial conflict and previous-trial conflict as within-subjects factors. RTs on C->C trials (931 ms) were faster than for N->C trials (955 ms) and RTs for C->N trials (845 ms) were slower than for N->N trials (841 ms), resulting in an overall adaptation effect of 27 ms ($CI=\pm 16$), $F(1,31)=13.06$, $MSe=479.79$, $p<.001$.

To examine the role of experienced conflict on the adaptation effect, we next turn to the relationship between of trial $n-1$ distractor fixations on trial n conflict effects. The upper-left Figure 2 shows the conflict effects across all three critical conditions. In order to present the effects in Figure 2, we used two non-orthogonal contrasts that compared RTs after no-

conflict trials with (a) RTs after conflict trials without distractor fixations and (b) with RTs after conflict trials with distractor fixations within a multilevel regression model. Trial n conflict (coded 0/1) and the interaction with the two contrasts were included as additional fixed effects, and all main effects/contrasts were also included as random effects. As Figure 2 shows, the large conflict effect following no-conflict trials was substantially reduced after conflict trials without distractor fixations, but much less so after conflict trials with distractor fixations.

To test our primary predictions, we focused on post-conflict trials and included current-trial conflict and previous-trial distractor fixations (both coded 0/1) as fixed effects, as well as both predictors as random, main effects. The corresponding multilevel analysis revealed a conflict effect of 81 ms ($CI=\pm 15.48$) after conflict trials without distractor fixations, which increased by 48 ms ($CI=\pm 28.03$; $t=3.38$), after trials with distractor fixations (i.e., a reliable interaction between current-trial conflict and the absence/presence of previous trial distractor fixations). This increase of the conflict effect implies that the conflict adaptation was diminished with greater experienced conflict during trial $n-1$. Error effects were both numerically and statistically similar to the pattern of RT effects (see Table 1).

The lower-left panel of Figure 2 shows the effects of previous-trial conflict and distractor fixations on current-trial distractor fixations. In line with RT effects, distractor fixations were reduced on C->C compared to N->C trials, but only when the distractor was not fixated on the $n-1$ trial (see confidence intervals in Figure 2).

The fact that distractor fixations increased after conflict trials with distractor fixations raises the question to what degree the RT increase in the same condition can be attributed exclusively to this fixation-level effect, potentially indicating a phenomenon that is contained within the oculomotor system. To examine this, we repeated our main multilevel analysis of RTs on post-conflict trials as a function of whether or not a distractor fixation occurred on the $n-1$ trial (coded 0/1) and whether or not the current trial was a conflict trial (0/1), but included the presence or absence of a distractor fixation on the current trial (0/1) as an additional predictor. The increase of the conflict effect after conflict trials with distractor fixations was reduced from 44 ms without controlling for fixations to 27 ms ($CI=\pm 26$), but remained marginally significant. This suggests that the effects of trial $n-1$ distractor fixations affect trial n conflict processing in a more general manner that reaches beyond the oculomotor system.

In this task design, distractor repetitions were rare (1 in 6 trials) and complete repetitions even rarer (1 in 36 trials with distractor+target position repetitions) and therefore should only have small effects. In fact, when we repeated all critical analyses after eliminating distractor repetitions we obtained virtually identical results.

Experiment 2

Again, we excluded error and post-error trials (6.8%) as well as trials with RTs slower than 2000 ms (0.6%). Aside from the first two blocks, this experiment focused exclusively on the endogenous task. The RT conflict effect for these endogenous blocks was 89 ms ($CI=\pm 19$), no-conflict=840 ms ($SD=79$), conflict=928 ms ($SD=111$), $t(29)=9.41$, $p<.001$, and the error

conflict effect was 1.0%. $CI=\pm.58$, no-conflict=3.11% ($SD=2.90$), conflict=4.11 ($SD=3.58$), $t(31)=3.54$, $p<.01$. The determination of distractor fixations was identical to the procedure used in Experiment 1. Participants fixated the sudden-onset distractor on 15.17% ($SD=9.19$) of conflict trials. As in Experiment 1, RTs on conflict trials with distractor fixations were much larger than on conflict trials without distractor fixations, 158 ms ($CI=\pm 35$). The conflict effect when controlling for distractor fixations was 64 ($CI=\pm 7$), rather than the 89 ms without distractor fixations.

We again examined the basic conflict adaptation effect using repeated-measures ANOVA. RTs on C->C trials (918 ms) were faster than for N->C trials (938 ms), whereas RTs for C->N trials (841 ms) were essentially equivalent to those on N->N trials (839 ms), resulting in an overall adaptation effect of 19 ms ($CI=\pm 12$), $F(1,31)=10.61$, $MSe=243.90$, $p<.01$. For the test of our primary question, namely whether the conflict effect was larger after high versus low experienced conflict, we again focused on post-conflict trials and compared trial n conflict effects when the trial $n-1$ distractor was either fixated or not. The corresponding multilevel analysis revealed a conflict effect of 73 ms ($CI=\pm 16.84$), after conflict trials without distractor fixations, which increased by 30 ms ($CI=\pm 17.14$), after trials with distractor fixations. The upper-right panel of Figure 2 shows the estimates of conflict effects for all three conditions from the same multilevel model used for Experiment 1.

Different from Experiment 1, there was a slight tendency for a pattern of errors that was counter to that of the RT effects. Specifically, as shown in Table 1, the error conflict effect was numerically smaller following conflict trials with distractor fixations than following conflict trials without distractor fixations, $F(1,29)=3.48$, $MSe<0.01$, $p=.07$. In order to assess to what degree this suggests a speed-accuracy tradeoff that qualifies the interpretation of the RT adaptation effects, we computed for each subject the difference between conflict effects following conflict trials compared to conflict+distractor fixation trials, for both RTs and errors. There was no correlation between these two difference scores, $r(29)=-.03$, $p=.88$, a result that speaks against a speed-accuracy tradeoff. Moreover, the increase of the conflict effect after trials with conflict+distractor fixations for RTs was highly reliable even in that half of the sample for which the error pattern was more consistent with the RT pattern, 35 ms ($CI=\pm 11$), $t=3.10$.

The lower-right panel of Figure 2 shows the effects of previous-trial conflict and distractor fixations on current-trial distractor fixations. In line with RT effects, distractor fixations were reduced on C->C compared to N->C trials, but only when the distractor was not fixated on the $n-1$ trial (see confidence intervals in Figure 2).

Again, we examined to what degree the critical RT conflict-adaptation pattern can be attributed completely to the observed carry-over of distractor fixations between trial $n-1$ and trial n conflict trials. When analyzing post-conflict RTs as a function of trial $n-1$ distractor fixations and trial n conflict with trial n distractor fixations as added predictor, the increase of the conflict effect after $n-1$ conflict trials with distractor fixations remained reliable: 21 ms ($CI=\pm 16$), albeit smaller than the 30 ms when not controlling for current-trial distractor fixations. Thus, as in Experiment 1 the increase of the conflict effect after high experienced

conflict cannot be explained in terms of a carry-over of distractor fixations alone. Finally, the pattern of results survived the same set of control analyses we conducted for Experiments 1.

General Discussion

We tested two categories of models about how our cognitive system regulates the amount or quality of attentional selection it exerts to counteract information-processing conflict. According to the “conflict-triggered-regulation” accounts, experienced conflict functions either as an unconditional trigger or a learned signal that elicits regulatory activity to overcome further conflict. For this type of model, greater experienced conflict on trial $n-1$ should lead to larger regulatory effort and consequently a larger adaptation effect on trial n .

By the second type of model, the high-level goal specification on a given trial (i.e., “focus on endogenous cue”) enables the system to converge onto an attentional setting that (more or less) effectively deals with the demands on that trial, including potential conflict (Gilbert & Shallice, 2002; Kikumoto, Hubbard, & Mayr, 2015; Mayr et al., 2013). The attentional control setting is then passively carried over into the next trial, resulting in efficient processing when trial $n-1$ and trial n demands are similar, and less-efficient processing otherwise. In this account, greater experienced conflict indicates a less-efficient trial $n-1$ attentional setting and therefore should lead to a decreased adaptation effect on trial n .

We measured the degree of experienced conflict in a trial-by-trial manner using fixations to distracting, sudden-onset stimuli. Our results were very clear: We observed robust conflict adaptation, but only when trial $n-1$ conflict was *not* accompanied by distractor fixations. When the distractor was fixated on trial $n-1$ (i.e., when experienced conflict was high) the conflict adaptation effect was essentially eliminated. This result is inconsistent with the conflict-triggered regulation account, but can be easily explained by the carry-over account. Also, consistent with the carry-over model, after conflict trials with distractor fixations, participants were much more likely to re-fixate the distractor on the next trial (see also Kikumoto et al., 2015; Mayr et al., 2013). This indeed suggests that inefficient control settings on trial $n-1$ were carried over to trial n , where they provided an insufficient counterforce to the pull from the sudden-onset distractor. Equally important, our detailed analyses indicate that the carry-over of distractor fixations between $n-1$ and n conflict trials alone could not explain the absence of an RT adaptation effect following conflict trials with distractor fixations. Thus, the critical pattern of RT effects we observed was not simply the result of carry-over within the oculomotor system.

The use of sudden onsets as a source of conflict also allowed us to assess conflict-adaptation effects irrespective of repetition priming while at the same time avoiding contingency confounds that typically arise when using a design that eliminates stimulus/response repetitions (Mordkoff, 2012). Thus, along with other recent findings with “purified paradigms” (e.g., Freitas et al., 2007; Weissman, Jiang, & Egner, 2014), our results confirm that at least in certain situations abstract conflict-adaptation does exist, irrespective of lower-level priming or contingency effects.

The Complex Nature of Experienced Conflict

Experienced conflict is the central variable in the current work. However experienced conflict has a complex relationship with experimentally manipulated, “objective” conflict. In addition, the fact that experienced conflict is an endogenous variable that is not under experimenter control adds interpretational difficulties. Both of these aspects deserve further discussion.

Relationship between Objective and Experienced Conflict—The difficulty about the relationship between objective and experienced conflict becomes clear when looking at research that assessed conflict adaptation while manipulating objective degree of conflict. For example, Wendt, Kiesel, Geringswald, Purmann, and Fischer (2014) found that increasing objective conflict (by increasing the temporal gap between a distractor and a target stimulus) also generated greater conflict adaptation—the opposite of what we found for experienced conflict. In evaluating such a result it is critical to appreciate that there is no straightforward relationship between objective and experienced conflict and that without actually assessing experienced conflict, it is difficult to predict how this relationship affects conflict adaptation. Note for example that, as discussed in the next section, the carry-over model predicts that on trials with larger external conflict the system is driven towards stronger control settings (e.g., Gilbert & Shallice, 2002), which then carry over into the next trial to produce larger adaptation effects. This is exactly what Wendt et al. (2014) found. Yet, if one were to separate trials in which attentional control was successful (i.e., low experienced conflict) versus unsuccessful (i.e., high experienced conflict)—as we were able to do in the current work via fixations to distractors—we would expect larger adaptation effects for the former than for the latter. Whether an increase of manipulated conflict leads to more or less observed adaptation effects on average would depend on the relative impact of the two counteracting forces: the conflict-induced strengthening of control versus the potentially larger opportunity for control failures. In short, without information about experienced conflict, evidence that conflict adaptation increases with external conflict is not diagnostic when it comes to evaluating the relative merits of the carry-over versus conflict-triggered control account.

Experienced Conflict is not under Experimenter Control—By its nature, experienced conflict cannot be directly manipulated by the experimenter. This leaves the possibility that our observed “breakdown” of efficient control after distractor-fixation trials is not actually a reflection of a carry-over of an inefficient attentional set created on the previous trial. Instead, it might reflect the gradual waxing and waning of an endogenous control state that affects both trial $n-1$ and trial n processing. To rule out such a third-variable explanation, we repeated the critical analyses that tested to what degree conflict effects were larger after conflict trials with distractor-fixations than after conflict-trials without distractor fixations, but added trial $n-2$ RTs as additional predictors. We assume here that trial $n-2$ RTs can serve as a proxy for the strength of the control setting just prior to entering trial $n-1$. To ensure that $n-2$ RTs reflect endogenous variations, we first residualized them with regard to conflict on trial $n-2$. If the pattern of conflict-adaptation effects we had observed is simply due to the status of a slow moving, attentional control parameter then statistically controlling for this parameter should also eliminate the effects of $n-1$ distractor fixations on trial n .

conflict processing. Consistent with the idea of gradual waxing and waning of control that affects both RTs and distractor fixations, we did find in a separate analysis that RTs from the previous trial (residualized by conflict on that trial) predicted probability of distractor fixations on the current trial, Experiment 1: $b=.00007$, $t=3.26$; Experiment 2: $b=.00005$, $t=3.40$). However, controlling for $n-2$ RTs did *not* affect the critical pattern of results: The RT conflict effect after conflict trials with distractor fixations was still much larger than after conflict trials without distractor fixations, Experiment 1: $b=43$ ms, $t=3.00$; Experiment 2: $b=28$ ms, $t=3.46$. This result strengthens our conclusion that the observed effects do reflect a carry-over process that originates on trial $n-1$, and not just unspecific fluctuations of attention.

When the goal is to test alternative models of conflict adaptation, there is no alternative to taking experienced conflict seriously as a theoretically important parameter. Yet, given its status as an endogenous variable, it is difficult to completely put questions of causality to rest. In this regard it would be useful to utilize “tighter” measures of internal control states than was possible here (i.e., using trial $n-2$ RT), for example by using pre-stimulus EEG measures that seem to track lapses of control on a millisecond by millisecond basis (e.g., Adams & Vogel, 2016).

Conflict-triggered Regulation versus Passive Carry-Over

Our finding that adaptation effects may result from passive carry-over of attentional control settings needs to be evaluated relative to evidence in favor of conflict-triggered regulation. So far, the strongest such evidence comes from neuroimaging studies showing that trial $n-1$ ACC activity predicts both LPFC activity and efficiency of conflict regulation (e.g., Kerns et al., 2004; see also Kerns, 2006). However, these studies (a) did not use methods that completely ruled out contributions from stimulus/response repetitions or contingencies and (b) did not provide the necessary statistical tests to ensure that the observed relationship actually reflects ACC activity as leading indicator of LPFC activity rather than just two brain areas that are correlated across time points (for a discussion of these two points, see Mayr & Awh, 2008). In an important study that did effectively control for stimulus repetitions, Egner and Hirsh (2005) showed particularly strong LPFC activity for conflict trials following conflict trials, but no relationship with ACC activity was observed. Thus, the larger LPFC activity on C->C trials could have been the result of carry-over from the preceding trial. Interestingly, studies on patients with ACC damage (e.g., Fellows & Farah, 2005) or on primates (Mansouri, Tanaka, & Buckley, 2009) also do not confirm the role of the ACC as a conflict-monitoring system.

In the seminal Cohen et al. (1990) network model of the Stroop effect, which also served as the basis of the conflict-monitoring model, control is exerted through activity in so-called task-demand nodes. These task-demand nodes receive constant input from the “instructional units” and impose task focus on lower-level nodes. On simulated conflict trials, it takes longer to resolve response conflict than on no-conflict trials, allowing for more activation flowing from instructional units to the task-demand units. Ultimately this leads to a stronger control setting at the time the actual response is executed on conflict than on no-conflict trials—with no need of a separate monitoring system. The only additional assumption

required to model the conflict-adaptation effect is that the activity level assumed on a given trial carries over into the next trial. Gilbert and Shallice (2002) proposed this mechanism to explain task-switching effects (see also Mayr et al., 2013), and more recently it was further elaborated by Scherbaum, Fischer, Dshemuchadse, and Goschke (2011) to explain adaptation effects. This shows that in principle a model without a dedicated monitoring/regulatory system can explain conflict-adaptation effects. What we add here is evidence that the carry-over account is not only more parsimonious, but—at least in the current context—better able to explain how conflict-adaptation effects responds to a key variable, the amount of experienced conflict.

It is worth noting that our results could still be explained by a hybrid, regulation/carry-over account where a fast-acting, conflict-detection process registers conflict and triggers regulation *within* a given trial, resulting in an attentional setting that then carries over into the next trial. However, without clear evidence for active, within-trial conflict-detection and regulation, for now the pure carry-over model can be considered the more parsimonious account.

One remaining question is how exactly the carry-over of control settings occurs. The idea that activation patterns in neural networks stay active from one trial to the next may be too simplistic to account for phenomena that occur on longer time scales, such as when adaptation effects survive intermediate trials and appear to be tied to a particular episodic context (e.g., Crump, Gong, & Milliken, 2006). A more realistic model is one where control settings become part of context-specific episodic memory instances (Blais, Robidoux, Risko, & Besner, 2007; Mayr, Kuhns, & Hubbard, 2014; Mayr & Bryck, 2005; Verguts & Notebaert, 2008) and are automatically retrieved when a similar context is encountered. As proposed by Egner (2014), such a model is also better suited to handle adaptation phenomena across different levels of abstraction (i.e., stimulus repetitions vs. abstract “task-set” repetitions), as well as interactions between levels of abstraction (see Weissman et al., 2014). By this view, the traditional trial-to-trial conflict-adaptation pattern is just a special instance of the general process of encoding of control/selection episodes into memory, and retrieving them at a later point (which in this case is the next trial).

Attentional versus Response-selection Conflict

In this work we used attentional conflict rather than the more standard response-selection conflict. On the one hand, this extends the conflict-adaptation phenomenon into an important area of attentional control in which adaptation effects have not been systematically examined. On the other hand, this is also a potential limitation as it is possible that functionally different regulatory mechanisms operate for different types of response conflict. In fact, in the original paper describing the conflict-monitoring model the focus was on response conflict. However, even there the authors explicitly mentioned the possibility that conflict monitoring is a more general phenomenon (Botvinick et al., 2001; see also Carter & van Venn, 2007). Furthermore, anterior cingulate activity has been shown to respond to perceptual conflict, albeit not at the exact same locations as for response-selection conflict (Kim, Kroger, & Kim, 2011).

In this context it is also of interest that Weissman and Carp (2013) recently provided evidence that the conflict-adaptation effect in a more traditional response-conflict paradigm was independent of previous-trial RTs. While failing to find the elimination of conflict-adaptation after high experienced conflict, this is at least partially consistent with our results as higher experienced conflict did *not* lead to greater conflict adaptation, as predicted by the conflict-monitoring model. In interpreting the differences in results across these two studies it is also important to note that RTs are an imperfect reflection of experienced conflict as they also reflect fluctuations in other aspects (e.g., response thresholds). Arguably, distractor fixations are a more direct indicator of conflict experienced by the information processing system.

On the basis of the current results we may not be able to definitely conclude that the carry-over model explains conflict-adaptation effects across all levels and domains. However, these results do provide an existence proof that conflict-adaptation effects can be produced by carry-over of abstract control settings, without requiring an explicit, conflict-triggered regulation process. Future work needs to solve the (non-trivial) problem of extending the current approach of utilizing direct, trial-by-trial indicators of conflict processing to response-selection paradigms, while at the same time eliminating the influence of low-level priming or contingency learning effects.

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Figure 1.

Stimulus displays used for both Experiments. In the endogenous task, participants responded to letters (L or R) embedded in the circle indicated by the central cue. In the exogenous task, participants responded to the letters embedded in the exogenous, sudden-onset circle.

Figure 2.

RT conflict effects (upper panels) and conflict-trial distractor fixation rates (lower panels) following no-conflict trials, conflict trials without distractor fixations, and conflict trials with distractor fixations separately for Experiments 1 and 2. Effects were estimated through multilevel linear models contrasting the lag-1 no-conflict condition with each of the two lag-1 conflict conditions. Error bars represent within-subject confidence intervals for the differences corresponding to the two contrasts.

Table 1

Mean error percentages (SD) as a function of trial $n-1$ conflict status and trial n conflict status.

Trial n	no conflict	conflict	Trial $n-1$ conflict + distr. fix.
Exp. 1			
no conflict	3.16 (2.68)	2.74 (3.16)	3.15 (3.23)
conflict	3.74 (5.34)	3.61 (4.46)	3.69 (3.61)
Exp. 2			
no conflict	3.16 (2.92)	2.95 (3.00)	4.47 (5.17)
conflict	4.22 (3.87)	4.06 (3.62)	3.75 (4.70)