

Attentional Adjustment to Conflict Strength

Evidence From the Effects of Manipulating Flanker-Target SOA on Response Times and Prestimulus Pupil Size

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Abstract. Current models of cognitive control assume gradual adjustment of processing selectivity to the strength of conflict evoked by distractor stimuli. Using a flanker task, we varied conflict strength by manipulating target and distractor onset. Replicating previous findings, flanker interference effects were larger on trials associated with advance presentation of the flankers compared to simultaneous presentation. Controlling for stimulus and response sequence effects by excluding trials with feature repetitions from stimulus administration (Experiment 1) or from the statistical analyses (Experiment 2), we found a reduction of the flanker interference effect after high-conflict predecessor trials (i.e., trials associated with advance presentation of the flankers) but not after low-conflict predecessor trials (i.e., trials associated with simultaneous presentation of target and flankers). This result supports the assumption of conflict-strength-dependent adjustment of visual attention. The selective adaptation effect after high-conflict trials was associated with an increase in prestimulus pupil diameter, possibly reflecting increased cognitive effort of focusing attention.

Keywords: conflict adjustment, executive control, flanker task

Conflict paradigms are characterized by the fact that target stimulus information is presented together with distractor stimulus information, which may be associated with an incorrect response. A prominent example is the Stroop task (Stroop, 1935; for an overview, see MacLeod, 1991), in which participants identify the print color of color words and the word of a given trial may correspond to the target color (e.g., “red” printed in red) or to a different color used as target (e.g., “green” printed in red). Similarly, in the Eriksen flanker task (Eriksen & Eriksen, 1974; for an overview see Eriksen, 1995), a target character (usually defined as target by its location) is presented together with distractors which, if they occurred as target, would call for the same response as the current target, or together with distractors which, if they occurred as target, would call for a different response. In the following, we refer to conditions, in which target and distractor(s) are associated with the same response as *compatible*, and conditions, in which target and distractor(s) are associated with different responses as *incompatible*. The most fundamental finding in conflict tasks is that performance is impaired on incompatible trials

compared to on compatible trials across a wide range of procedural variations. This *compatibility effect* is taken to reflect incompleteness of selective attention, that is, participants process distractor stimulus information despite they being informed about their irrelevance to the current task. More precisely, it is assumed that distractor information is translated into corresponding response activation, which interferes with responding to the target stimulus information in the case of an incompatible trial by evoking response conflict (and possibly facilitates responding on compatible trials due to “double” activation of the required response; e.g., Cohen, Dunbar, & McClelland, 1990; Coles, Gratton, Bashore, Eriksen, & Donchin, 1985; Eriksen & Schultz, 1979).

Current models of cognitive control assume that conflict between incompatible responses acts as a trigger to enhance selective attention. Most prominently, connectionist models (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Botvinick, Cohen, & Carter, 2004, see also Blais, Robidoux, Risko, & Besner, 2007; Verguts & Notebaert, 2008) assume that conflict induces adjustment of

attentional weights assigned to the processing of information from the target and the distractor(s), thereby reducing the risk of erroneous responding (i.e., emitting the distractor-related response) on a future occasion. These models hold that conflict detection includes the generation of an internal quantitative measure of conflict (i.e., the degree of concurrent activation of multiple responses). Distribution of attentional weights across target and distractor dimensions is then adjusted as a function of this conflict measure, resulting in gradual variations of processing selectivity (i.e., dominance of processing target information over distractor information).

Evidence for the conflict adjustment conjecture is seen in findings of reduced compatibility effects after an incompatible compared to after a compatible predecessor trial, termed the *Gratton effect* (Gratton, Coles, & Donchin, 1992). Although the Gratton effect is well established and has been observed in different experimental paradigms such as the Stroop task (e.g., Kerns et al., 2004), the flanker task (e.g., Gratton et al., 1992), and the Simon task (e.g., Stürmer, Leuthold, Soetens, Schröter, & Sommer, 2002), evidence for the presumed quantitative relationship between conflict and adjustment is rare. This is because most studies contrasted only compatible and incompatible conditions, void of further variations of conflict strength, thereby not being able to detect differences in conflict adjustment other than associated with these two categorical levels.

An explicit attempt to examine adjustment as a function of conflict strength was made by Takezawa and Miyatani (2005). These authors used a flanker task and manipulated the spatial distance between target and flanker stimuli. Replicating previous findings, the compatibility effect was overall higher with greater target-flanker proximity, suggesting enhanced conflict in this condition. In addition, whereas reaction times (RTs) increased in compatible trials following an incompatible trial associated with a proximal target-flanker pairing, no effects of previous target-flanker distance were found in compatible trials following a compatible trial or in incompatible trials following both compatible and incompatible trials. No explicit comparison of the compatibility effects for compatible and incompatible predecessor trials was reported, however.

To manipulate conflict strength parametrically, Forster, Carter, Cohen, and Cho (2011) used a modified flanker task in which a target letter (*H* vs. *S*) was symmetrically surrounded by three letters on each side and presented both stimuli involving homogeneous flankers (e.g., *HHHHHHH* and *SSSHSSS*, for compatible and incompatible conditions, respectively) and stimuli made up of mixed flankers (e.g., *SSHHSS* or *SHHHHS*). RT and error rate increased with the number of incompatible flankers, suggesting a correlation with conflict strength. Moreover, in line with the assumption of conflict-strength-dependent adjustment, the performance difference for compatible trials (i.e., homogeneous letter strings) and incompatible trials (i.e., averaged across all heterogeneous letter strings) was lower after an incompatible trial associated with homogeneous than with mixed flankers.

Less consistent findings were obtained in conflict task studies which included “neutral” stimuli, that is, stimuli

that involve a distractor which is not assigned a response, such as the letter *X* in a flanker task which requires a discrimination between the target letters *H* and *S*. The lack of a distractor-response relationship should yield an intermediate level of conflict strength compared to compatible trials (in which the same response is activated by both stimulus features) and incompatible trials. Consistent with the notion of gradual adjustment to conflict strength, some studies found the compatibility effect after neutral trials to be smaller than after compatible and larger than after incompatible trials (although statistical significance of these differences was not established; Verguts, Notebaert, Kunde, & Wühr, 2011, Experiment 1; Wühr & Ansorge, 2005), whereas in other studies the compatibility effect after neutral trials resembled the compatibility effect after compatible trials (Bugg, 2007; Davelaar & Stevens, 2009; Verguts et al., 2011, Experiment 2) or after incompatible trials (Lamers & Roelofs, 2011). Furthermore, in the studies of Bugg (2007) and of Davelaar and Stevens (2009) the pattern of results markedly differed for trials associated with repetition and with alternation of the response, a factor not considered in the studies of Lamers and Roelofs (2011) and Verguts et al. (2011).

A notorious difficulty regarding the interpretation of sequential compatibility effects relates to a confound of the sequence of compatibility levels and the sequences of specific stimulus features and responses, possibly putting some sequences of compatibility levels at a disadvantage for non-attentional reasons. More precisely, trials associated with repetition of some features and alternation of other features of a preceding S-R event (i.e., partial feature repetitions) may be put at a disadvantage compared to trials in which all features repeat or alternate as a result of retrieval of a previous processing episode which includes information that mismatches current stimulus-response requirements (e.g., Hommel, Proctor, & Vu, 2004). Breaking down pairs of consecutive trials according to compatibility level sequence and distractor-response sequence shows that compatible trials following an incompatible trial and incompatible trials following a compatible trial are associated with a higher proportion of partial feature repetitions than repetitions of the compatibility level, and this difference is particularly marked with small sets of distractors and responses. In the extreme case of only two possible distractors and responses (as, e.g., in the study of Takezawa & Miyatani, 2005), compatible trials following a compatible trial and incompatible trials following an incompatible trial are exclusively made up of complete distractor/response repetitions or alternations, whereas compatible trials following an incompatible trial and incompatible trials following a compatible trial are exclusively made up of partial feature repetitions. Lacking careful control, a Gratton effect may therefore, at least in part, reflect feature integration and retrieval processes, rather than attentional adjustment.

Consideration of feature sequences may also be important when neutral stimuli or stimuli with mixed flankers are used to create intermediate conflict levels. Regarding neutral predecessor trials, this is because contrasting with the feature sequences on non-neutral trials following a non-neutral trial laid out above, both compatible and

incompatible trials are necessarily associated with alternation of the distractor whereas the response may either repeat or alternate (e.g., $XHX \rightarrow HHH$, and $XHX \rightarrow HSH$). Inter-mixing trials with mixed flankers, in turn, produces a complicated relationship of trial-to-trial conflict level and target-flanker sequences, with various kinds of partial feature repetitions.

We are aware of two other findings which bear on the issue of conflict-strength-dependent adjustment. First, in a numerical flanker task – in which the digits 1–9 (except 5) are used as targets and flankers – Nuerk, Bauer, Krummenacher, Heller, and Willmes (2005) found a negative relationship of the compatibility effect with numerical target-flanker distance. Assessing the compatibility effect as a function of the numerical target-flanker distance on the preceding trial, Notebaert and Verguts (2006) found a positive relationship, that is, a larger compatibility effect after trials in which response conflict was assumed to be high. Notebaert and Verguts attributed this effect to a different type of conflict, taking place at the level of stimulus processing, which was assumed to be negatively correlated with response conflict.¹

Second, a particularly large Gratton effect seems to occur when the overall proportion of conflict trials is low (e.g., Kerns et al., 2004). This is not surprising, if it is assumed that frequent presentation of conflict stimuli results in sustained enhancement of attentional selectivity (Funes, Lupiáñez, & Humphreys, 2010) – thereby decreasing the difference in conflict strength on compatible and incompatible trials (e.g., Botvinick et al., 2001, cf. Durston et al., 2003; Jiménez & Méndez, 2013) and, by consequence, attenuating trial-to-trial conflict adjustment. Pursuing this idea, Purmann, Badde, and Wendt (2009) compared the Gratton effect, devoid of any distractor or response feature repetition, for conditions associated with low and high proportions of conflict trials and found it reduced in the latter.

In summary, although evidence consistent with the assumption of conflict-strength-dependent focusing of attention has been obtained by different experimental manipulations, this evidence either stems from studies lacking control of stimulus and response feature sequences (i.e., Forster et al., 2011; Takezawa & Miyatani, 2005; Wühr & Ansorge, 2005) or requires additional assumptions (e.g., sustained adjustment to frequent conflict stimuli). Moreover, a number of studies including neutral stimuli failed to yield corresponding evidence, and there is at least one study in which the compatibility effect correlated positively with preceding target-flanker response compatibility effects. Given this overall situation, further corroboration of the notion of conflict-strength-dependent adjustment seems desirable.

In the current study, we sought evidence regarding the relationship of conflict strength and attentional adjustment using a flanker task and varying the stimulus onset asynchrony (SOA) between targets and flankers. Previous studies showed that giving flanker processing a “head start”

yields an increased compatibility effect as compared to simultaneous onset of flanker and target, and this has been attributed to enhanced flanker-based response activation (Eriksen & Schultz, 1979; Flowers, 1980). By consequence, the amount of conflict should be increased on incompatible trials (and possibly decreased on compatible trials) associated with advance presentation of the flankers. Assuming conflict-strength-related adjustment, then, predicts a larger Gratton effect after these trials than after trials in which target and flankers occur simultaneously. To avoid all confounds with low-level feature sequence effects, we applied a 4:4 mapping of stimuli to responses which allowed us to construct trial sequences devoid of any target/response or flanker repetitions (Experiment 1) or exclude data from all trials associated with any target/response or flanker repetitions from the statistical analyses (Experiment 2).

Experiment 1

Besides the main purpose of assessing conflict adjustment, depending on conflict strength, Experiment 1 was designed to pursue two additional aspects. First, we investigated another putative adjustment mechanism that has been proposed to account for occasional findings of generally slowed performance after an incompatible as compared to after a compatible trial (e.g., Akçay & Hazeltine, 2007; Fernandez-Duque & Knight, 2008; Fischer, Dreisbach, & Goschke, 2008; Lamers & Roelofs, 2011; Verguts et al., 2011; Wendt, Kluwe, & Peters, 2006). Such *post-conflict slowing* suggests that participants tend to adopt a more conservative response criterion after experiencing a conflict trial. When only compatible and incompatible stimuli are used, the effect may be difficult to observe, however, because of the facilitated responding to an incompatible stimulus after an incompatible predecessor trial, resulting from conflict-induced attentional focusing. To obtain a pure measure of post-conflict slowing, and its dependence on conflict strength, we included neutral trials and analyzed performance on these trials as a function of the compatibility level of the preceding trial (cf. Verguts et al., 2011).

Second, we applied pupillometry to determine the relation of conflict adjustment to variations in baseline pupil size. It has been known for a long time, that pupil size is positively correlated with difficulty of a task (e.g., working memory load, Kahneman & Beatty, 1966). For instance, it has been shown that pupil size increases with number of digits in a digit span task (Granholm, Asarnow, Sarkin, & Dykes, 1996), with increasing task-difficulty in a short-term memory task (Kahneman & Beatty, 1966; Peavler, 1974), with increasing complexity of sentences in a sentence comprehension task (Just & Carpenter, 1993), and with increasing memory load in a digit span task and sentence length in a recall task (Piquado, Isaacowitz, & Wingfield, 2010).

¹ The assumption that stimulus conflict is higher for numbers with greater numerical proximity was derived from the notion that distributions of activation of numbers adjacent on the mental number line should have greater overlap than distributions of activation of distant numbers (Notebaert & Verguts, 2006).

Pupil dilation in these studies has been interpreted as an index of increased cognitive effort needed for successful performance under more difficult task conditions.

In a recent Stroop study, it has been found that pupil size increases between 200 ms and 500 ms after stimulus onset and between about 200 ms and about 800 ms after the response has been given. Only during the second time period, pupil dilation was larger for incompatible stimuli than for compatible or neutral stimuli, while there was no difference during the first time period (see Figure 2 in Laeng, Ørbo, Holmlund, & Miozzo, 2011). As the later pupil dilation effect occurred after the response has been given, one might argue that it reflects more adjustment processes which affect information processing in the following trial (i.e., adaptation to recent conflict) than processing of the current stimulus. In the current study, we attempted to extend these findings by examining pupil dilation during the preparation interval of an upcoming stimulus (i.e., shortly before stimulus onset) after both low- and high-conflict trials. Assuming a relationship with attentional focusing, we expected to find a positive relation of pupil size and conflict strength on the preceding trial.

Method

Participants

Fifteen students (11 female and 4 male, age range 19–31 years, mean age was 24 years) of Otto-von-Guericke University Magdeburg participated in this experiment. All had normal or corrected-to-normal vision. Participants attended a single experimental session lasting about 40 min in exchange for payment or partial course fulfillment.

Apparatus and Stimuli

Stimulus presentation and response recording were controlled using the Psychtoolbox (Brainard, 1997; Pelli, 1997) and the EyeLink Toolbox (Cornelissen, Peters, & Palmer, 2002) under Matlab (The MathWorks, Ltd., Cambridge, UK) on a PC with a 22-inch SyncMaster 2233RZ LCD monitor (Samsung Electronics, Seoul, South Korea). The monitor was 474 mm (1,680 pixels) wide and 296 mm (1,050 pixels) high and the vertical refresh rate was 120 Hz. The viewing distance to the screen was kept constant at 85 cm by stabilizing participant's head using a chin- and forehead rest, leading to a pixel size of 0.019° of visual angle. Responses were recorded with a ResponsePixx Handheld five button response box (Vpixx response box, model VPX-ACC-3100, Saint-Bruno, QC, Canada). Participants were tested individually in a dimly lit room.

Circles encompassing a gap at the top, left, bottom, or right (also known as Landolt rings and commonly used as test objects to evaluate visual acuity) were used as targets and flankers (cf. Purmann et al., 2009). In addition, rings without gap were used as neutral flankers. The target was presented at the center of the screen and was flanked by four copies of either the same or a different Landolt ring

in a cross-like fashion. All stimuli, including instructions and feedback, were presented in white on a gray background. Participants made discriminative responses by pressing one of four response keys on the button response box. The response keys were arranged in a cross-like fashion. Participants were instructed to press the key spatially corresponding with the gap in the target stimulus gap. The left and right keys were pressed by the middle and index finger of the left hand, respectively. The top and bottom keys were pressed by the index finger and the thumb of the right hand, respectively. This setup allowed participants to keep their eyes focused on the screen (compared to a setup where they had to use one finger of one hand) and was the most comfortable position for their hands (compared to other setups). Participants were instructed to make their responses as fast as possible while maintaining an error rate of less than 10%.

Procedure

The experiment consisted of 16 blocks of 80 trials each. After each block, performance feedback was provided concerning the participant's mean RT and error rate. Participants were encouraged to take a short break after each block and they started each block at a self-determined point in time.

To induce flanker-target conflict of different strength, the flanker-target SOA was varied between 0 ms (i.e., simultaneous onset) and -150 ms (flanker onset preceding target onset by 150 ms). A prime-probe-like paradigm was used, in which probe trials were always associated with a SOA of 0 ms whereas prime trials involved a SOA of 0 ms or of -150 ms. That is, trials in the trial sequence were categorized as successive pairs of S-R events. The first trial of each pair of trials in which the second trial was associated with a SOA of 0 ms was considered the prime, the second trial of a pair was considered the probe and used to assess conflict adjustment as a function of compatibility and SOA in the prime trial. Note that by this procedure, trials associated with a SOA of 0 ms that preceded another trial associated with a SOA of 0 ms act as both a prime and a probe trial. A proportion of probe trials but no prime trial contained neutral flankers. Participants were not made aware of these relationships.

To make it easier for participants to keep their eyes still, a fixation cross was presented at the center of the screen. Each trial then comprised the following events: After the presentation of a blank screen for 100 ms, the fixation cross appeared and remained on the screen for 500 ms. Subsequently, the flankers were either presented alone or together with the target, depending on the SOA condition. In the 0 ms SOA condition, the target and the flankers were presented simultaneously and remained on the screen for 150 ms. In the -150 ms SOA condition, the flankers were presented alone for 150 ms and remained on the screen for another 150 ms together with the target. After presentation of the stimulus, the fixation cross reappeared at the center of the screen. There was no time limit for participants to respond. The next trial started approximately 1,000 ms

after the response was given. In case a wrong response was given, a 500 ms long 500 Hz tone was presented as immediate error feedback. SOA, target, and flanker were chosen randomly on each trial with the constraints that a trial with a SOA of -150 ms was always followed by a trial with a SOA of 0 ms, that neutral flankers only occurred in trials with a SOA of 0 ms, and that none of the four Landolt rings nor the neutral ring occurred on successive trials.

Measurement and Preprocessing of Pupillary Data

Head movements were minimized by stabilizing participants' heads using a chin- and forehead rest, allowing reliable recording of eye position and pupil dilation. The stimuli were viewed binocularly and the eye position and pupil size of the right eye were recorded at 1,000 Hz using a dark pupil/corneal reflection eye tracker (Eyelink 1000, SR Research, Osgoode, ON, Canada) which was placed immediately below the computer screen. Pupil dilation was measured in arbitrary units (pixel) as recorded by the eyetracker. Participants did not receive any particular instructions regarding eye blinks.

Eye tracking data were analyzed with custom Python scripts. We extracted 1,500 ms epochs around stimulus presentation from 600 ms before onset of the target to 750 ms after onset of the target. We then applied the Savitzky-Golay FIR smoothing filter algorithm (Savitzky & Golay, 1964) with second-order polynomials and a filter length of 20 ms to the gaze position data of these epochs, to obtain smoothed velocity and acceleration profiles from its second and third derivative, respectively. In order to detect periods of invalid or missing gaze data due to eye blinks or signal loss, we first calculated the total angular velocity and acceleration of the eye as the Euclidean distance of the respective x - and y -components. We then identified invalid gaze samples as those periods in which the gaze position was outside of the display or in which the measured movement of the eye was not physiologically plausible, that is, the angular velocity exceeded $1,000^\circ/\text{s}$ or the angular acceleration of exceeded $100,000^\circ/\text{s}^2$. The pupil data during those

invalid periods were interpolated with third-order polynomials, from the end of the last period in which the speed of the eye was below $20^\circ/\text{s}$ for at least 100 ms, to the first period fulfilling these criteria after the noise event. Additionally, the pupil data were interpolated for all periods in which the eye-movement exceeded $20^\circ/\text{s}$. Following artifact correction, we averaged the epochs for each participant and condition. The same exclusion criteria as for the response time analysis were applied.

Results

The first block was considered training and not analyzed. Error trials and the trials that followed an error were excluded from the RT analyses (together 14% of all trials). Furthermore, trials with RTs that did not fit the outlier criterion ($> 2.5 SD$ for each participant and condition mean, on average 1.4%) were excluded from analyses. The mean RTs are presented in Figure 1, the mean error percentages are presented in Table 1.

Prime Trials

Repeated measures ANOVAs were conducted on RTs and percent error, including the factors SOA (-150 ms, 0 ms) and Flanker compatibility (compatible, incompatible). RTs differed for the SOA conditions, $F(1, 14) = 1053.30$, $p < .001$ (422 ms for the SOA of 0 ms and 533 ms for the SOA of -150 ms). Furthermore, participants responded faster on compatible (439 ms) compared to incompatible trials (515 ms), $F(1, 14) = 167.29$, $p < .001$. Most importantly, the size of the flanker compatibility effect was larger when flanker stimuli preceded the target (i.e., 122 ms) than when the flankers occurred simultaneously with the target (i.e., 28 ms), $F(1, 14) = 124.47$, $p < .001$, although it reached statistical significance for each of the two SOA conditions ($t[14] = 12.8$, $p < .001$, and $t[14] = 8.0$, $p < .001$, respectively).

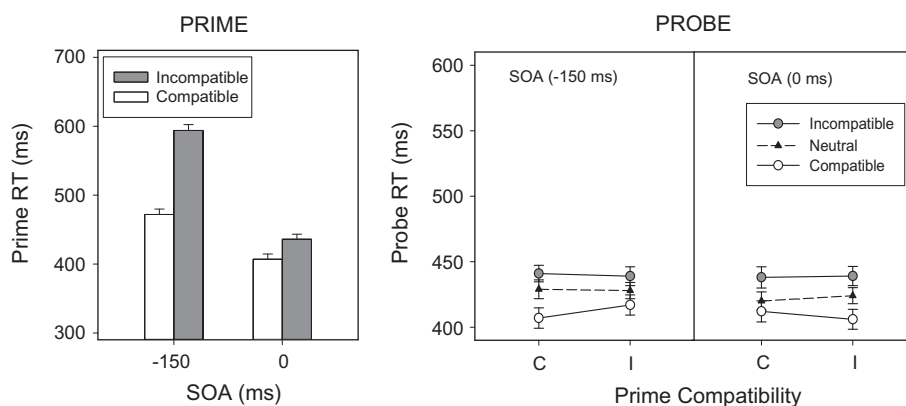


Figure 1. Mean reaction times for prime trials (as a function of compatibility and flanker-target SOA) and probe trials (as a function of compatibility of the current and the preceding trial and the SOA of the preceding trial) in Experiment 1.

Table 1. Error percentages of prime and probe trials in Experiment 1

SOA (of prime)	Prime		Probe			
	-150 ms	0 ms	-150 ms		0 ms	
			Prev. C	Prev. I	Prev. C	Prev. I
Incompatible	22.3	5.1	5.2	3.0	4.9	5.4
Neutral	–	–	4.5	3.3	3.5	4.5
Compatible	2.1	3.0	4.0	1.9	3.1	3.0

Note. C = Compatible; I = Incompatible; Prev. = Previous (Prime) trial.

Error rate differed for the SOA conditions, $F(1, 14) = 39.6$, $p < .001$ (4.1% for the SOA of 0 ms and 12.2% for the SOA of -150 ms). Furthermore, participants' responses were less errorprone on compatible (2.6%) compared to incompatible trials (13.7%), $F(1, 14) = 62.6$, $p < .001$. This flanker compatibility effect was larger when flanker stimuli preceded the target (i.e., 20.2%) than when presented together with the target (i.e., 2.1%), $F(1, 14) = 61.5$, $p < .001$, although it reached statistical significance for each of the two SOA conditions, $t(14) = 8.0$, $p < .001$ and $t(14) = 3.3$, $p < .01$, respectively.

Probe Trials

A repeated measures ANOVA on the RTs in the probe trials, including the factors Prime-trial compatibility (compatible, incompatible), Prime-trial SOA (-150 ms, 0 ms), and Probe-trial compatibility (compatible, incompatible), yielded neither a significant main effect of SOA, $F(1, 14) = 1.06$, $p = .32$ (426 ms vs. 423 ms, for SOA -150 ms and SOA 0 ms, respectively), nor of Prime-trial compatibility, $F < 1$. There was an interaction between Prime-trial SOA and Prime-trial compatibility, $F(1, 14) = 5.31$, $p = .04$, reflecting that after prime trials with the SOA of -150 ms, RTs were slightly longer when the prime was incompatible than when it was compatible (428 ms vs. 424 ms), whereas after prime trials with the SOA of 0 ms RTs were, if anything, shorter when the prime was incompatible than when it was compatible (422 ms vs. 425 ms). Nevertheless, none of the effects was statistically significant on its own, $F(1, 14) = 2.35$, $p = .15$, and $F < 1$, respectively. While responses were faster for compatible (410 ms) compared to incompatible trials (439 ms), $F(1, 14) = 57.45$, $p < .001$, this flanker compatibility effect neither depended on the Prime-trial SOA, $F < 1$, nor on the Prime-trial compatibility, $F < 1$. Of most importance regarding our research question, however, the Gratton effect differed between the SOA conditions of the prime trial, indicated by a three-way interaction, $F(1, 14) = 8.86$, $p = .01$. Analyzing trials following prime trials with a SOA of -150 ms and prime trials with a SOA of 0 ms independently revealed that the compatibility effect was reduced after an incompatible prime trial with the SOA of -150 ms, $F(1, 14) = 9.29$, $p < .01$ (probe-trial compatibility effects of 34 ms vs. 22 ms, for trials following compatible and incompatible trials, respectively), but not after an incompatible prime trial with the SOA of 0 ms,

$F(1, 14) = 1.95$, $p = .18$ (probe-trial compatibility effects of 26 ms vs. 33 ms, for trials following compatible and incompatible trials, respectively).

The ANOVA on the error data for the probe trials revealed a significant effect of probe-trial compatibility, $F(1, 14) = 8.3$, $p = .012$. More errors were committed in incompatible (4.6%) compared to compatible trials (3.0%). We did not observe an overall Gratton effect in the error data, nor was a Gratton effect modulated by prime-trial SOA, both F s < 1 .

To assess post-conflict slowing, an additional repeated measures ANOVA was conducted on the mean RTs of the neutral probe trials only, including the factors Prime-trial compatibility (compatible, incompatible) and Prime-trial SOA (-150 ms, 0 ms). Responding on neutral probe trials was slower after prime trials with the SOA of -150 ms than with the SOA of 0 ms (428 ms vs. 422 ms; $F(1, 14) = 8.30$, $p = .01$). Neither the effect of Prime-trial compatibility nor the two-way interaction with SOA was significant ($F < 1$ and $F(1, 14) = 1.07$, $p = .32$, respectively). In the error analysis, a marginally significant interaction between Prime-trial compatibility and SOA, $F(1, 14) = 3.5$, $p = .08$, emerged. This was because for the SOA of 0 ms more errors were committed after incompatible (4.5%) compared to compatible trials (3.5%), whereas for the SOA of -150 ms more errors were committed after compatible (4.5%) compared to incompatible (3.3%) trials.

Pupillometry Results

To obtain a grand average we averaged the artifact corrected pupil size curves for each condition over all participants (see Figure 2). For examining differences in prestimulus pupil size for the different conditions, we averaged the measured pupil size for each participant and condition for the time interval from -400 ms to -100 ms before stimulus onset. A repeated measures ANOVA including the factors Prime-trial compatibility (compatible, incompatible) and Prime-trial SOA (-150 ms, 0 ms) yielded a significant main effect of SOA, $F(1, 14) = 9.69$, $p < .01$ (1,383 pixels vs. 1,361 pixels, for probe trials after SOA -150 ms and SOA 0 ms prime trials, respectively), a main effect of Prime-trial compatibility, $F(1, 14) = 18.04$, $p < 0.001$ (1,361 pixels vs. 1,382 pixels, for probe trials after compatible and incompatible prime trials, respectively) and an interaction between Prime-trial SOA and

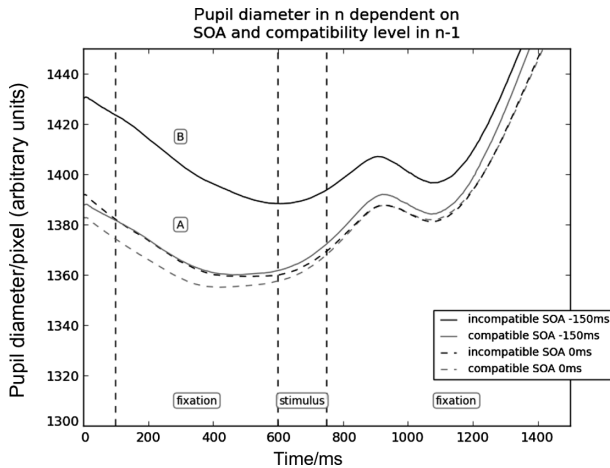


Figure 2. Pupil size averaged over all participants, as a function of compatibility and flanker-target SOA of the previous trial. The three curves for trials following 0 ms SOA compatible, 0 ms SOA incompatible, and -150 ms SOA compatible trials (A) differ from trials following -150 ms SOA incompatible trials (B). Vertical dashed lines mark trial events (see methods section for details).

Prime-trial compatibility, $F(1, 14) = 8.89$, $p < .01$, reflecting that after prime trials with a SOA of -150 ms prestimulus pupil sizes in the probe trials were larger when the prime was incompatible than when it was compatible (1,401 pixels vs. 1,364 pixels, $t(14) = 4.05$, $p < .01$), whereas after prime trials with the SOA of 0 ms no such effect was observed (1,364 pixels vs. 1,358 pixels, $t(14) = 1.24$, $p = .24$).

Discussion

The main purpose of Experiment 1 was to assess adjustment of processing strategy to conflict evoked on a preceding trial associated with varying SOA, uncontaminated by stimulus and response feature sequence effects. In addition, we investigated post-conflict slowing after both SOAs of -150 ms and 0 ms, uncontaminated by the interplay of flanker-response associations and processing selectivity. To this end, we applied a 4:4-mapping between stimuli and responses and realized only probe trials, in which neither the target/response nor the flanker was repeated from the prime and we, additionally, introduced probe trials with neutral flankers.

Replicating previous findings (Eriksen & Schultz, 1979; Flowers, 1980) the compatibility effect of the prime trials was larger when the flankers were presented in advance of the target as compared to simultaneous onset, suggesting that the conflict strength manipulation was successful. Consistent with the notion of conflict-strength-dependent adjustment, a Gratton effect occurred after trials associated

with a SOA of -150 ms but not after trials with a SOA of 0 ms. To the best of our knowledge, this is the first demonstration of a more pronounced Gratton effect under conditions of a higher degree of conflict strength, devoid of any stimulus and response feature repetitions, thus not attributable to a conflict-strength modulation of feature sequence effects. Moreover, the selective adjustment effect after prime trials associated with a SOA of -150 ms is not easily accounted for in terms of episodic retrieval of a previous control setting (e.g., Spapé & Hommel, 2008) because such retrieval should be more likely with a match than with a mismatch of context conditions between prime and probe trials. Obtaining an adjustment effect on probe trials associated with a SOA of 0 ms after prime trials associated with a SOA of -150 ms but not after prime trials associated with a SOA of 0 ms is the opposite of this prediction.

Although the same argument applies to the difference in flanker duration in trials with SOAs of 0 ms and -150 ms (which was caused by the synchronization of the offsets of targets and flankers in all trials), this aspect of our experimental setup makes it, in principle, possible that the reduction of flanker interference after prime trials with a SOA of -150 ms goes back to some form of satiation of processing stimulus information presented at the flanker positions.² Although we cannot rule out this possibility, we know of no evidence in its support.

Contrasting with the modulation of the Gratton effect by the SOA of the prime trial, prime trial SOA had no effect on general processing speed. Post-conflict-slowing neither occurred after prime trials associated with a SOA of 0 ms nor after prime trials associated with a SOA of -150 ms. This finding is not too surprising, given that post-conflict slowing has been found only occasionally. Looking at the Gratton effect after prime trials with a SOA of -150 ms reveals that it was driven by slowed responding in compatible trials after incompatible prime trials, whereas there was no corresponding speed-up of responding in incompatible trials. Lamers and Roelofs (2011) accounted for such a response pattern by assuming post-conflict slowing working in concert with attentional focusing. The absence of response slowing in neutral trials after incompatible prime trials lends no support to this notion, however. Although we can only speculate about the reason for the lacking adjustment effect in incompatible probe trials, the relatively high error rates after compatible prime trials with a SOA of -150 ms suggest the possibility that participants adopted a more liberal response criterion in this condition, trading long RTs for errors, eventually leading to RT underestimation.

Additionally to the behavioral measures in Experiment 1, we recorded pupil size data while participants performed the task. No measurable change of pupil diameter was observed in trials following low-conflict trials. In contrast, in trials following high-conflict trials (incompatible trials with a SOA of -150 ms), pupils were significantly enlarged during the prestimulus period. This result nicely mirrors the behavioral data and may reflect an increase in

² We are grateful to Robert Proctor for making us aware of this possibility.

cognitive effort in trials following high-conflict trials, leading to more selective information processing (i.e., a smaller interference effect) during these trials (see, section General Discussion for further elaboration).

Experiment 2

Experiment 1 revealed a Gratton effect after trials associated with a SOA of -150 ms in the absence of all trial-to-trial stimulus feature repetitions, thus ruling out an alternative account of stimulus/response feature integration (Hommel et al., 2004). However, elimination of feature repetitions was achieved by using pseudorandom lists of stimulus administration which introduced certain trial-to-trial contingencies. Specifically, the no feature repetition constraint confined the set of possible target-flanker conjunctions more strongly after an incompatible trial than after a compatible trial. Although it is not obvious how these contingencies should lead to a Gratton effect exclusively after trials with a SOA of -150 ms, it would be valuable to replicate the findings of Experiment 1 with a stimulus selection protocol devoid of these constraints. Therefore, we conducted a second experiment in which target and flankers were chosen independently of their occurrence on the preceding trial and analyzed only the subset of the data in which no stimulus or response feature repeated from the preceding trial (cf. Akçay & Hazeltine, 2011; Puccioni & Vallesi, 2012). To maintain a reasonable amount of data for the analyses, we increased the overall number of trials and administered no neutral trials.

Method

Participants

Twelve participants (8 female, age range 22–33, mean age) took part in this experiment in exchange for payment. All had normal or corrected-to-normal vision. The experiment lasted ca. 60 min.

Apparatus and Stimuli

A standard PC equipped with a 17 inch VGA-display and the software package E-Prime 2 (Schneider, Eschman, & Zuccolotto, 2002) was used for stimulus presentation and response sampling. Again, Landolt rings encompassing a gap at the top, left, bottom, or right were used as targets and flankers and a centrally presented target ring was flanked by four identical copies of either the same or a different ring in a cross-like fashion, thus making up compatible or incompatible conditions, respectively. Each Landolt ring had a diameter of 1 cm and the rings were arranged with a distance of 1 cm. All stimuli, including instructions and feedback, were presented in white on a gray background. We used external response keys arranged in a

cross-like fashion. Participants were instructed to press the key spatially corresponding with the gap in the target stimulus gap. The left and right keys were pressed by the middle and index finger of the left hand, respectively. The top and bottom keys were pressed by the index finger and the thumb of the right hand, respectively. Participants were instructed to respond as fast as possible and to avoid making errors.

Procedure

The experiment consisted of 24 blocks of 80 trials each. After each block, performance feedback was provided concerning the participant's mean RT and error rate. Participants were encouraged to take a short break after each block and they started each block at a self-determined point in time. Prime and probe trials occurred in alternating order. One half of the prime trials were associated with a SOA of -150 ms, in the other half of prime trials there was a SOA of 0 ms. Probe trials were always associated with a SOA of 0 ms. Each trial comprised the following events. A fixation cross occurred at the center of the screen for 200 ms. Depending on the SOA, the flankers were either presented alone for 150 ms and remained on the screen for another 150 ms together with the target or both target and flankers occurred together and remained on the screen for 150 ms. Participants had to respond within 5 s after target onset. After each response, the screen remained blank for 500 ms before the next trial started. Errors or missing responses were fed back in this time interval.

In order to obtain 50% compatible trials, we construed a trial list in which each combination of incompatible flankers in prime and probe trial occurred once. Each combination of compatible flankers in prime and incompatible flankers in probe trial and each combination of incompatible flankers in prime and compatible flankers in probe trials occurred three times. And each combination of compatible flankers in prime and probe trial occurred nine times. Together with the SOA manipulation in prime trials, this resulted in 1,152 trials possibilities. In each of the 24 blocks we randomly drew 40 pairs of prime and probe trials without replacement resulting in 960 pairs of prime-probe trials (or 1,920 single trials).

Results

For the analysis of the probe trials, only trials in which no stimulus was repeated from the previous prime trial were entered into the analysis. This led to the exclusion of 51.7% of the trials. In addition, probe trials were excluded from the analysis when participants made an error in the previous prime trial (3.3% of all probe trials). Furthermore, trials with RTs that did not fit the outlier criterion (> 2.5 *SD* for each participant and condition mean, on average 0.6%) were excluded from analysis. For the analysis of prime trials, we excluded trials with RTs deviating more than 2.5 standard deviations from the mean RT of each

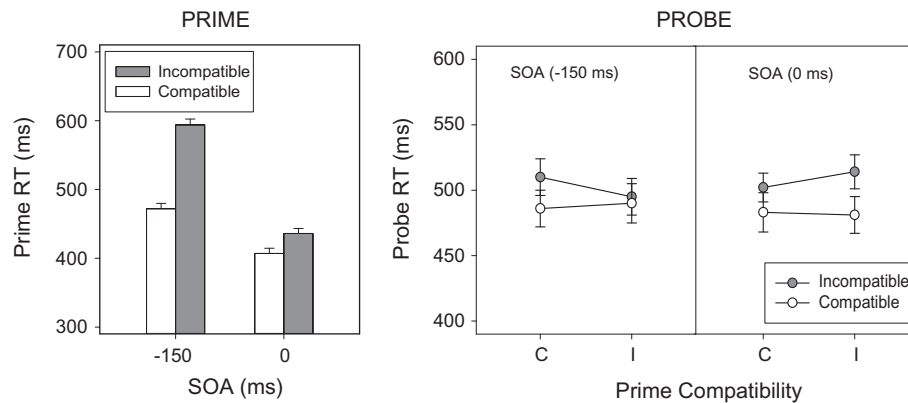


Figure 3. Mean reaction times for prime trials (as a function of compatibility and flanker-target SOA) and probe trials (as a function of compatibility of the current and the preceding trial and the SOA of the preceding trial) in Experiment 2.

condition per participant (1.6% of all prime trials). The mean RTs are presented in Figure 3, the mean error percentages are presented in Table 2.

Prime Trials

Repeated measures ANOVAs were conducted on RTs and percent error, including the factors SOA (–150 ms, 0 ms) and Flanker compatibility (compatible, incompatible). RTs differed for the SOA conditions, $F(1, 11) = 271.16$, $p < .001$ (490 ms for the SOA of 0 ms and 447 ms for the SOA of –150 ms). Furthermore, participants responded faster on compatible (437 ms) compared to incompatible trials (501 ms), $F(1, 11) = 105.36$, $p < .001$. And most importantly, the size of the flanker compatibility effect was larger when flanker stimuli preceded the target than when presented simultaneously (i.e., 108 ms for the SOA of –150 and 20 ms for the SOA of 0 ms), $F(1, 11) = 113.55$, $p < .001$, although it reached statistical significance in both SOA conditions ($t[11] = 10.81$, $p < .001$, and $t[11] = 5.82$, $p < .001$, respectively).

Error rates differed for the SOA conditions, $F(1, 11) = 20.50$, $p < .001$ (4.7% for the SOA of 0 ms and 12.4% ms for the SOA of –150 ms). Furthermore, participants' responses were less errorprone on compatible (3.0%) compared to incompatible trials (14.1%), $F(1, 11) = 25.39$, $p < .001$. This flanker compatibility effect was larger when flanker stimuli preceded the target (i.e., 20.3% for the SOA of –150 ms and 1.9% for the

SOA of 0 ms), $F(1, 11) = 26.21$, $p < .001$, although, again it reached statistical significance in both SOA conditions ($t[11] = 5.15$, $p < .001$, and $t[11] = 2.39$, $p = .036$, respectively).

Probe Trials

A repeated measures ANOVA on the RTs in the probe trials, including the factors Prime-trial compatibility (compatible, incompatible), Prime-trial SOA (–150 ms, 0 ms), and Probe-trial compatibility (compatible, incompatible), yielded neither a significant main effect of SOA, nor of Prime-trial compatibility, both F 's < 1 . The factors Prime-trial SOA and Prime-trial compatibility were close to interact, $F(1, 11) = 4.49$, $p = .058$, reflecting that after prime trials with the SOA of –150 ms, RTs were slightly longer when the prime was compatible than when it was incompatible (498 ms vs. 493 ms), whereas after prime trials with the SOA of 0 ms RTs were slightly longer when the prime was incompatible than when it was compatible (498 ms vs. 493 ms). Responses were faster for compatible (485 ms) compared to incompatible trials (505 ms), $F(1, 11) = 46.03$, $p < .001$, but this flanker compatibility effect neither depended on the Prime-trial SOA, $F(1, 11) = 2.73$, $p = .126$, nor on the Prime-trial compatibility, $F < 1$. Of most importance regarding our research question, however, the Gratton effect differed between the SOA conditions of the prime trial, indicated by a three-way interaction, $F(1, 11) = 5.97$, $p = .033$.

Table 2. Error percentages of prime and probe trials in Experiment 2

SOA (of prime)	Prime		Probe			
	–150 ms	0 ms	–150 ms		0 ms	
			Prev. C	Prev. I	Prev. C	Prev. I
Incompatible	22.6	5.7	6.1	5.0	6.0	5.4
Compatible	2.3	3.8	2.4	4.0	4.4	3.8

Note. C = Compatible; I = Incompatible; Prev. = Previous (Prime) trial.

Analyzing trials following prime trials with the SOA of -150 ms and prime trials with the SOA of 0 ms independently revealed that the compatibility effect was reduced after an incompatible prime trial with the SOA of -150 ms, $F(1, 11) = 10.00$, $p < .01$ (probe-trial compatibility effects of 23 ms vs. 5 ms, for trials following compatible and incompatible trials, respectively), but not after an incompatible prime trial with the SOA of 0 ms, $F(1, 11) = 1.80$, $p = .21$ (probe-trial compatibility effects of 18 ms vs. 33 ms, for trials following compatible and incompatible trials, respectively).

The ANOVA on the error data for the probe trials revealed only a significant effect of probe-trial compatibility, $F(1, 11) = 6.43$, $p = .028$. More errors were committed in incompatible (5.6%) compared to compatible trials (3.6%). No other effect reached significance (all F s < 1).

Discussion

Experiment 2 closely replicated the behavioral results of Experiment 1 by means of an unconstrained stimulus administration protocol (i.e., inclusion of trial-to-trial stimulus/response feature repetitions) and confinement of the analyses to the subset of trials in which no stimulus/response feature repetitions occurred. In particular, we obtained a larger compatibility effect on trials with a SOA of -150 ms as compared to 0 ms (i.e., on prime trials), presumably reflecting enhanced conflict strength evoked by advance presentation of incompatible flankers, as well as a selective Gratton effect (on probe trials) when the preceding trial was associated with a SOA of -150 ms but no Gratton effect when the preceding trial was associated with a SOA of 0 ms. The corresponding findings in Experiment 1 can, therefore, not be ascribed to the contingencies introduced by confining stimulus administration to include feature alternations only.

Contrasting with the Gratton effect found in Experiment 1, the Gratton effect found in Experiment 2 was mainly driven by speeded responses in incompatible trials following incompatible prime trials. Although both patterns of results have been taken to reflect conflict-related attentional focusing (for a discussion, see Lamers & Roelofs, 2011), this discrepancy deserves further investigation.

General Discussion

In formal models of conflict adjustment the assignment of processing weights to target and distractor stimulus information is adjusted to the strength of conflict evoked by distractor processing (Blais et al., 2007; Botvinick et al., 2001; Verguts & Notebaert, 2008, 2009). Although some studies yielded evidence consistent with this assumption (Forster et al., 2011; Purmann et al., 2009; Takezawa & Miyatani, 2005; Wühr & Ansorge, 2005), the overall picture is characterized by various heterogeneous findings. This situation may, in part, result from insufficient control of stimulus and response feature sequences in previous studies.

In search of evidence speaking to the assumption of conflict-strength-dependent adjustment, we used a manipulation widely assumed to affect the degree of response conflict evoked by flanker stimuli, that is, flanker-target SOA, exerting control of stimulus/response feature sequence by confining the administration of stimuli (Experiment 1) or the statistical analyses (Experiment 2) to complete feature alternation trials. In both experiments, presenting the flankers 150 ms in advance of the target yielded a larger flanker compatibility effect than simultaneous presentation of the target and the flankers, suggesting that the manipulation was successful in producing differential degrees of conflict strength. Supporting the notion of conflict-strength-dependent adjustment, a Gratton effect was only found after trials associated with a flanker-target SOA of -150 ms.

Moreover, the observation of a selective Gratton effect after high-conflict trials constitutes novel evidence for the notion of conflict-induced attentional focusing as such. This is because it cannot be easily explained in terms of a contingency bias which has recently been proposed to underlie the Gratton effect in studies in which stimulus/response feature sequence control was exerted. More precisely, Schmidt (2013, see also Schmidt & De Houwer, 2011) questioned the conflict adjustment interpretation of the Gratton effect on the basis of a confound between the compatibility level and the level of distractor-response contingency, which arises from the use of a 4:4 stimulus-response mapping while keeping the overall proportions of compatible and incompatible trials at 50% each. Schmidt argued that under such conditions, compatible trials are associated with higher distractor-response contingency (i.e., more predictive regarding the correct response) than incompatible trials. Because contingency effects (i.e., faster responding on trials associated with higher contingency) tend to be larger after a trial associated with low contingency than with high contingency, a confound of contingency and compatibility may produce a Gratton effect in the absence of conflict-induced attentional focusing. Although the precise mechanisms underlying the sequential modulation of the contingency effect are currently unclear, there seems no obvious reason why this sequential modulation should be enhanced after trials associated with advance presentation of the flankers.

One implication of the current study is that manipulations associated with low-conflict strength are liable to fail to produce a Gratton effect. In fact, studies that exerted careful control of stimulus/response feature sequences are usually associated with small Gratton effects (e.g., Mayr & Awh, 2009; Puccioni & Vallesi, 2012; Purmann et al., 2009; Ullsperger et al., 2005; see also Blais & Verguts, 2012) or even absence thereof (e.g., Kornblum & Stevens, 2002, Experiment 3; Schmidt & De Houwer, 2011). Indeed, in the experiments of the current study, the Gratton effect following trials with a SOA of 0 ms was numerically even reversed (see Figures 1 and 3). Such a finding might suggest that under the conditions set up in our experiments participants tended to focus attention more strongly after compatible than after incompatible trials associated with a SOA of 0 ms. However, given that the reversal was far from statistical significance in both experiments, such interpretation seems premature. Future research may help to clarify the

precise consequences of intermixing trials with different flanker-target SOAs on the deployment of attention.

The selective conflict adaptation effect after high-conflict prime trials was replicated in the data of prestimulus pupil size in Experiment 1, which was maximal after incompatible prime trials with a SOA of -150 ms, whereas no difference was found regarding the other prime trial conditions. This finding possibly reflects increased cognitive effort to achieve a higher degree of selective processing after a high-conflict trial. Some support for this interpretation comes from studies that demonstrated increased distractor interference under conditions of working memory load, suggesting that working memory load, which has been shown to yield enhanced pupil size (e.g., Kahneman & Beatty, 1966; Piquado et al., 2010), and selective attention draw on the same cognitive resource (De Fockert, Rees, Frith, & Lavie, 2001; Lavie, Hirst, de Fockert, & Viding, 2004).

Our pupillometry findings may also fit with recent ideas on conflict-induced binding of stimulus features and control settings. According to the adaptation-by-binding account (Verguts & Notebaert, 2009) conflict signals in medial prefrontal cortex trigger adaptation effects by modulating associations of task-relevant stimulus features and attention units. More specifically, conflicts are assumed to elicit an aversiveness (Dreisbach & Fischer, 2012) and arousal response (Verguts & Notebaert, 2009) that may activate the neuromodulatory locus coeruleus-norepinephrine (LC-NE) system. According to the adaptive gain theory (Aston-Jones & Cohen, 2005) the phasic release of norepinephrine by the activation of the LC specifically increases gain (i.e., signal-to-noise ratio) of task-relevant neurons in the neocortex. Therefore, arousal-triggered release of norepinephrine might serve as reinforcement signal that improves Hebbian learning of task-relevant representations which, in consequence, leads to a decreased interference effect in the immediately following trial (Verguts & Notebaert, 2009). Importantly, such conflict/arousal-induced modulations of the LC activity may be indexed by changes in pupil diameter, as LC activity and pupil diameter have been shown to be closely related (e.g., Rajkowski, Kubiak, & Aston-Jones, 1993). Our results of increased pupil size following high-conflict trials seem to support the adaptation-by-binding account, if we assume that the arousal-based LC activation leads to increased pupil size that is maintained into the next trial. Because the adaptation-by-binding account is particularly suited to account for item-specific conflict adaptation effects, examining conflict adaptation effects as a function of the pupil diameter change associated with currently presented stimuli in previous conflict trials may be a useful method to gain more insight into these processes (for similar approaches see e.g., Gilzenrat, Nieuwenhuis, Jepma, & Cohen, 2010; Jepma & Nieuwenhuis, 2011).

Acknowledgments

This research was supported by a grant of the Friedrich-Ebert-Stiftung to Sascha Purmann. Franziska Geringswald is currently supported by a grant of the Deutsche Forschungsgemeinschaft (PO 548/8-1). Mike Wendt is

currently supported by a grant of the Deutsche Forschungsgemeinschaft (WE 4105/1-1).

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Received September 21, 2012
Revision received June 5, 2013
Accepted June 6, 2013
Published online October 24, 2013

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