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Metacognition and cognitive control: behavioural adaptation requires conflict experience

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ABSTRACT

Cognitive control allows adapting our behaviour to improve performance. A behavioural signature of cognitive control is the Gratton effect. This effect is observed in conflict tasks and indicates smaller congruency effects after incongruent trials than after congruent trials. Metacognitive experience may play a role in this effect: When participants introspect on their conflict experience, the Gratton effect follows the conflict introspection instead of the stimulus congruency. However this Gratton effect could also be triggered by the labelling that the introspective method implies and/or by a misperception of the stimulus conflict. The current study investigated whether the experiential component of the introspection is necessary to trigger cognitive control or whether labelling a trial as conflicting or not can be sufficient. In a priming task, Gratton effects following metacognitive conflict experience and conflict label were contrasted. Replicating earlier reports, results showed that the metacognitive experience of conflict can trigger a Gratton effect. However a conflict label, either generated by the participants themselves or presented to the participants via feedback was not able to induce cognitive control. Results are discussed in light of current theories of cognitive control.

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In everyday life situations, we frequently need to control and adjust our behaviour to avoid conflicting responses. For example, when being confronted with an unexpected detour on our daily trip home, cognitive control interrupts our habitual—often automatic—behaviour and adjusts it to the changed situation. In the lab, one of the most prominent cognitive adaptation effects is the Gratton effect (Gratton, Coles, & Donchin, 1992). The effect reflects the observation that the influence of an irrelevant stimulus dimension is reduced on the trial following a trial where a conflict occurred. For example, in the Flanker task, participants have to respond to a central target while inhibiting flankers (e.g., <><). In this task, a congruency effect is usually observed: Reaction times (RTs) are longer, and the error rate is larger on incongruent trials (<><) than on congruent trials (>>>). The Gratton effect is the observation that a smaller congruency effect is observed after incongruent trials than after congruent trials. Similar cognitive adaptation effects have been observed in other congruency tasks like the Simon task (e.g., Notebaert & Verguts, 2011; Stürmer, Leuthold, Soetens, Schröter, & Sommer, 2002), or the Stroop task (e.g., Kerns et al., 2004), or in priming studies (e.g., Desender, Van Opstal, & Van den Bussche, 2014; van Gaal, Lamme, & Ridderinkhof, 2010).

Having a clear insight into the precise circumstances under which such cognitive adaptations are triggered is an important aim in the domain of cognitive control. According to the seminal conflict monitoring theory (Botvinick, Braver, Barch, Carter, & Cohen, 2001), the Gratton effect originates from adaptation to the processing of competing responses,

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elicited by the target and the flankers. Indeed, according to this theory, targets and flankers are automatically processed in parallel, and both activate their corresponding response. When both stimuli trigger opposite responses, this results in competition at the response level. This response competition is captured by a dedicated cognitive monitoring system. Once the conflict is detected, this monitoring system signals another cognitive module that induces behavioural adaptation on the subsequent trial by increasing the attentional focus towards the target stimuli and/or inhibiting the processing of the flanker. Both components (detection and adaptation components) would function in close interaction with each other: The higher the competition detected between the responses, the more adaptation is initiated (Kerns et al., 2004).

This subdivision between detection and adaptation opens the important and still debated issue of how the adaptation system is informed about the detection of response conflict (e.g., Alexander & Brown, 2010). The conflict monitoring theory assumes that the presentation of conflicting information in itself is sufficient to generate competition at the response level, sufficient to be picked up by the adaptation mechanism to intervene (for review, see Carter & van Veen, 2007). Recent findings, however, suggest that another element termed metacognitive experience may also play an important role in informing the adaptation system about the presence of conflict. To demonstrate this, Desender et al. (2014) used a masked priming task to investigate the role of metacognitive experience for conflict adaptation to occur. For this purpose, participants were instructed to respond to left- or right-pointing target arrows that were preceded by prime arrows presented near the threshold of awareness. These barely visible primes could be congruent (pointing in same direction as target) or incongruent (pointing in opposite direction to target) with the target, evoking a congruency effect (i.e., faster responses when responding to congruent than to incongruent trials) on the target. Crucially, after each trial, participants were asked to introspect and label the just-experienced trial as conflicting or not. This allowed investigating whether it was the actual conflict presented on the screen (i.e., a prime that points to the other direction than the target), or the metacognitive experience of this conflict (which could differ from the actual conflict) that was driving the conflict adaptation mechanism. Remarkably, conflict adaptation (i.e., the Gratton effect) was only observed after trials *experienced* as conflicting—irrespective of the presence of actual objective conflict. In other words, a smaller congruency effect was observed after congruent trials that were experienced as conflicting than after trials experienced as not conflicting, regardless of the objective congruency. This suggests that the metacognitive experience of conflict needs to be taken into account if we want to obtain a better insight in how our cognitive system optimizes our behaviour in conflicting situations. Note that, in the study of Desender et al. (2014), besides metacognitive experience, also visibility of the primes may have played an important role to initiate cognitive control mechanisms. With brief and masked presentation of the primes, it is possible that the participants incorrectly perceived some primes. For instance, a congruent prime could have been misperceived as being an incongruent one, leading in turn to a higher metacognitive experience of conflict. As such, also the Gratton effect could have been triggered by this incorrect perception instead of by the metacognitive experience of conflict. The current study aims to get a deeper understanding of the relation between metacognitive experience of conflict and cognitive control, by excluding any potential effect of prime visibility.

Metacognitive experiences are the feelings participants report being aware of during the processing of a task and task-related information (Efklides, 2008; Flavell, 1979). Inherent to the study of metacognitive experience is that the obtained measures are subjective in nature. Furthermore, there exists no direct and objective way to measure the metacognitive experience of another person (for review, see Timmermans & Cleeremans, 2015). The most direct method is to ask participants to label their experience when performing a task (e.g., Desender et al., 2014; Morsella et al., 2009; Naccache et al., 2005; Wenke, Fleming, & Haggard, 2010). So when the metacognitive experience of another person is measured, both the subjective experience itself and the labelling of this experience are involved in the process. This can be problematic, particularly when the interest is to study the influence of metacognitive experience of conflict on cognitive control. Indeed, it is possible that the labelling and not the metacognitive experience itself initiates cognitive control mechanisms. Consider again the study of Desender et al. (2014) who reported a Gratton effect depending on the level of experienced conflict. Here, participants introspected, but also labelled their experience of conflict after each trial, making it impossible to disentangle whether the experience itself or the labelling as such induced the Gratton effect. Note that experiencing and labelling of conflict are not necessarily one and the same process. Conflict labelling can potentially induce a belief about the nature of the previous experienced trial. As participants most likely trust their own label, they would believe that the label assigned to the previous trial truthfully reflects the congruency of that trial, even in the absence of any experience of conflict. The labelling of such a trial as conflicting could be sufficient to categorize it as incongruent to increase cognitive control.

Although the role of labelling has not been studied in the context of conflict adaptation, it has already been investigated in studies looking at the influence of error-related feedback. Cognitive control and behavioural adaptations are observed after people make mistakes. A typical observation is post-error slowing—that is, the finding that RTs increase after an error is made (Rabbitt, 1967). The general interpretation of post-error slowing is that participants adopt a more careful strategy (i.e., slower RTs) to avoid making similar errors again (Botvinick et al., 2001). Notebaert et al. (2009) varied the perceptual discriminability of the targets such that participants were not always able to experience themselves whether the given response was correct or not. Correct feedback about the accuracy of the response was provided after each trial. Importantly, slowing was observed in line with the feedback label, regardless of whether participants could experience the accuracy of their response or not. Apparently, participants relied on the feedback label to initiate adaptive behaviour (e.g., to slow down). Given that Gratton effects and post-error slowing are believed to rely on (partly) dissociable mechanisms (Notebaert & Verguts, 2011), it remains to be investigated whether and how these findings observed for post-error slowing generalize to the Gratton effect.

In summary, the main goal of our study is to observe whether the experiential component of the introspection is necessary to trigger cognitive control or whether a label in itself can be sufficient. In two experiments, a masked priming task with arrows is presented to the participants. As in the study of Desender et al. (2014) we investigate whether the Gratton effect is observed in relation to the metacognitively experienced conflict. Two new features were introduced. First, in order not to contaminate the metacognitive experience of conflict with erroneous visibility of the primes, we introduced "lure" trials: Even though participants believe they are looking at both primes and targets, on lure trials only targets are presented. As such, metacognitive experiences cannot originate from incorrect prime perception. Second, in parallel to post-error slowing, we explored whether the Gratton effect could be observed in the absence of a metacognitive experience of conflict but with a feedback label. In Experiment 1, a random feedback label was presented on the screen indicating whether the trial contained conflict or not. In Experiment 2, participants themselves randomly labelled trials as conflicting or not.

Experiment 1

In Experiment 1, we started from the masked priming design of Desender et al. (2014). A prime arrow was followed by a target arrow. Participants had to respond to the direction of the target. Two different conditions were created. In the first condition, the *label condition,* the computer provided an external label after each trial. This label indicated the presence or the absence of conflict. In the second condition, the *experience condition*, participants had to indicate themselves whether they thought the trial was conflicting or not. In both conditions, on half of the trials, both primes and targets were presented creating the standard congruency effect. Crucially, on the other half of the trials, only the target arrows but no primes were presented. The absence of objective congruency on these lure trials enables us to investigate whether labelling (Condition 1: *label condition*) and metacognitive experience (Condition 2: *experience condition*) can induce cognitive control in the absence of objective stimulus congruency. Importantly, in both conditions these lure trials were introduced as containing a subliminally presented prime. In the *label condition*, participants received a correct label after real prime trials but a randomly chosen "conflict" or "no conflict" label after lure trials. In the *experience condition*, participants labelled the lure trials themselves as conflicting or not. The observation of a Gratton effect after lure trials depending on the metacognitive experience would demonstrate that actual conflict is not necessary to induce cognitive control. The observation of a Gratton effect depending on the external label would demonstrate that a conflict label can be sufficient to induce cognitive control.

Method

Participants

Forty-six healthy students (7 men; $M_{age} = 18.82$ years, $SD = 1.52$) participated for course credits. All participants were naive to the purpose of the experiment.

Material

Stimuli were presented on a 19-inch LCD monitor (screen resolution: 1280 × 1024 pixels) synchronized with a refresh rate of 60 Hz. All stimuli where presented in white on a black screen and were constructed similar to the study of Desender and colleagues (2014, Experiment 1). Stimuli were white arrows (1.5° wide and 1.1° high) pointing to the left or to the right. Masks were rectangles (2.9° × 1.5°) filled with randomly selected grey, white, and black pixels. The external labels in the first condition were the words "CONFLICT" (in French: "*conflit*") and "SAME" (in French: "*même*") written in capital letters in Calibri font, size 24. Responses were recorded with an AZERTY keyboard (keys "w" and "n").

Procedure

The experiment consisted of two parts. The first part, the *label condition*, was presented as a learning phase. The second part, the *experience condition*, was presented as the test phase. Conditions were run in a fixed order. In the *label condition,* participants were instructed to pay attention to the external label. They were also instructed to use this external label to learn to subjectively "perceive" a subliminal conflict between two successive arrows. During the subsequent *experience condition*, participants themselves classified the trials as conflicting or not depending on their own metacognitive experience of conflict.

Label condition. Participants were told that two successive arrows were presented and that they had to indicate the direction of the second arrow as fast and as accurately as possible. It was explained to them that on some trials, the presentation of the first arrow would be too fast to be consciously perceived, but that the conflict label after each trial could be used to increase their sensitivity for the prime. Participants were not aware that on 50% of the trials, only a target was presented (i.e., lure trials). Half of the lure trials were randomly provided with a "conflict" label, while the other half received a "same" label. If a prime was presented, its duration was short, but long enough to be consciously perceivable on most of the trials. On these prime trials, the label was always in agreement with the actual conflict to make sure that the participants believed in the accuracy of the label.

A trial began with the presentation of a fixation cross (1000 ms) followed by the presentation of two different masks, each lasting 33 ms. Next, a prime arrow (in the case of congruent and incongruent trials) or an empty screen (in the case of lure trials) was presented for 33 ms. Two other different masks were again presented after the prime for 33 ms each. They were followed by a black screen lasting 33 ms. Finally a target arrow was presented for 160 ms. A 3000-ms response deadline was installed starting from the onset of the target. Immediately following the response, the label was provided during 1200 ms followed by the start of the next trial (see Figure 1).

Participants first completed a training phase of four randomly intermixed congruent and four incongruent trials presenting all possible combinations between the two arrows. During this first training phase, the prime was presented for 50 ms to make them clearly visible and thereby to encourage that participants trusted the label. Next, participants completed a second training part consisting of 40 trials: 10

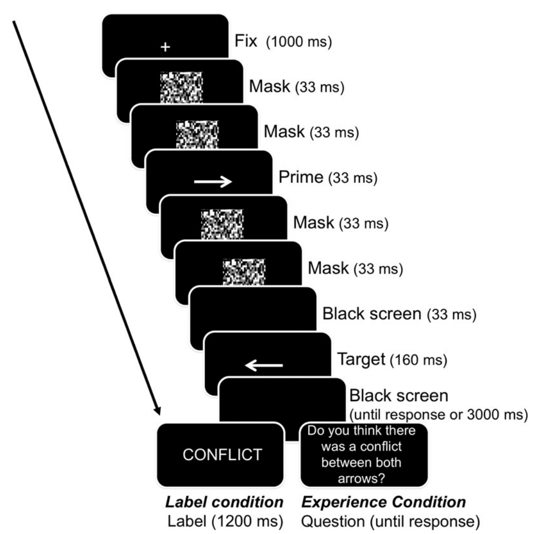

Figure 1. Example of incongruent trial for both conditions. A trial began with a fixation cross (1000 ms), followed by two masks (33 ms). Next, the prime (replaced by black screen for lure trial) appeared (33 ms), followed by two other masks (33 ms), and by a black screen (33 ms). Finally the target appeared (160 ms.) After the response of the participant, a "conflict" label appeared on the screen in the *label condition,* or the subjective question appeared in the *experience condition.*

congruent trials, 10 incongruent trials, 10 lure trials with a "same" label, and 10 lure trials with a "conflict" label. When participants made an error, an exclamation mark appeared before the label for 500 ms. After training, six experimental blocks of 60 trials each were completed. In total, the *label condition* consisted of 90 congruent trials, 90 incongruent trials, 90 lure trials with a "conflict" label, and 90 lure trials with a "same" label. Because of our interest in the Gratton effect after lure trials, the sequence of trials was pseudo-randomized so that at least 30 trials of the two types of lure trials (with "same" or "conflict" label) were followed by congruent trials, 30 were followed by incongruent trials, 15 were followed by lure trials with the "same" label, and 15 were followed by lure trials with a "conflict" label. The 30 remaining transitions were randomly determined. At the end of each experimental block, the mean RT and accuracy score appeared on the screen.

Experience condition. The second part of the experiment was presented to the participants as a test phase to observe whether participants had learned to perceive the conflict between the prime and the target arrows. Trials were the same as those in the first part of the experiment except that instead of an external label, the following question (identical to Desender et al., 2014) appeared at the centre of the screen (see Figure 1): (translated from French) "*Do you think there was a conflict between the two arrows on this trial?*". Four responses to this question were possible: (a) "conflict": I think there was a conflict; (b) "Rather conflict"*:* I don't know but I guess there was a conflict; (c) "Rather same": I don't know but I guess there was no conflict; or (d) "same": I think there was no conflict. Participants were asked to respond based on their metacognitive experience of response conflict, defined abstractly to the participant as the feeling of "something strange" or "more difficult" during their response. To avoid response interference with the subsequent trial, participants responded verbally. The experimenter encoded the response. The next trial started as soon as the response was encoded. The number of trials was the same as those in the *label condition*.

At the end of the experiment, participants were asked to explain what they believed was the aim of the experiment. This was done to verify that participants were unaware that primes were presented on half of the trials only.

Results

One participant reported at the end of the experiment that he did not believe that there was a prime on each trial. This participant was removed from the sample. Among the other participants, 31 believed that the experiment was related to the study of subliminal perception, 12 had no idea of the goal of the study; two participants believed that the study explored the capacity in dual-task performance; and one final participant believed that the study investigated the influence of the arrows on reaction times. In sum, participants did not mention any doubt concerning the presence of the primes.

Because the responses to the *experience condition* were not homogeneously distributed across the four categories, especially on lure trials (19 participants used at least one of the outer categories less than 5% of trials, and the use of the intermediate categories "rather conflict" and "rather same" varied, respectively, between 0 to 54.0% and 0 to 81.6% of trials across the participants), they were regrouped in two categories: "Conflict" and "rather conflict" were considered as "conflict" trials, and "same" and "rather same" trials were considered as "same". The data of one participant were not considered because he/she rarely experienced a conflict on lure trials, resulting in several empty cells when all factors of the experimental design were crossed (see below). On average, participants classified 36.55% ($SD = 15.73$) of lure trials as conflict trials. Of the trials with a real prime, 98.39% ($SD = 3.57$) were classified in agreement with the prime.

The first trial of each block and trials following an error or an omission were excluded from the analyses (i.e., 3.81% in the *label condition*; 2.41% in the *experience condition*) because of sequential effects induced by errors (Rabbitt, 1967).

Reaction times

A repeated measures analysis of variance (ANOVA) with *condition* (2: label/experience), *current congruency* (2: congruent/incongruent), *previous label/experience* (2: same/ conflict), and *previous trial type* (2: real prime previous trial/lure previous trial) was performed on the correct mean RTs of trials with actual prime. Trials with a real prime on the previous trial but experienced in disagreement with this prime (i.e., congruent trials experienced as "conflict" and conversely) were not considered because they were extremely rare (0.49%).

There was a significant effect of *condition*, $F(1, 43) = 44.70$, $MSE = 16{,}935.74$, $p \leq .001$, $\eta_p^2 = .51$. Participants were slower in the *experience condition* than in the *label condition* (505 ms vs. 439 ms). There was also a main effect of the *previous label/experience*, $F(1, 43) = 13.29$, $MSE = 1473.88$, $p \leq .001$, $\eta_p^2 = .24$, showing that participants were slower after a "conflict" label/experience than after a "same" label/experience (477 ms vs. 467 ms). The two-way interaction between *condition* and *previous trial type*, $F(1, 43) = 6.76$, $MSE = 1634.84$, $p = .012$, $\eta_p^2 = .14$, and the three-way interaction between *condition, previous trial type, and previous label/experience,* $F(1, 43) = 4.93$, $MSE = 1594.32$, $p = .031$, $\eta_p^2 = .10$, revealed that in the *label condition,* participants slowed down after a real prime trial (442 ms vs. 435 ms), especially when it was a "conflict" trial (454 ms vs. 431 ms). The congruency effect was significant, $F(1, 43) = 267.79$, $MSE = 8269.69$, $p \leq .001$, $\eta_p^2 = .86$, with slower responses on incongruent than on congruent trials (528 ms vs. 416 ms). This congruency effect interacted with *condition*, $F(1, 43) = 242.44$, $MSE = 2889.19$, $p \leq .001$, $\eta_p^2 = .34$. The congruency effect was larger in the *experience condition* than in the *label condition* (congruency effect: 131 ms vs. 93 ms). The congruency effect was also larger after a lure trial than after a real prime trial (congruency effect: 123 vs. 101 ms), $F(1, 43) = 30.16$, $MSE = 688.77$, $p \leq .001$, $\eta_p^2 = .41$. Importantly, the interaction between the *previous label/experience* and the *current congruency* was significant, $F(1, 43) = 43.04$, $MSE = 1727.25$, $p \leq .001$, $\eta_p^2 = .50$, suggesting the presence of a Gratton effect. However, this interaction was modulated by several higher order interactions. There was a three-way interaction involving *previous label/experience, current congruency, and previous trial type,* $F(1, 43) = 28.335$, $MSE = 2332.97$, $p \leq .001$, $\eta_p^2 = .40$. Also the three-way interaction between *condition*, *previous label/experience, and current congruency* was significant, $F(1, 43) = 4.57$, $MSE = 1193.90$, $p = .038$, $\eta_p^2 = .10$. Finally, the four-way interaction between *condition, previous label/experience, current congruency, and previous trial type*, $F(1, 43) = 4.27$, $MSE = 1271.25$, $p = .045$, $\eta_p^2 = .09$, reached significance as well. To get a clear view on these interactions, planned comparisons were performed to assess the presence of the Gratton effect in each condition after a lure or after a real prime trial. The three-way interactions between *previous label/experience*, *current congruency*, and *previous trial type* were significant in both the *label condition,* $F(1, 43) = 21.93$, $MSE = 763.61$, $p \leq .001$, $\eta_p^2 = .34$, and the *experience condition,* $F(1, 43) = 19.21$, $MSE = 2840.62$, $p \leq .001$, $\eta_p^2 = .31$. As can be seen in Figure 2, this indicates that the Gratton effect was observed following real prime trials both in the *label condition*, $F(1, 43) = 24.72$, $MSE = 1474.35$, $p \leq .001$, $\eta_p^2 = .37$, and in the *experience condition*, $F(1, 43) = 31.45$, $MSE = 3642.51$, $p \leq .001$, $\eta_p^2 = .42$. Even though present in both, this Gratton effect was stronger in the *experience condition* than in the *label condition* (102 ms vs. 58 ms).

Conversely, as can be seen in Figure 2, the Gratton effect was not observed after lure trials, neither in the *label condition*, $F(1, 43) = 0.157$, $MSE = 394.44$, $p = .694$, nor in the *experience condition,* $F(1, 43) = 0.06$, $MSE = 1014.08$, $p = .801$, with both conditions not differing from each other, $F(1, 43) = 0.0004$, $MSE = 570.25$, $p = .995$. A Bayesian repeated measures ANOVA using JASP-software (Version 0.7.1.12) with *condition*, *previous label/experience,* and *current congruency* as independent factors on correct RT of trials was performed to further establish the absence of the Gratton effect after lure trials. The inclusion Bayes factor associated with the two-way interaction between the *previous label/experience* and the *current congruency* (.086) and the inclusion Bayes factor associated with the three-way interaction between *condition*, *previous label/experience,* and *current congruency* (.026) were both below the standard threshold of .33. This analysis provided evidence for an absence of the Gratton effect after lure as well as an absence of difference between the *experience condition* and the *label condition* after lure trials.

In sum, a Gratton effect was observed after real prime trials in both the *experience* and the *label condition*. On the other hand, the Gratton effect was not observed after lure trials.

Error rate

There was a ceiling effect on congruent trials: Participants were correct on 99.47% ($SD = 1.22\%$) and 99.87% ($SD = 0.36\%$) in the *experience condition* and *label condition*, respectively. Because of this ceiling effect on congruent trials, we decided not to further analyse the Gratton effect on the error rates. Indeed, as performance can no longer improve on congruent trials, a Gratton effect could be artificially caused by a main effect of label presented on the previous trial. We still analysed the error rates but only for the incongruent trials. We conducted a repeated measures ANOVA with *condition* (2: label/experience), *previous label/experience* (2: same/conflict), and *previous trial type*

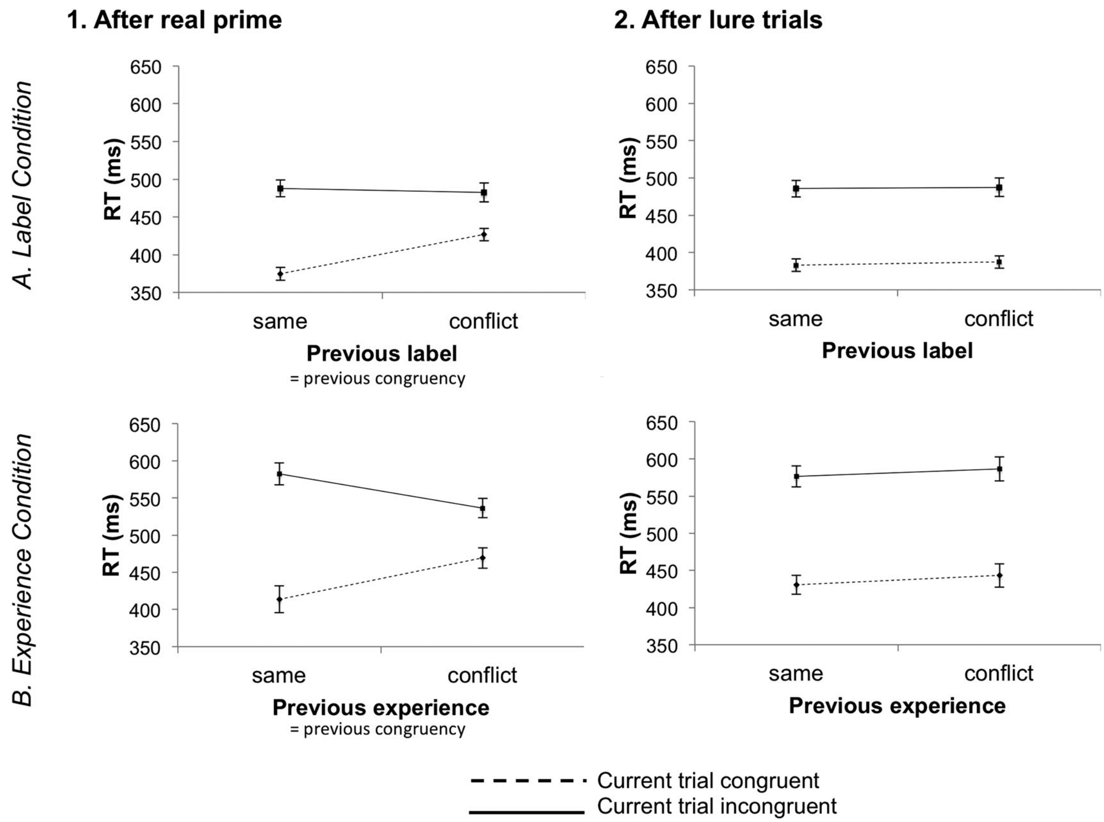

Figure 2. Experiment 1. Reaction times (RTs) as a function of *current congruency*, and *previous label or experience* in (A) *label condition* and in (B) *experience condition*, after (1) real prime trials and after (2) lure trials. After real prime trials the previous label/experience corresponds to the congruency of the previous trials. Error bars represent the standard error.

(2: real prime previous trial/lure previous trial) on the error rate of trials with actual prime.

There was a main effect of the *condition*, $F(1, 43) = 11.06$, $MSE = 155.88$, $p = .002$, $\eta_p^2 = .20$. Participants made fewer errors in the *experience condition* (7.90%) than in the *label condition* (12.32%). The main effect of the *previous label/experience* was also significant, $F(1, 43) = 40.12$, $MSE = 85.87$, $p \leq .001$, $\eta_p^2 = .48$. Participants made fewer errors after a "conflict" label/experience (6.98%) than after a "same" label/experience (13.24%). Importantly, the two-way interaction between *previous label/experience* and *previous trial type* was significant, $F(1, 43) = 29.62$, $MSE = 66.10$, $p \leq .001$, $\eta_p^2 = .41$. This interaction showed that the decrease of the error rate after a "conflict" label/experience was observed after real prime trials, $F(1, 43) = 50.53$, $MSE = 8.95$, $p < .0401$, $\eta_p^2 = .54$, but not after lure trials, $F(1, 43) = 2.21$, $MSE = 47.11$, $p = .144$. The three-way interaction was marginal, $F(1, 43) = 3.53$, $MSE = 54.67$, $p = .067$, $\eta_p^2 = .08$. Planned comparisons revealed that the decrease in errors after "conflict" real prime trials was larger in the *label condition* than in the *experience condition* (13.59% vs. 8.96%), $F(1, 43) = 4.44$, $MSE = 67.91$, $p = .040$, $\eta_p^2 = .09$. As for the reaction times, no difference was observed after lure trials, $F(1, 43) = 0.22$, $MSE = 23.61$, $p = .641$: No effect of the *previous label/experience* was observed regardless the conditions, *label condition*, $F(1, 43) = 0.99$, $MSE = 31.69$, $p = .325$; *experience condition*, $F(1, 43) = 2.00$, $MSE = 39.04$, $p = .165$. Again, this absence of effect was confirmed via a Bayesian repeated measures ANOVA with *condition* and *previous label/experience* as independent factors. Inclusion Bayes factor associated with the effect of *previous label/experience* (.326) and the Bayes factor associated with the two-way interaction between *condition* and *previous label/experience* (.260) were both below the threshold of .33. In sum, a decrease of errors was observed after real conflict trials in both conditions, whereas the errors remained the same after lure trials, regardless of the *previous label/experience*.

Interim discussion

In Experiment 1, we investigated whether a random label indicating the presence or absence of conflict was sufficient to induce a Gratton effect or whether the experiential part of the introspection was necessary to trigger the effect. For this purpose, we looked at the Gratton effect after real trials and after lure trials receiving a "conflict" or "same" random label (*label condition*) or experienced as "conflict" or "same" by the participant *(experience condition)*.

We did not observe any adaptation after lure trials receiving labels in the *label condition*. Only after real trials was adaptation observed. This suggests that a random conflict label disconnected from any metacognitive experience is not sufficient to induce cognitive control, even when it is believed to accurately indicate the conflicting nature of the primes. Indeed, all participants believed that primes were always presented and correctly indicated the presence or absence of conflict. Furthermore, given that labels were always accurate when primes were visible, there was no reason for the participants not to trust the accuracy of the labels.

It seems that the absence of the Gratton effect depending on a label could be related to recent studies suggesting that the negative emotional aspect experienced on incongruent trials is an important element to trigger adaptation (for review, see Dreisbach & Fischer, 2015). Indeed, same or conflict labels were provided randomly after each trial. This randomness essentially eliminated any systematic link between possible negative emotional experiences and the label. On the contrary, such negative emotional aspects may have been the source of the metacognitive judgement in the experience condition. However, surprisingly, in the *experience condition* of Experiment 1, adaptation was still not observed depending on the previous metacognitive experience of conflict (e.g., in contrast to Desender et al., 2014). Note that, in Experiment 1, participants always performed the *experience condition* after having performed the *label condition*. During the *label condition*, lure trials were randomly labelled as conflicting or not. Consequently, the random label most likely frequently mismatched with the metacognitive experience of the participants [e.g., I thought this was a conflict (same) trial but the label indicates it to be a same (conflict) trial]. This may have caused participants to distrust their own metacognitive experience of conflict, which could explain why it did not robustly induce adaptive behaviour in the subsequent condition. To investigate this possibility, a second experiment was conducted.

Experiment 2

In the previous experiment, the use of a random external *label condition* before the *experience condition* may have prevented participants relying on their metacognitive experience. To investigate this, an exact replication of the previous *experience condition* was run but changes were made to the *label condition*. In the *experience condition*, exactly as in Experiment 1, participants were told that a prime was presented on each trial, but that sometimes the prime was presented too briefly to be consciously perceived. Again, as in Experiment 1, participants were unaware that some trials contained no primes. Finally, after each trial, participants indicated whether they experienced conflict on that given trial. Some important changes were made to the *label condition*. Instead of receiving a random label from the computer as in Experiment 1, participants were asked to provide a random label themselves. This time, conversely to the *experience condition,* it was explicitly explained to participants that only half of the trials would contain a prime while the other half of the trials would be lure trials containing no prime (e.g., If you did not perceive a prime, this means that no prime was presented). On trials with a prime, participants were asked to indicate correctly whether the trial contained a conflict or not. On lure trials, participants were asked to randomly label the trial as "conflict" or "same". The *label condition* was adapted in such a way for two main reasons. First, this *label condition* allowed having a very similar design to Experiment 1, with both an *experience* and a *label* condition, while preventing participants from decreasing their confidence in their metacognitive experience because they were aware that the label in the *label condition* was given randomly. If adaptation can be triggered by metacognitive experience, we expect a Gratton effect in the *experience condition*. Second, this *label condition* allowed extending the results from Experiment 1. Results from Experiment 1 suggested that a conflict label, dissociated from any conflict experience, does not trigger a Gratton effect. The *label condition* of Experiment 2 allows observing whether an internally generated label (instead of a label given externally by a computer as in Experiment 1) leads to the same result.

Method

Participants

Twenty-five healthy students (4 men; $M_{age} = 19.68$ years, $SD = 1.4$) participated for course credits. All participants were naive to the purpose of the experiment.

Material and procedure

The *experience condition* was a replication of the same condition in Experiment 1. All experimental parameters remained the same with the only difference that here the question "*Do you think there was a conflict between the two arrows on this trial?*" was changed into a two-alternative forced-choice task. This was done because of the observed distribution of the subjective responses in Experiment 1 (see results of Experiment 1). During the *label condition*, participants were informed that half of the trials would not contain a prime. In that case, they were asked to randomly attach a label ("same" or "conflict") to the trial. Participants were asked to categorize about 50% of these no-prime trials as "same" and 50% as "conflict". In cases where an actual prime was presented, participants were asked to categorize the trial as containing a conflict or not, as accurate as possible. The experimental procedure was the same as that in Experiment 1. All participants performed both conditions. The order was counterbalanced. Participants who began with the *experience condition* were told that there was a prime on each trial but that this prime was presented so briefly that it could not be consciously perceived. Before the subsequent *label condition*, it was explained to participants that they had been misled. When no prime was observed, this actually indicated that no prime had been presented. So, in the *label condition*, participants were asked to correctly label the prime when it was perceived but to provide a random label if the prime was not perceived (i.e., not presented). Participants starting with the *label condition* were presented with identical instructions but in the reversed order. In the first label condition they were informed that there was no prime on half of the trials. Before the subsequent *experience condition*, they were told that they had been misled. Primes had been presented on all trials but too briefly to be consciously perceived. During the *experience condition*, participants were now asked to try to experience the conflict associated with these briefly presented primes. Again, at the end of the experiment, participants were asked to explain the goal of the experiment. This was done to verify whether participants were unaware of the fact that primes were presented only on half of the trials during the *experience condition*.

Results

All participants indicated at the end of the study that they believed the cover story: Eleven participants believed that the experiment was about consciousness and/or subliminal perception, and five participants had no idea of the goal of the experiment. The others referred to several other possibilities (i.e., the study of the effect of the laterality, the effect of dual task, the effect of attentional capacity and the study of the response times). Crucially, none of the participants indicated they doubted the presence of primes in the *experience condition*.

The data of five participants were not considered because he/she labelled or experienced lure trials as "conflict" only rarely, resulting in several empty cells when all factors of the experimental design were crossed (see below).

On average, in the *label condition*, participants labelled 32.28% ($SD = 12.87$) of lure trials as conflict trials. Of the trials with a real prime, 90.28% ($SD = 8.84$) were classified in agreement with the prime. In the *experience condition*, participants classified 32.04% ($SD = 10.82$) of lure trials as conflict trials. Of the trials with a real prime, 91.84% ($SD = 7.31$) were classified in agreement with the prime.

The first trial of each block and trials following an error or an omission were excluded from the analyses (i.e., 1.47% in the *label condition*; 1.61% in the *experience condition*). Because of technical problems in the encoding of the subjective responses, some trials were also excluded from the analyses (0.65% in the *label condition* and 0.74% in the *experience condition*).

Reaction times

As in Experiment 1, a repeated measures ANOVA with *condition* (2: label/experience), *current congruency* (2: congruent/ incongruent), *previous label/experience* (2: same/ conflict), and *previous trial type* (2: real prime previous trial/lure previous trial) was performed on the correct mean RTs of trials with actual prime. Trials with a real prime on the previous trial but labelled/ experienced in disagreement with this prime (i.e., congruent trials experienced as conflict or conversely) were not considered because of their too small number (2.86% in the *label condition*; 2.43% in the *experience condition*). Together with these factors, a first ANOVA involved order of conditions as between-subjects factor. The order of condition did not interact with any of the effects of interest and was therefore discarded from further analyses.

There was a main effect of the *previous label/experience*, $F(1, 19) = 30.71$, $MSE = 1448.2836$, $p \leq .001$, $\eta_p^2 = .62$, indicating that participants were slower after a "conflict" label/experience than after a "same" label/experience (562 ms vs. 539 ms). There was also a main effect of the *previous trial type*, $F(1, 19) = 20.00$, $MSE = 1306.20$, $p \leq .001$, $\eta_p^2 = .51$, indicating that participants were slower after lure trials than after real prime trials (559 ms vs. 541 ms). The three-way interaction between *condition, previous trial type*, and *previous label/experience* was also significant, $F(1, 19) = 7.69$, $MSE = 740.00$, $p = .012$, $\eta_p^2 = .29$. This interaction showed that in the *experience condition*, the slowing after a "conflict" experience was larger after a lure trial (33 ms) than after a real prime trial (8 ms). The congruency effect was significant, $F(1, 19) = 55.85$, $MSE = 10{,}132.90$, $p \leq .001$, $\eta_p^2 = .75$. Participants were slower on incongruent trials (592 ms) than on congruent trials (508 ms). The interaction between *previous label/experience* and *current congruency* was significant, suggesting the presence of a Gratton effect, $F(1, 19) = 43.03$, $MSE = 645.15$, $p \leq .001$, $\eta_p^2 = .69$. However, the three-way interaction between *previous label/experience, current congruency,* and *previous type*, $F(1, 19) = 33.62$, $MSE = 682.83$, $p \leq .001$, $\eta_p^2 = .65$, and the four-way interaction, $F(1, 19) = 7.00$, $MSE = 1156.28$, $p = .016$, $\eta_p^2 = .27$, were also significant. As in Experiment 1, planned comparisons were performed to analyse these interactions and to be able to assess the presence of the Gratton effect in each condition after a real prime trial or after a lure trial.

In the *label condition*, the three-way interaction between *previous label/experience, current congruency,* and *previous trial type* was significant, $F(1, 19) = 41.87$, $MSE = 709.14$, $p \leq .001$, $\eta_p^2 = .69$. As can be seen in Figure 3, the Gratton effect was significant after real prime trials, $F(1, 19) = 49.37$, $MSE = 815.73$, $p \leq .001$, $\eta_p^2 = .72$, but not after lure trials. After lure trials, a marginal interaction between the *previous label/experience* and the *current congruency* was observed in the direction opposite to what would be expected on the basis of the Gratton effect, $F(1, 19) = 3.02$, $MSE = 612.49$, $p = .098$, $\eta_p^2 = .14$.

In the *experience condition*, the three-way interaction between *previous label/experience*, *current congruency,* and *previous trial type* was not significant, $F(1, 19) = 1.80$, $MSE = 1129.97$, $p = .195$. As can been seen in Figure 3, the Gratton effect was observed both after real prime trials, $F(1, 19) = 8.76$, $MSE = 1636.15$, $p = .008$, $\eta_p^2 = .32$, and after lure trials, $F(1, 19) = 6.21$, $MSE = 503.20$, $p = .022$, $\eta_p^2 = .25$. The congruency effect was smaller after a real "conflict" trial (47 ms vs. 101 ms), but also after a lure trials experienced as "conflict" (65 ms vs. 90 ms). To assess more directly the absence of the difference between the Gratton effect after real prime trials and after lure trials in the *experience condition*, we conducted a Bayesian repeated measures ANOVA with *previous label/experience*, *current congruency,* and *previous trial type* as independent factors on correct RT of trials in the *experience condition* using JASP-software (Version 0.7.1.12). The inclusion Bayes factor associated with the three-way interaction was .457. While this Bayes factor inferior to 1 suggested more evidence for an absence of difference in the Gratton effect observed after real prime trials and lure trials, it was not below the standard threshold of .33. Thus, we cannot affirm with enough confidence that both Gratton effects, observed after real prime trials or after lure trials, were equivalent.

In sum, in the *label condition,* a Gratton effect was observed after real prime trials but not after lure trials. In the *experience condition,* a Gratton effect was observed both after real prime trials and after lure trials. No difference was observed between the Gratton effect observed after real prime trials and that after lure trials, but there was not enough evidence to conclude that both Gratton effects were identical.

Error rates

As in Experiment 1, there was a ceiling effect on congruent trials: Accuracy rate was 99.59% ($SD = 0.78$) in the *label condition*, and 99.63% ($SD = 0.92$) in the *experience condition.* Therefore, only the error rates for incongruent trials were analysed. We conducted a repeated measures ANOVA with *condition* (2: label/experience), *previous label/experience* (2: same/conflict), and *previous trial type* (2: real prime previous trial/lure previous trial) on the error rates of incongruent trials.

There was only a main effect of the *previous label/experience,* $F(1, 19) = 14.83$, $MSE = 28.96$, $p = .001$, $\eta_p^2 = .44$. Participants made more errors after a "conflict"

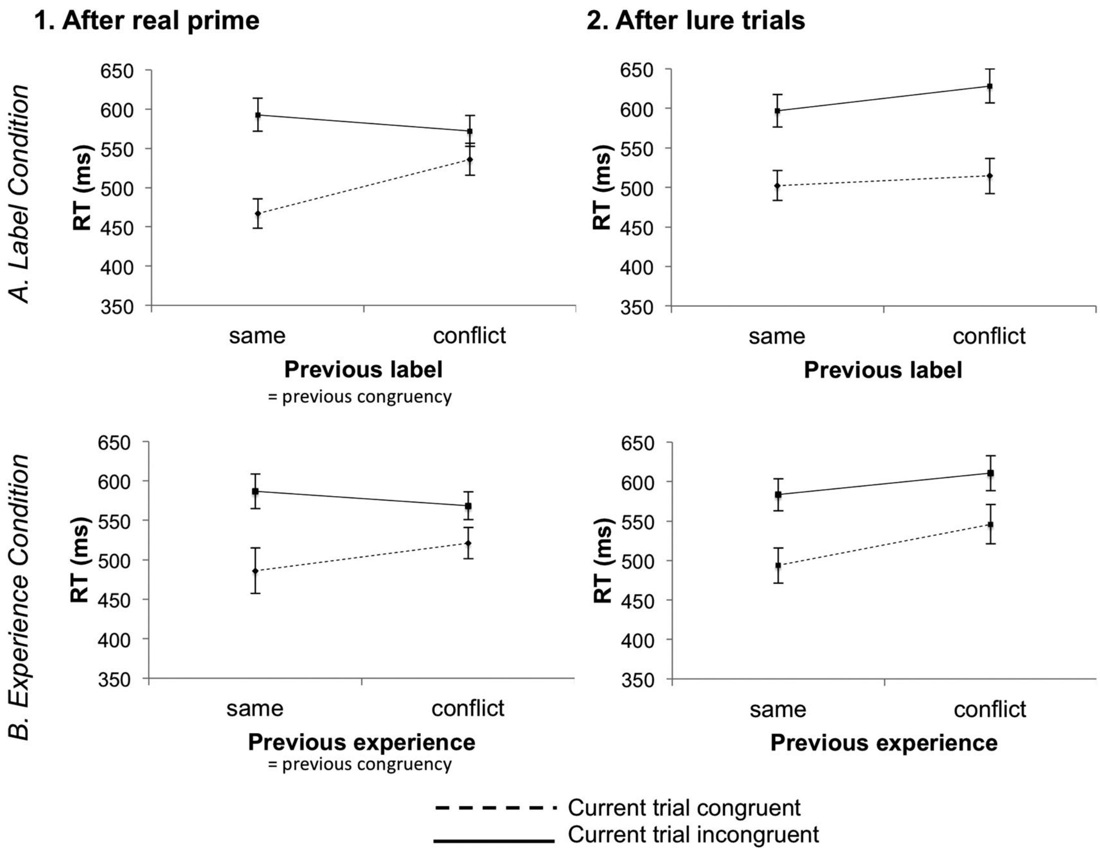

Figure 3. Experiment 2. Reaction times (RTs) as a function of *current congruency*, and *previous label or experience* in (A) *label condition* and in (B) *experience condition*, after (1) real prime trials and after (2) lure trials. After real prime trials the previous label/experience corresponds to the congruency of the previous trials. Error bars represent the standard error.

label/experience than after a "same" label/experience (5.11% vs. 1.83%). Other effects and interactions were not significant (all confirmed using inclusion Bayes factors < .33).

In sum, a decreased error rate was observed after "conflict" trials in all conditions regardless of the previous trial type.

Supplementary analysis

In Experiment 1, the Gratton effect was observed only after real prime trials. We suggested that the *label condition*, always performed before the *experience condition*, could decrease participants' confidence in their metacognitive experience. This would lead the participant to not use this experience to adapt after lure trials. To prevent this problem in Experiment 2, we changed the task context of the *experience condition*. We expected a difference between both experiments at the level of the Gratton effect observed after lure trials in each *experience condition*. Conversely to Experiment 1, the results of Experiment 2 indicated that a Gratton effect was observed both after real prime trials and after lure trials. We directly tested the significance of the difference between both *experience conditions* of both experiments. We computed the difference between the Gratton effect after real prime trials and after lure trials in each experiment and performed a one-tailed independent sample *t* test on this difference between experiments. Importantly the difference between both experiments was significant, $t(62) = 1.94$, $p = .029$, $d = 0.52$.

Interim discussion

In the *experience condition,* a Gratton effect was observed when participants experienced conflict on the previous trials even if no prime was presented on that previous trial. This suggests that objective conflict is not necessary to observe cognitive adaptation and that a subjective feeling of conflict in itself can be sufficient to induce cognitive control. The results did not show any difference between the Gratton effect triggered by real conflict trials and the Gratton effect triggered solely on the basis of metacognitive experience of conflict. This does not necessarily mean that both effects are really similar and that metacognition is the driving force of all sorts of adaptation. However, our results do imply that objective conflict is not necessary to induce cognitive adaptation. As in Experiment 1, results of the *label condition* demonstrate that merely the labelling of a trial is not sufficient to induce cognitive control. If a trial is labelled as conflicting but this label is not associated with the experience of a conflict, no adaptation occurs. The experiential part of the introspection appears to be a necessary condition for behavioural adaptation.

General discussion

The main goal of this study was to get a deeper understanding of the relation between metacognitive experience of conflict and cognitive control, without the possible confound of prime visibility. We more specifically wanted to observe whether a metacognitive experience of conflict is needed to trigger cognitive adaptation or whether a conflict label (self-generated or through feedback) in itself can be sufficient. The possible confound of prime visibility was removed by introducing lure trials, trials where no prime but only a target was presented. These lure trials were either randomly labelled by the computer (Experiment 1) or labelled by the participants themselves (Experiment 2) or evaluated by the participants according to their metacognitive experience of conflict (Experiments 1 and 2). Note that to make some of the conditions possible, participants were deceived: Most of the time, they were told that there was a prime even on lure trials. The impossibility to objectively check that participants believed in that story remains a limit of the design. However, the explicit subjective reports of the participants and the pattern of results made us confident that participants believed it.

Cognitive adaptation related to the metacognitive experience of conflict was observed in Experiment 2 but not in Experiment 1. Conflict labels, on the other hand, never resulted in cognitive adaptation. Even though participants believed that the feedback labels accurately reflected the presence or absence of conflict, this information did not induce a Gratton effect (Experiment 1). Similarly, when participants randomly labelled lure trials themselves as conflicting or not, this did not result in a Gratton effect (Experiment 2).

In Experiment 2, the Gratton effect could be related to the metacognitive experience reported on the previous trial. In other words, if the previous trial was a lure trial but associated with a metacognitive experience of conflict, then a smaller congruency effect was observed in the reaction times on the next trial. As explained above, in Experiment 1, participants made their metacognitive experience judgments *after* having performed a condition where feedback was given to lure trials indicating whether the just-experienced trial contained conflict or not. Given that this feedback was random, it frequently mismatched with the metacognitive experience of the participants. As a result, participants probably learned not to trust their own metacognitive experience of conflict. When this learning factor was removed from the design in Experiment 2, cognitive adaptation related to the metacognitive experience was observed, as in Desender et al. (2014). It thus seems important that the task context allows participants to trust their metacognitive experience to install cognitive adaptation. While we believe that trust or distrust in metacognitive experience provides a plausible explanation, further systematic research of this topic is clearly needed.

The current results extend those obtained in a previous study (Desender et al., 2014), by demonstrating that the presence of objective stimulus congruency is not needed to observe cognitive adaptation. We do not deny that the presence of an objective stimulus congruency could still have a specific influence on adaptation, but it does not seem to be a necessity. We demonstrate that the metacognitive experience of conflict by itself can be sufficient to initiate adaptive control. Additionally, because primes were absent in lure trials, this also directly demonstrates that the effect cannot be driven by an incorrect visual perception of the prime. While a (trusted) metacognitive experience of conflict can be sufficient to induce cognitive control, this seems not to be the case for conflict labels. For labels it does not seem to matter whether they are presented via external feedback (Experiment 1) or whether they are generated internally by the participants themselves (Experiment 2). Furthermore, in the first experiment, participants believed that the feedback labels were correctly indicating the presence or absence of conflict. In contrast, in the second experiment participants were asked to randomly assign conflict labels to trials, knowing that no prime had been presented. Regardless of whether participants believed the accuracy of the label or not, conflict adaptation was not observed. In sum, regardless of whether a label is provided externally or generated internally, or is believed by the participants or not, conflict labels do not seem able to induce cognitive adaptation.

For a few years, researchers have been interested in the role of awareness for cognitive adaptation (e.g., can the Gratton effect be observed, even if participants are unaware about the objective presence of conflict? For review, see Desender & Van den Bussche, 2012). Using subliminal stimuli to induce unconscious conflict, most studies did not find any adaptation when participants were unaware of conflict objectively presented on the screen (e.g., Frings & Wentura, 2008; Kunde, 2003). This led researchers to conclude that conflict adaptation is initiated only when participants are conscious about the objective conflict, and adaptation would thus be the result of an intention-mediated strategy (Kunde, 2003). More recently, however, adaptation was also observed in situations where participants were unaware of the presence of the objective conflict presented on the screen (van Gaal et al., 2010), thereby refuting the idea that intention-mediated strategies are needed to induce cognitive adaptation. Desender and colleagues (Desender, Van Opstal, Hughes, & Van den Bussche, 2016; Desender et al., 2014) showed that participants could have a metacognitive experience of conflict, even on subliminal stimulus conflict. In a priming task, participants were not able to consciously perceive the primes causing the conflict, but they still reported a larger metacognitive experience of conflict on incongruent trials, based on a general feeling of something "more difficult" or something "strange". In this case, conflict adaptation could again be interpreted as being a strategic adaptation, this time not based on awareness of the objective stimulus conflict presented on the screen, but instead based on the subjective feeling of conflict (i.e., the metacognitive experience). The current study confirms but also refines this hypothesis. Results in the *label condition* of Experiment 1 show that mere conflict awareness is not sufficient to trigger adaptation. In this condition, participants believed that the conflict label provided by the computer truthfully reflected the conflict nature of the stimulus, and this independent of their own experience. However, adaptation was not observed. Seemingly, conflict needs to be metacognitively experienced before adaptation is triggered.

In its original conception, the conflict monitoring theory (Botvinick et al., 2001) suggested that the starting point of conflict adaptation is the detection of response conflict. At that moment, the model did not take the metacognitive experience into account. More recently, to accommodate results coming from the field of decision making, an extension of the conflict monitoring theory was proposed (Botvinick, 2007). Different results indicated that the anterior cingulate cortex (ACC) was not a structure for the detection of conflict (e.g., Botvinick et al., 2001), but rather a structure evaluating action outcomes to guide decision making (e.g., Gehring & Willoughby, 2002; Holroyd & Coles, 2002; Nieuwenhuis, Yeung, Holroyd, Schurger, & Cohen, 2004). Reconciling these two perspectives, it was suggested that the ACC would detect the aversive aspect of the conflict, biasing the action towards strategies that decrease the re-occurrence of this aversive event (Botvinick, 2007). Rendering this hypothesis plausible, recent studies demonstrated that response conflict is indeed perceived as an aversive event (Dreisbach & Fischer, 2012; Fritz & Dreisbach, 2013) and that aversive events, dissociated from the conflict itself, could trigger adaptation (for review, see Dreisbach & Fischer, 2015). Additional support to emotional foundations of cognitive control was provided by Inzlicht, Bartholow, and Hirsh (2015), who reviewed converging evidences coming from cybernetics, animal research, cognitive neuroscience, and social and personality psychology, showing that emotion is an essential component of cognitive control. Emotion would act as a signal to adapt goal-directed behaviours. Such a framework could explain why metacognitive experience is able to trigger conflict adaptation. The metacognitive experience of conflict could be exactly this, an aversive signal to adapt behaviour (see Damasio, 1999, for a theory on the relations between emotion, feeling, and experience). This would also explain why a mere labelling has no influence, even when it is believed to reflect the conflicting nature of the stimulus. If a label is not experienced as an aversive signal, it would not trigger cognitive control mechanisms.

The exact origin of the metacognitive experience of conflict and its potential associated aversive effect remains an open issue. The aversive account of cognitive control suggests that the aversive affect originates from the conflict triggered by the stimulus congruency (for review, see Inzlicht et al., 2015). Incongruent trials would be processed less fluently (e.g., see Dreisbach & Fischer, 2011), would require an increased effort to be resolved (e.g., see Song & Schwarz, 2008), or would trigger a negative effect more directly. In the current study, however, participants reported an experience of conflict on trials where objective conflict was completely absent (i.e., lure trials where no prime was presented). In other words, it is impossible that objective congruency induced the experience of conflict.

Another possibility is that response conflict occurred on neutral trials for undetermined reasons, leading to the experience of conflict (Abrahamse & Braem, 2015; Desender et al., 2014; Yeung, Cohen, & Botvinick, 2011). One such undetermined reason could be the violation of expectancies: I expected a left hand response but instead, I had to press on the right side. As a result, co-activation of incompatible motor actions could occur even on lure trials. This could explain why participants sometimes reported an experience of conflict on these lure trials. This hypothesis was already suggested by Desender et al. (2014) to explain why a feeling of conflict was reported on congruent trials. Importantly, if response conflict is the cause of the metacognitive experience of conflict, this hypothesis could be used to argue that the relation between metacognitive experience and the Gratton effect is not necessarily causal (Abrahamse & Braem, 2015). If a metacognitive experience of conflict arises from the occurrence of a response conflict, mere response conflict could be the source of the Gratton effect, as it was proposed in the original version of the conflict monitoring theory. The metacognitive experience of conflict would be merely epiphenomenal, just as the conflict monitoring theory (Botvinick et al., 2001) would suggest. However, it is more difficult to imagine why undetermined factors like expectancies would differ between our two experiments. Indeed, if the undetermined factors were the same across both experiments, it is not easy to explain why cognitive adaptation was observed as a function of metacognitive experience in Experiment 2 but not in Experiment 1. Even though speculative, this combination of results seems to favour the idea that it is the metacognitive experience of conflict in itself that has a specific effect on the Gratton effect.

A third possible source of the metacognitive experience of conflict on lure trials can be identified.[1] It is possible that the reported experience of conflict is the read-out of an increased level of difficulty occurring during the inter-stimulus interval of lure trials. During this interval, participants are asked to indicate whether they perceived conflict or not. On lure trials this is relatively difficult if compared to trials where an actual prime was presented. As such, it is theoretically possible that the reported experience of conflict reflected this level of difficulty.

In sum, we observed that the presence of objective stimulus conflict is not necessary to induce cognitive adaptation: Metacognitive experience of conflict in itself can be sufficient to trigger a Gratton effect. The main contribution of our study is that we showed that the experiential part of the introspection is a necessary component to trigger this Gratton effect. Neither the labelling of the trials nor the beliefs about the conflicting nature of the trials was sufficient to induce adaptation.

Note

1. We thank an anonymous reviewer for pointing us to this possibility.

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