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Focused Cognitive Control in Dishonesty: Evidence for Predominantly Transient Conflict Adaptation

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Giving a dishonest response to a question entails cognitive conflict due to an initial activation of the truthful response. Following conflict monitoring theory, dishonest responding could therefore elicit transient and sustained control adaptation processes to mitigate such conflict, and the current experiments take on the scope and specificity of such conflict adaptation in dishonesty. Transient adaptation reduces differences between honest and dishonest responding following a recent dishonest response. Sustained adaptation has a similar behavioral signature but is driven by the overall frequency of dishonest responding. Both types of adaptation to recent and frequent dishonest responses have been separately documented, leaving open whether control processes in dishonest responding can flexibly adapt to transient and sustained conflict signals of dishonest and other actions. This was the goal of the present experiments which studied (dis)honest responding to autobiographical yes/no questions. Experiment 1 showed robust transient adaptation to recent dishonest responses whereas sustained control adaptation failed to exert an influence on behavior. It further revealed that transient effects may create a spurious impression of sustained adaptation in typical experimental settings. Experiments 2 and 3 examined whether dishonest responding can profit from transient and sustained adaption processes triggered by other behavioral conflicts. This was clearly not the case: Dishonest responding adapted markedly to recent (dis)honest responses but not to any context of other conflicts. These findings indicate that control adaptation in dishonest responding is strong but surprisingly focused and they point to a potential trade-off between transient and sustained adaptation.

Public Significance Statement

When asked a simple yes/no question, humans automatically retrieve the correct response from memory. Automatic retrieval renders honest responding smooth and efficient—but it triggers cognitive conflict whenever 1 intends to give a dishonest response. This study explored whether cognitive conflict during dishonesty can be mitigated by experimental manipulations that have been demonstrated to boost cognitive control in general. Our results highlight 1 of these manipulations to be particularly efficient: Having responded dishonestly in the immediate past helps to counter conflict. By contrast, all remaining manipulations—frequent dishonest responding or overcoming cognitive conflicts that are unrelated to dishonesty—did not make any unique contributions to the resolution of conflict during dishonesty. Countering the automatic retrieval of honest responses is only possible if recent experiences have just paved the way for giving a dishonest response.

Keywords: dishonesty, lying, deception, conflict, control adaptation

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Most people lie regularly, and many do so on a daily basis (e.g., Debey, De Schryver, Logan, Suchotzki, & Verschuere, 2015; DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996; Halevy, Shalvi, & Verschuere, 2014; Hilbig & Hessler, 2013). This renders lying an integral part of human communication and, not surprisingly, a considerable amount of research seeks to elucidate such deceptive behavior.

General theoretical frameworks highlight that deception can come in different forms, comprising not only outright lying, but also deliberate acts of withholding relevant information or strategically using other conversational norms to one's advantage (see, e.g., recent formulations of information manipulation theory; McCornack, 2015; McCornack, Morrison, Paik, Wisner, & Zhu, 2014). These different kinds of deception may differ in the motivational and cognitive processes that are involved in producing the

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deceptive response and may require individual empirical approaches.

For the present argument, we focus on outright lying, that is, delivering a factually wrong response. This type of behavior has traditionally been studied either from a motivational perspective or from a cognitive perspective. Motivational approaches to lying typically investigate situational factors, justifications, and moral considerations that will cause a given individual to lie or cheat, and they often employ economic games to incentivize dishonest responding (e.g., Fischbacher & Föllmi-Heusi, 2013; Gneezy, 2005; Levine, Kim, & Hamel, 2010). Cognitive approaches to lying, by contrast, target the cognitive processes that are assumed to mediate dishonest responding (Debey, De Houwer, & Verschuere, 2014). Rather than incentivizing dishonest responding, cognitive approaches are based on controlled laboratory tasks that isolate individual processes and therefore allow testing specific predictions from cognitive theories on deception (e.g., activation-decision-construction-action theory; Walczyk, Harris, Duck, & Mulay, 2014). Furthermore, the profound interest in the cognitive signature of lying also has potential practical applications: discovering a reliable signature of dishonesty—a cognitive counterpart of Pinocchio's long nose—would be invaluable for the development of lie detection methods.

Even though previous research did not yet uncover any index that would be as telling as Pinocchio's nose, dishonest responding has been found to recruit a series of cognitive processes that are not as involved during honest responding in a comparable way (Walczyk, Roper, Seemann, & Humphrey, 2003; Debey et al., 2014). Because of this difference, honest and dishonest behavior may be understood as being controlled by qualitatively different task sets (Debey, Liefooghe, De Houwer, & Verschuere, 2015; Foerster, Wirth, Kunde, & Pfister, 2016). Lying is set apart from honest responding because it necessarily involves an initial activation of the truthful response as stated in the "activation" component of activation-decision-construction-action theory (Walczyk et al., 2014; for a corresponding theoretical notion, see Truth-Default Theory; Levine, 2014). This initial honest action tendency has to be inhibited in order to generate a dishonest response, which is more effortful than giving in to an initial action tendency as can be done for honest responding (for a recent review and meta-analysis, see Suchotzki, Verschuere, Van Bockstaele, Ben-Shakhar, & Crombez, 2017).

Whereas other aspects, such as the construction of a plausible lie, the source of motivation or the intensity of a temptation to lie or tell the truth, can also play an important role in determining the occurrence and difficulty of dishonest responding (e.g., Hilbig, & Thielmann, 2017; Levine et al., 2010; Schindler, & Pfattheicher, 2017; Shalvi, Eldar, & Bereby-Meyer, 2012; Walczyk et al., 2003), the current experiments specifically targeted the described two-step process of activation and inhibition and associated control adaptation.

To isolate this two-step process in controlled experimental tasks, participants are usually asked to respond to simple yes/no questions about autobiographical or semantic content on a PC with keypresses. They further do not have to fear any negative consequences of their lies. In this setting, dishonest responses have been shown to be slower and less accurate than honest ones, to come with electrophysiological patterns that indicate a more difficult response retrieval and to lead to a stronger activation of brain areas that are associated with executive functions (e.g., Bhatt et al., 2009; Debey, Verschuere, & Crombez, 2012; Johnson, Barnhardt, & Zhu, 2003, 2004; Pfister, Foerster, & Kunde, 2014; Spence et al., 2001; Suchotzki, Crombez, Smulders, Meijer, & Verschuere, 2015; Walczyk et al., 2003).

These findings document that the two processing steps required for dishonest responding cause cognitive conflict as they mirror the behavioral and neurophysiological effects that have been observed in a range of cognitive conflict tasks (Botvinick, Braver, Barch, Carter, & Cohen, 2001). This two-step process seems to pose a considerable challenge for agents as performance differences between honest and dishonest responses are impressively large, often fueling arguments in favor of using such effects as a basis for lie detection (e.g., Suchotzki et al., 2017). However, the efficiency of the execution of these dishonest processes is not definite but a function of cognitive control settings and the current experiments thoroughly examine such adaptation of cognitive control in dishonest responding. Viewing dishonest responding from the perspective of cognitive conflict suggests that overcoming conflict—that is, successful dishonest responding—should leave a noticeable fingerprint on following behavior. In particular, conflict-monitoring theory assumes that cognitive conflicts can be detected and this detection leads to enhanced cognitive control (Botvinick et al., 2001), resulting in smaller conflict effects immediately after another conflict and when conflict is frequent (e.g., Logan & Zbrodoff, 1979; Gratton, Coles, & Donchin, 1992).

The current study observes dishonest responding in (dis)honest and other conflicting contexts of varying scopes and, thus, provides insight into how cognitive processing of dishonest responses adapts to a wide variety of behavioral contexts. Such a close examination of the scope and specificity of control adaptation in lying contributes to a deeper theoretical understanding of cognitive processing of dishonest responses and provides relevant insights for the development of lie detection methods. The study also puts great emphasis on methodological details of the examination of conflict contexts which might prompt a reinterpretation of previous research on frequency effects of lying and provide the groundwork for future studies on context effects of lies and other conflicts.

Such context effects have recently been reported in a range of studies that investigated how performance during lying and honest responding is affected by the recency and relative frequency of (dis)honest responding (Debey et al., 2015; Foerster et al., 2016; Van Bockstaele et al., 2012; Van Bockstaele, Wilhelm, Meijer, Debey, & Verschuere, 2015; Verschuere, Spruyt, Meijer, & Otgaar, 2011). This broader perspective provides an elaborate approach to studying the role of cognitive control for dishonest responding by addressing dynamic changes in cognitive control (i.e., *control adaptation*). Control adaptation becomes visible in improved lying performance if dishonest responses are generated frequently or have been generated immediately before. That is, whenever an agent has lied very recently or frequently, lying becomes easier and possibly even easier than telling the truth. This is a crucial finding for lie detection efforts that seek to classify truth-tellers and liars on the basis of behavioral differences originating from the mentioned effortful cognitive processing of dishonest responses. In a nutshell, a thorough understanding of the different forms of control adaptation in dishonesty and their appropriate triggers is not only motivated by basic cognitive research efforts but also warranted for the development of cognitive lie detection methods.

So as a first goal, the present experiments targeted the scope of control processes in lying, namely whether transient adaptations to recent dishonest responses and sustained adaptations to frequent dishonest responses operate independently or whether they interact with each other (Foerster et al., 2016; Van Bockstaele et al., 2012). The current study approached both control mechanisms in dishonesty in concert, to evaluate whether both types of adaptation can operate simultaneously whereas previous studies are limited by studying the impact of either recent dishonest responses or of frequent dishonest responses in separation. The conflict-monitoring theory predicts simultaneous adaptation to recent and frequent conflict as both are the result of the same mechanism, namely the detection of conflict in terms of competing response activations (e.g., Botvinick et al., 2001).

Furthermore, conflict-monitoring theory also predicts that control adaptation operates globally for all types of conflict, allowing transfer of control adaptation between types of conflicts (e.g., Botvinick et al., 2001; Kunde & Wühr, 2006; but see also Braem, Abrahamse, Duthoo, & Notebaert, 2014). As a second goal, the present experiments therefore examined the specificity of control processes in lying—that is, how dishonest conflict adapts to transient and sustained contexts of other, unrelated cognitive conflicts as induced via typical conflict tasks (Simon & Rudell, 1967; Stroop, 1935).

Experiment 1: Transient and Sustained Adaptation

Adaptation to cognitive conflict in terms of decreased congruency effects can occur either transiently, in response to recent conflict (Gratton et al., 1992), or in a sustained fashion when conflict is frequent (Logan & Zbrodoff, 1979). Because the automatically activated true answer to a question and the actual response of the participant are congruent for honest responses and incongruent for dishonest responses, we will similarly refer to the difference between dishonest and honest responses as a congruency effect. Although descriptions in terms of congruent and incongruent responses are not a common choice in the literature on dishonesty, this terminology emphasizes the potential link to control processes in other domains and it facilitates the description of the statistical analyses in the following experiments that targeted other sources of conflict besides dishonesty.

Applied to dishonesty, transient conflict adaptation becomes evident in a reduced performance difference between honest and dishonest responses immediately following a dishonest relative to an honest response. Sustained adaptation, by contrast, becomes evident in a reduced performance difference between honest and dishonest responses when dishonest responses are frequent as compared to frequent honest responding. A common method to study sustained adaptation relies on inducer stimuli and probe stimuli. Inducer stimuli are used to manipulate the overall frequency of conflict and we used this method in the current experiments by employing inducer questions that always required either an honest response or always a dishonest response. This manipulation yields frequent honest/dishonest responding but it also comes with a consistent stimulus-response pairing for each question stimulus because each inducer question always requires the same response and hence stimulus-response regularities can be learned over the course of the experiment. As such, delivering a dishonest response in a dishonest context would be easy because the dishonest response can be directly retrieved from the question and the same is true for honest responses in an honest context. In this case, question-specific learning mechanisms as well as control adaptation could be the source of adaptation effects. That is why probe questions (intermixed with inducer questions) have to be answered honestly and dishonestly with an equal frequency to separate control adaptation from question-specific learning. Answering a question with an honest response in half of the trials and with a dishonest response in the other half of trials precludes learning of a particular response to a question (see Foerster, Wirth, Herbort, Kunde, & Pfister, 2017). Indeed, increasing the frequency of dishonest responses to the inducer questions reduced the difference between honest and dishonest performance in probe questions (Van Bockstaele et al., 2012, 2015; Verschuere et al., 2011).

Even though question-specific learning mechanisms cannot drive this effect, it is not clear which top-down control mechanism is responsible for the modulation, that is, transient or sustained processes. Both could be in charge, as changing the frequency of (dis)honest responses also leads to an unbalanced set of transitions between honest and dishonest responding (Foerster et al., 2016; Van Bockstaele et al., 2012, 2015). When both intentions are instructed with the same frequency and in a random sequence, "honest → honest", "honest → dishonest", "dishonest → honest" and "dishonest → dishonest" sequences appear about equally often. When dishonest responding is more frequent than honest responding, "dishonest → honest" sequences are more likely than "honest → honest" sequences as are "dishonest → dishonest" sequences compared to "honest → dishonest" sequences. In regard to the impact of transient adaptation (e.g., Debey et al., 2015; Foerster et al., 2016), those frequent sequences render honest responding relatively difficult ("dishonest → honest" > "honest → honest") and dishonest responding relatively easy ("dishonest → dishonest" > "honest → dishonest"). For the opposite ratio with more honest than dishonest responding, in contrast, frequent sequences render honest responding relatively easy ("dishonest → honest" < "honest → honest") and dishonest responding relatively difficult ("dishonest → dishonest" < "honest → dishonest"). Thus, adaptation effects for different proportions of dishonesty could not just stem from sustained control adaptation processes, but it is also plausible to assume that transient control adaptation is the true source of this effect. As such, there would be no general change in attentional processing to favor the frequent task, but only flexible transient adaptation to the recent task (which also happens to be frequent).

Methods to disentangle the influence of transient and sustained adaptation processes have been suggested for standard conflict tasks like the Simon and the spatial Stroop task (Torres-Quesada, Funes, & Lupiáñez, 2013). In both tasks, participants were to press a left and a right key to upward and downward pointing arrows. In the Simon task, the arrows appeared either on the left or on the right side of the display, causing stimulus-response (S–R) incongruency. In the spatial Stroop task, the arrows appeared either on the upper or lower half of the display, causing stimulus-stimulus (S–S) incongruency (e.g., Kornblum, Hasbroucq, & Osman, 1990). In a training block, participants only worked on the Simon task, one group of them with a high proportion of congruent trials, and another group with a low proportion of congruent trials. In the following blocks, participants worked on a random sequence of both tasks with an equal frequency of congruent and incongruent trials. Crucially, the proportion manipulation of the Simon task in the training block transferred to the spatial Stroop task. The congruency effect was smaller for Stroop responses for participants who had responded frequently to incongruent Simon stimuli in the training block than for those who had frequently responded to congruent Simon trials. This modulation can only be attributed to sustained but not to transient control adaptation.

In a similar vein, transfer of sustained effects to a situation where transient adaptation is controlled for was examined for honest and dishonest responses (Van Bockstaele et al., 2012). In a design in which the proportion of dishonest trials was manipulated via inducer questions, sustained effects also emerged for balanced probe questions. However, in a subsequent test block, participants gave equally frequent honest and dishonest responses to both, inducer and probe questions. In this condition, and in contrast with the results obtained with standard cognitive conflicts (Torres-Quesada et al., 2013), sustained effects only emerged for inducer questions, but not for probe questions. The continued effect on inducer questions is likely driven by question-specific learning mechanisms. The absent effect on probe questions in this situation gives a further hint that proportion manipulations of dishonesty do not induce sustained but transient adaptation processes by means of changing the frequency of transitions between honesty and dishonesty (Foerster et al., 2016; Van Bockstaele et al., 2012).

In a recent study, however, transient influences were controlled for with a slightly different design: Inducer and probe trials were arranged in a fixed sequence to hold transient influences constant while examining the impact of sustained influences (Experiment 1 in Van Bockstaele et al., 2015). For example, a sequence of 10 dishonest inducer trials was followed by a sequence of 10 probe trials with honest and dishonest responses in alternation, which were again followed by a sequence of 10 dishonest inducer trials. In this setting, smaller differences between honest and dishonest responding still emerged in error rates but were not evident in reaction times (RTs) with a high frequency compared with a low frequency of dishonest responses. This modulation must stem from sustained adaptation processes as the influence of transient adaptation was held constant.

Taken together, sustained adaptation effects can emerge when transient processes cannot come into action (Experiment 1 in Van Bockstaele et al., 2015) but there are also strong hints that allegedly sustained effects could in fact stem from transient adaptation processes to dishonest conflict (Foerster et al., 2016; Van Bockstaele et al., 2012). A missing puzzle piece is the role of sustained processes when transient processes can operate as well. Do agents adapt to both, recent and frequent dishonest responding at the same time? And when they do, does adaptation to recent and frequent dishonest responding happen independently or interactively?

Conflict-monitoring theory predicts the presence of both adaptation mechanisms as they merely rely on the detection of conflict, but empirical work suggests that control adaptation does not seem to be an inevitable consequence of recent or frequent conflict experience (Botvinick et al., 2001). Studies on standard cognitive conflicts showed that sustained mechanisms seem to operate independently from transient mechanisms as they did not interact within the same task (e.g., Funes, Lupiáñez, & Humphreys, 2010). For cognitive conflict, sustained control adaptation further transferred between two tasks, while at the same time such transfer was not observed for transient control adaptation in most studies (e.g., Funes et al., 2010; Torres-Quesada et al., 2013; Torres-Quesada, Lupiáñez, Milliken, & Funes, 2014; Wühr, Duthoo, & Notebaert, 2015). These studies suggest that if adaptation to recent and frequent dishonest responding takes place, independent operations of both mechanisms but no interaction between them should be observed. However, this is not necessarily the case for dishonest responding, as the conflict that is triggered by dishonest responding differs from standard conflict tasks, like in the Simon, Eriksen or Stroop task. Whereas the conflicting information is necessary for response selection when giving unrehearsed dishonest responses (e.g., Debey et al., 2014; Walczyk et al., 2014), it can be completely ignored in the standard conflict tasks as it is not necessary to select a response (e.g., Hommel, 2011; Kornblum et al., 1990).

Experiment 1 of the present study tackled the scope of cognitive control in dishonest processing by examining whether transient and sustained adaptation emerge simultaneously and whether those two adaptation processes operate independently or in interaction. Our procedure featured simple yes/no questions about daily events and participants were cued to respond honestly or dishonestly in each trial to isolate the dishonest conflict from other processes that are involved in dishonest processing (see, e.g., Foerster et al., 2016). The proportion of dishonest trials varied between experimental blocks while honest and dishonest responses changed randomly from trial to trial. The manipulation of dishonest proportion was implemented via inducer questions whereas it was always 50/50 for probe questions to control for question-specific learning mechanism (cf. Van Bockstaele et al., 2012, 2015; Verschuere et al., 2011). Accordingly, results on inducer questions provided a manipulation check, whereas the results on probe questions were of central interest here. For these probe questions, transient and sustained effects were assessed separately. Sustained adaptation should become evident by means of a larger congruency effect in mostly honest than in mostly dishonest contexts, and this interaction effect should still be present when including sequential (transient) factors. That is, congruency effects should be smaller, both after a dishonest than after an honest response in the preceding trial, and when dishonest responses are frequent compared to frequent honest responses. However, preceding studies suggest that transient effects might play a larger role than sustained effects (Foerster et al., 2016; Van Bockstaele et al., 2015). Accordingly, transient adaptation effects should be stronger than sustained adaptation effects and both adaptation processes are expected to operate independently (e.g., Funes et al., 2010; Torres-Quesada et al., 2013; Wühr et al., 2015).

Method

Participants

Thirty-two participants (age: mean (M) = 25.9, standard deviation (SD) = 8.97; 24 female; 28 right-handed) were recruited. They gave written informed consent and received either monetary compensation or course credit. This sample size ensures a power of 80% to detect a medium effect size d_Z of about 0.5 in a two-tailed test (with α = 5%; calculated with the power.t.test function in R version 3.1.1). Medium effect sizes are a conservative estimate for effects of dishonesty on RTs and error rates and their transient and sustained modulation, because studies on all these effects observed large effects ($d_Z > 0.80$; e.g., Foerster et al., 2016; Suchotzki et al., 2017; Van Bockstaele et al., 2012). One participant of this sample was excluded from statistical analyses as the number of trials left for RT analyses was more than 2.5 standard deviations below the mean of all participants in at least one experimental cell.

Apparatus and Stimuli

Participants sat in front of a 22-in. TFT monitor. They responded to questions about daily activities from a set of 72 questions (see the Appendix). These questions were adapted from previous work (Van Bockstaele et al., 2012), translated to German and modified slightly. Participants responded with *yes* and *no* by pressing the keys *D* and *K* on and a standard German QWERTZ keyboard with their index fingers. The assignment of the responses to keys was counterbalanced across participants.

Procedure

To use an equal amount of questions about already performed and not performed activities in the experiment, participants responded to a random selection of the question pool beforehand. If participants had performed the probed action on the same day, they answered *yes,* whereas they responded *no* if they had not performed it. Participants were to respond at leisure and were strongly encouraged to contact the experimenter if they were uncertain about a response or gave a false one. The procedure stopped when participants had given 10 affirmative and 10 negative questions, respectively. The program discarded any surplus questions if more than 10 affirmative (or negative) answers had been provided before the tenth negative (or affirmative) answer.

Each trial started with a white fixation cross, centrally presented on black background for 250 ms. Then the question (font: Arial, font size: 18 pt.) appeared centrally on black background. The font color of the question was either yellow or blue and indicated whether participants were to respond honestly or dishonestly in the current trial. The assignment of congruency to color was counterbalanced across participants. Furthermore, the response labels *yes* and *no* (font: Arial, 15 pt.) were presented in the lower left and right corner of the display (centered around 25% and 75% in the horizontal and 70% in the vertical of the display) in accordance with response-key assignment. When participants responded too early (during fixation), did not respond within 3,000 ms, or provided a false response to the question, they received an appropriate error message for 1,500 ms. The next trial started after 250 ms.

In an initial practice block, participants responded to four additional questions ("Are you at a beach?", "Are you in a room?", "Are you lying down?", "Are you sitting in front of the PC?") eight times honestly and eight times dishonestly in a random order without any response deadline. In the experimental blocks, five affirmative and five negative questions were inducer questions and the remaining 10 questions were probe questions. Inducer questions afforded an unequal frequency of honest and dishonest responses whereas probe questions had balanced frequencies. In one block of the experiment, each inducer question came with an honest instruction in 80% of the trials and a dishonest instruction in 20% of the trials (low dishonest proportion). In the other block of the experiment, the relation was reversed with 20% honest trials (high dishonest proportion). Instructions of the probe questions were 50% honest and 50% dishonest in each of the two blocks. Inducer and probe questions appeared equally often within a block. Accordingly, overall 65% of the trials in the low dishonest proportion block were honest whereas 35% were in the high dishonest proportion block. A block featured 200 trials. The sequence of the dishonest proportion conditions was counterbalanced across participants. All manipulated conditions within a block followed a random sequence. Participants were offered a self-paced break after every 50th trials and between blocks.

Results

Analyses and Data Treatment

The data and the commented syntaxes with our statistical analyses of all three experiments are publicly available on the Open Science Framework (osf.io/gqv8p/). We ran two separate analyses of variance (ANOVAs) for both, the RT and the percentage error (PE) data. The first ANOVA was conducted with the within-subjects factors item (inducer vs. probe), dishonest proportion (low vs. high), and current congruency (honest vs. dishonest) to assess whether the dishonest proportion manipulation for the inducer questions transferred to probe questions. This analysis corresponds to previous assessments of sustained conflict adaptation which does not account for potential transient effects. The second ANOVA targeted sustained and transient effects simultaneously only in probe items by employing the within-subjects factors dishonest proportion (low vs. high), current congruency (honest vs. dishonest), and preceding congruency (honest vs. dishonest). Preceding congruency refers to the congruency in the preceding trial. Complete ANOVA tables can be found in Table S1 and Table S2 in the online supplemental material, accompanied by separate Bayes factors (BFs) for each effect. We scrutinized significant three-way interactions in separate 2×2 ANOVAs and two-way interactions in paired-samples *t*-tests and report BFs for these tests in the text. BFs > 3 indicate evidence in favor of the null hypothesis of no effect, whereas BFs < 0.3 indicate evidence in favor of the alternative hypothesis.

For both analyses, we excluded the first trial of each block and those following a break as well as trials that featured the same question as the preceding one to eliminate potential repetition effects (4.5% of trials). We selected trials that were correct or entailed a commission error (i.e., honest response instructed, dishonest response delivered; dishonest response instructed, honest response delivered) and followed a correct trial (12.1% trials excluded) for error analyses. For RT analyses, we also excluded all erroneous trials and those trials that followed them (20.2%). For the second ANOVA, we also excluded inducer trials. PEs were calculated as the rate of incorrect responses in relation to the remaining correct trials. Outliers were defined as RTs that deviated more than 2.5 standard deviations from their respective cell mean. Note that the number of observations for each cell differed between the two ANOVAs reported below. Accordingly, two distinct outlier identification and exclusion procedures were conducted (outlier exclusion rate was at 2.5% in the first procedure and at 2.1% in the second procedure).

Inducer Versus Probe

Table 1 depicts the mean PEs and RTs, computed separately for each combination of the factors item (inducer vs. probe), dishonest

Table 1
Mean Percentage Error and Reaction Time (Standard Deviations in Parentheses) for Each Combination of Item, Dishonest Proportion and Current Congruency and the Mean Differences (Δ; Standard Deviations in Parentheses) Between Honest and Dishonest Responses

Item	Dishonest proportion	Current congruency	Percentage error *M* (*SD*)	Percentage error Δ*M* (*SD*)	Reaction time *M* (*SD*)	Reaction time Δ*M* (*SD*)
Inducer	Low	Honest	6.5 (4.73)	4.4 (7.68)	1066.0 (165.25)	259.5 (185.45)
		Dishonest	10.9 (9.96)		1325.5 (241.76)	
	High	Honest	11.1 (9.46)	2.8 (9.42)	1197.1 (211.54)	68.2 (146.49)
		Dishonest	13.9 (8.00)		1265.3 (196.20)	
Probe	Low	Honest	5.3 (4.05)	7.1 (7.77)	1082.6 (166.10)	213.1 (142.33)
		Dishonest	12.4 (7.53)		1295.7 (201.79)	
	High	Honest	8.2 (5.85)	3.9 (6.49)	1178.2 (217.15)	110.8 (101.66)
		Dishonest	12.1 (6.97)		1289.0 (218.20)	

proportion (low vs. high) and current congruency (honest vs. dishonest). On average each cell included 37 observations.

Dishonest responses were significantly more error-prone than honest responses, $F(1, 30) = 34.74$, $p < .001$, $\eta_p^2 = .54$. Furthermore, a high proportion of dishonest trials increased PEs in comparison to a low proportion of dishonest trials, $F(1, 30) = 9.26$, $p = .005$, $\eta_p^2 = .24$. None of the remaining effects approached significance (*F*s ≤ 2.87, *p*s ≥ .101).

Dishonest responses showed increased RTs in comparison to honest responses, $F(1, 30) = 78.50$, $p < .001$, $\eta_p^2 = .72$. This main effect was qualified by a significant interaction of current congruency and dishonest proportion, $F(1, 30) = 39.79$, $p < .001$, $\eta_p^2 = .57$, as the difference between dishonest and honest responding was evident for both proportions, but considerably larger with a low proportion of dishonest trials, $t(30) = 9.15$, $p < .001$, $d = 1.64$, BF < 0.01, than with a high proportion of dishonest trials, $t(30) = 5.35$, $p < .001$, $d = 0.96$, BF < 0.01. There was only a nonsignificant trend toward a three-way interaction of all factors, $F(1, 30) = 3.51$, $p = .071$, $\eta_p^2 = .11$, and none of the remaining effects were significant (*F*s ≤ 1.50, *p*s ≥ .230).

Sustained and Transient Effects Combined

Figure 1 shows the mean PEs (upper panels A and B) and RTs (lower panels C and D) for probe items for each combination of current and preceding congruency for low (left panels A and C) and high dishonest proportion trials (right panels B and D). On average each cell included 18 observations.

Mirroring the results reported for the first ANOVA, PEs showed a main effect of current congruency, $F(1, 30) = 22.89$, $p < .001$, $\eta_p^2 = .43$. The main effect of dishonest proportion, $F(1, 30) = 1.05$, $p = .313$, $\eta_p^2 = .03$, and the interaction between current congruency and dishonest proportion, $F < 1$, were not significant. The two-way interaction between current and preceding congruency, $F(1, 30) = 21.28$, $p < .001$, $\eta_p^2 = .42$, was significant because of a considerable congruency effect after honest responses, $t(30) = 6.77$, $p < .001$, $d = 1.22$, BF < 0.01, but no such effect after dishonest responses, $t(30) = 0.51$, $p = .616$, $d = 0.09$, BF = 4.63. None of the remaining effects were significant (*F*s ≤ 1).

Again, RTs were higher for dishonest than for honest responses, $F(1, 30) = 76.91$, $p < .001$, $\eta_p^2 = .72$. The main effect of dishonest proportion, $F(1, 30) = 1.63$, $p = .211$, $\eta_p^2 = .05$, and the interaction between current congruency and dishonest proportion, $F(1, 30) = 1.71$, $p = .201$, $\eta_p^2 = .05$, were not significant. The interaction between current and preceding congruency was significant, $F(1, 30) = 78.50$, $p < .001$, $\eta_p^2 = .72$. Dishonest responding was slower than honest responding after honest responses, $t(30) = 10.88$, $p < .001$, $d = 1.95$, BF < 0.01, but a smaller reversed effect was evident after dishonest responses, $t(30) = 2.53$, $p = .017$, $d = 0.45$, BF = 0.35. None of the remaining effects were significant (*F*s ≤ 2.65, *p*s ≥ .114).

Discussion

The aim of Experiment 1 was to evaluate the scope of conflict adaptation in lying by examining whether sustained adaptation to (dis)honest contexts emerges when agents can also adapt transiently to (dis)honest contexts and whether these adaptation mechanisms work independently or interactively. That is, we investigated how dishonest responding is affected by recent and/or frequent dishonest responding. Participants showed the typical pattern of impaired performance when responding dishonestly in our experiment (e.g., Debey et al., 2015; Pfister et al., 2014; Spence et al., 2001; Suchotzki et al., 2017). At first sight, the current results also seem to corroborate previous findings on sustained adaptation, as a high dishonest proportion leads to more errors and, more importantly, diminished the congruency effect on RTs in inducer and probe questions (Van Bockstaele et al., 2012; Verschuere et al., 2011). A joint observation of transient and sustained influences for probe items showed, however, a considerable modulation of the congruency effect only through previous congruency but not through dishonest proportion.

These results demonstrate that transient modulations of the difference between honest and dishonest responding can, in principle, completely account for assumed sustained modulations (Foerster et al., 2016; Van Bockstaele et al., 2012; but see Van Bockstaele et al., 2015). As noted earlier, manipulating the frequency of dishonest and honest responding also renders certain trial sequences more frequent, whereas it decreases the frequency of other trial sequences. In low dishonest proportion blocks, an honest trial succeeds mostly another honest trial but it rarely succeeds a dishonest trial. A dishonest trial, however, mostly follows after an honest trial but rarely after another dishonest trial. So the described frequent sequences largely account for honest and dishonest means (the left pair of bars in Figure 1A and 1C), rendering honest responses relatively easy but dishonest responses relatively difficult in this condition (cf. Debey et al., 2015; Foerster et al., 2016). The very opposite is true for high dishonest proportion blocks. Now, "dishonest → honest" and "dishonest → dishonest" sequences made up for the majority of trials (the right pair of bars in Figure 1B and 1D), rendering honest responses relatively difficult but dishonest responses relatively easy. The examination of performance in regard to sustained and transient influences, thus, suggests that recent dishonesty changes honest and dishonest processing, and that this transient adaptation just happens to also appear frequently.

Although the current study manipulated the proportion of dishonest responding within-subjects, preceding studies relied on a between-subjects comparison (Van Bockstaele et al., 2012, 2015; Verschuere et al., 2011). As such, the absence of sustained effects in the current study could be the result of implementing two different proportion conditions for each participant. Following the suggestion of an anonymous reviewer, we conducted two explanatory analyses on the combined sustained and transient effects in probe trials. First, we selected only the first introduced proportion condition for each participant, thus, having a between-subjects comparison of proportion dishonest.[1] This analysis replicated the presence of transient adaptation effects and the absence of sustained adaptation effects. Second, we introduced the order of dishonest proportion as a between-subjects factor in the original analysis.[2] This analysis indicated that a high proportion of dishonest responses enhanced transient adaptation effects compared to a low proportion of dishonest responses but only for participants who started with the high dishonest proportion condition. Independent sustained adaptation effects were, however, still not evident.

Transient adaptation effects to dishonesty corroborate findings on adaptation after conflicts and, thus, point toward similar underlying control processes of dishonest responding and other conflicts (e.g., Gratton et al., 1992). It is unclear from Experiment 1, however, whether slowed dishonest responding and responses to conflicting stimuli in standard conflict tasks do indeed rely on the same control processes. One possible corollary of such a common mechanism account would predict that control adaptations in one task (e.g., responding in a dishonest trial) generalize to another task (e.g., responding in a conflict trial). Experiments 2 and 3 were designed to test whether transient and sustained control settings generalize from other conflict tasks to dishonest responding and vice versa.

Experiment 2: Transfer Between Dishonesty and Conflict From Irrelevant Stimulus Dimensions

Experiment 1 focused on adaption processes within the domain of conflict that is triggered by dishonest responding. However, there is currently no data to assess whether control adaptation processes can transfer between dishonesty and other behavioral conflicts. That is, whether dishonest responding triggers conflict adaptation for behavioral conflicts that are unrelated to lying, and vice versa.

While conflict-monitoring theory assumes that transfer of adaptation should emerge between different types of conflict (e.g., Botvinick et al., 2001), research on standard cognitive conflicts has identified conditions that render the transfer of control adaptation between different tasks and/or conflicts more or less likely. Relevant moderators for the transfer of transient and sustained control adaptation include the similarity of relevant stimulus dimensions, conflict dimensions and context as well as task boundaries (Hazeltine, Lightman, Schwarb, & Schumacher, 2011; Notebaert & Verguts, 2008; Spapé, & Hommel, 2008; Wühr et al., 2015; for a review on transient transfer effects, see Braem et al., 2014). Transfer of transient and sustained processes do not necessarily go hand in hand as transfer for one of the adaptation mechanisms can emerge while at the same time the other adaptation mechanism operates task-specifically (e.g., Torres-Quesada et al., 2013; Wühr et al., 2015).

There are also proposals and observations that particularly distinctive tasks can share control settings, presumably, because interference between the two tasks is low (Braem et al., 2014) or agents are especially motivated to use high levels of control in a task (Kleiman, Hassin, & Trope, 2014). In the latter study, control adaptation transferred from a standard letter Flanker task to a task that measures stereotypical biases by using a prime and a target (Kleiman et al., 2014, Experiment 2). The prime showed either a white or a black face, the target a weapon or a tool. Participants were to classify the targets as tools or weapons. Participants showed stereotypical biases with faster weapon- and slower tool-identification after the presentation of black compared to white faces, critically, only after congruent but not after incongruent standard Flanker trials. The authors argued that control settings might have passed from one task to the other despite their distinctiveness as people do not want to appear biased and, thus, benefit from the transfer.

The transfer of control adaptation between standard conflict tasks was the target of many empirical studies and researchers made first steps toward the definition of clear boundary conditions

[1] ANOVAs with the within-subjects factors current congruency (honest vs. dishonest), and preceding congruency (honest vs. dishonest) and the between-subjects factor dishonest proportion (low vs. high) were conducted only for the first block of each participant and, for the sake of brevity, we only report interactions relating to adaptation effects. The interaction of current and preceding congruency was significant for PEs, $F(1, 29) = 8.07$, $p = .008$, $\eta_p^2 = .22$, and RTs, $F(1, 29) = 62.55$, $p < .001$, $\eta_p^2 = .68$. The interaction of current congruency and dishonest proportion and the three-way interaction were not significant for PEs or RTs (Fs $\leq$ 1.96, ps $\geq$ .172).

[2] ANOVAs with the within-subjects factors dishonest proportion (low vs. high), current congruency (honest vs. dishonest), and preceding congruency (honest vs. dishonest) and the between-subjects factor proportion order (low first vs. high first) were conducted and, for the sake of brevity, we only report interactions relating to adaptation effects and their modulation by proportion order. PEs showed a significant interaction of current and preceding congruency, $F(1, 29) = 20.75$, $p < .001$, $\eta_p^2 = .42$, which was not further modulated by proportion order ($F < 1$). The interaction of current congruency and proportion dishonesty was not significant and was also not qualified by proportion order (Fs < 1). The interaction of current, preceding congruency and dishonest proportion as well as the four-way interaction were not significant (Fs < 1). In RTs, the interaction of current and preceding congruency was significant, $F(1, 29) = 80.14$, $p < .001$, $\eta_p^2 = .73$, but was not further modulated by proportion order, $F(1, 29) = 1.97$, $p = .171$, $\eta_p^2 = .06$. The interaction of current congruency and proportion dishonesty was not significant and was also not qualified by proportion order (Fs $\leq$ 1.66, ps $\geq$ .208). The interaction of current, preceding congruency and dishonest proportion was not significant ($F <$ 1), but the four-way interaction was significant, $F(1, 29) = 8.14$, $p = .008$, $\eta_p^2 = .22$. The interaction of current, preceding congruency and dishonest proportion was not significant in the low first condition, $F(1, 14) = 2.15$, $p = .165$, $\eta_p^2 = .13$, but in the high first order condition, $F(1, 15) = 6.86$, $p = .019$, $\eta_p^2 = .31$, as transient adaptation effects were larger in the high than in the low proportion dishonest condition, $t(15) = 2.62$, $p = .019$, $d = 0.65$.

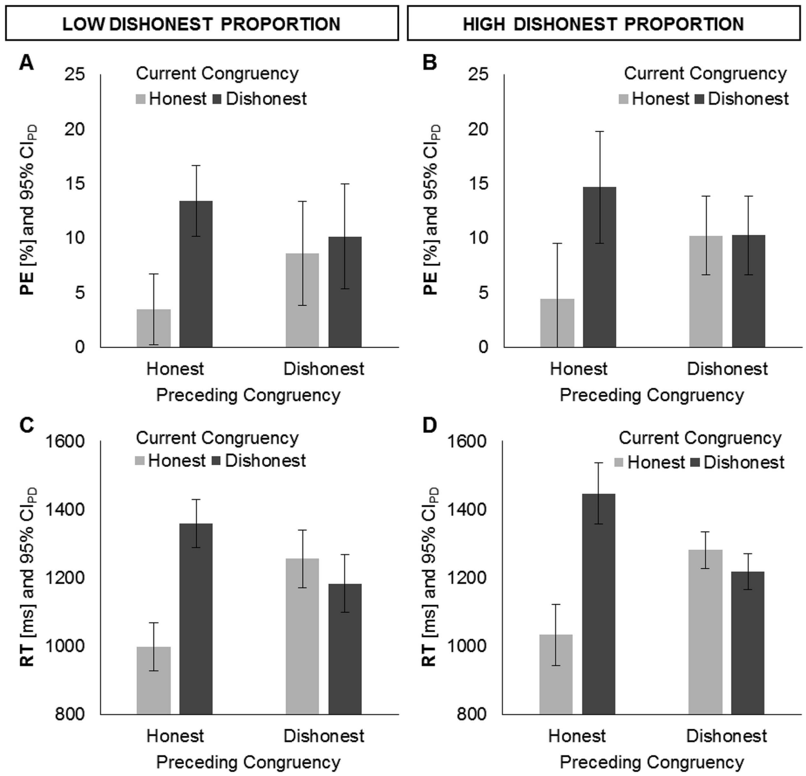

Figure 1. Mean PEs (upper panels, A and B) and RTs (lower panels, C and D) for probe items in Experiment 1, plotted as function of current congruency and preceding congruency for the low dishonest proportion (left panels, A and C) and the high dishonest proportion (right panels, B and D). Dishonest responses were more error-prone and slower than honest responses after honest responding. A reversed congruency effect was evident after dishonest responding in RTs but not in PEs. Error bars represent the 95% confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013), computed separately for preceding honest and dishonest trials in each dishonest proportion condition.

for the emergence or failure of this transfer. Still, there is plenty of work to do to understand how (un)specific control processes operate and which conditions set the parameters of the scope of transfer (e.g., Braem et al., 2014). In the same vein, previous studies on conflict adaptation in dishonesty have addressed very specific processes. Especially, as the dishonest conflict differs from the usually observed conflicts (i.e., the conflicting information is relevant for task execution), it is difficult to make a prediction about whether and how strongly control settings can transfer from the dishonest to another conflict or vice versa. While control adjustments to standard conflicts would ideally lead to a complete inhibition of the irrelevant stimulus or stimulus dimension, this is not useful for dishonest responding. For dishonest responding it would be plausible to assume that experience of dishonesty improves dishonest processing by means of facilitating the switch from the dominant honest response to the appropriate dishonest response. However, examinations about how control adaptation affects dishonest responding in particular are not available, yet. Existing evidence on stereotypical biases (Kleiman et al., 2014) suggests that transfer could take place from standard conflict tasks to responding dishonestly, as a successful liar should normally be inclined to hide dishonesty (like stereotypical biases).

To examine control transfer between conflicts, Experiment 2 combined the setup of Experiment 1 with a Stroop task. In the Stroop task, participants have to respond to the font color of a color word while ignoring the semantic meaning of the word (e.g., RED printed in blue; Stroop, 1935). So relevant and irrelevant stimulus dimensions overlap in this task and cause response conflict when these stimuli are mapped to different responses (e.g., Kornblum et al., 1990). In dishonest responding, conflict emerges from the automatic activation of the dominant truthful response and the required response that has to be derived from the dominant one (e.g., Debey et al., 2015). Even though the two tasks and their sources of conflict differ considerably, control adaptation settings could transfer between both tasks (e.g., Braem et al., 2014; Kleiman et al., 2014). The current experiment targets whether sustained control adaptation from the Stroop task can generalize to the (dis)honest task and whether transient control adaptation transfers from one of the tasks to the other in whatever direction. Therefore, both tasks appeared in a random sequence while the proportion of congruency within the Stroop task was manipulated between experimental blocks.

As previous findings suggest that transient and sustained effects should operate independently with a greater chance of transfer in the sustained domain (e.g., Funes et al., 2010; Torres-Quesada et al., 2013; Wirth, Pfister, & Kunde, 2016; Wühr et al., 2015), we expected a transfer of sustained Stroop conflict adaptation to (dis)honest responding. This should lead to smaller differences between honest and dishonest responding in the high conflict context compared to the low conflict context. In addition, if agents adapt similarly to recent dishonest and Stroop conflict, we should observe unspecific effects of transient control adaptation. Congruency effects should be smaller after dishonest and incongruent trials.

Method

Participants

A new sample of 32 participants (age: $M = 30.6$, $SD = 9.34$; 22 female; 30 right-handed) took part in the experiment for either monetary compensation or course credit. All participants gave written informed consent. Based on the same criteria as in Experiment 1, 3 participants could not be considered in the following statistical analyses.

Apparatus and Stimuli

Experiment 2 was similar to Experiment 1 except for the following changes. Participants sat in front of a 17-in. monitor and responded on and a standard German QWERTZ keyboard. In this experiment, participants responded with *yes* and *no* by pressing the keys *A* and *S* with their left middle and index finger. The assignment of the responses to keys was counterbalanced across participants. The Stroop task featured four color words and font colors: blue, brown, yellow and purple. Participants were to respond according to the font color of the color word with their right index (blue), middle (brown), ring (yellow) and little finger (purple) which rested on the adjacent keys *K*, *L*, *Ö* and *Ä*. The keys were marked with appropriately colored labels. We used four font colors, color words and responses in the Stroop task so we could select trial sequences for statistical analyses with complete stimulus alternations to control for feature integration within the Stroop task. This stimulus constellation is still confounded with the proportion manipulation as the color word is highly predictive for the response when the proportion of congruent trials is high. However, this problem only relates to effects within the Stroop task but not to the transfer effects between tasks.

Procedure

Again, participants started with a preexperimental procedure to select an equal amount of questions that asked about activities that had been performed and activities that had not been performed on the same day. In contrast to Experiment 1, a selection of 15 affirmative and 15 negative questions was taken from the question pool.

The trial procedure was the same as in Experiment 1 except that in half of the trials, a Stroop stimulus instead of a question was presented. The position of the Stroop stimulus (font: Arial, 15 pt.) was in the center of the display on black background. As in (dis)honest trials, participants had to respond within 3,000 ms in the Stroop task. Stroop and (dis)honest trials appeared in a random sequence. The font style of the question (i.e., bold or italic) indicated whether participants were to respond honestly or dishonestly in the current trial to disentangle the congruency manipulations of both tasks. The assignment of intention to font style was counterbalanced across participants. After the first practice block with questions, a new practice block introduced participants to the Stroop task. Each possible color word/font color combination appeared once, resulting in 16 practice trials without a response deadline.

Participants responded to each question equally often honestly and dishonestly within a block, whereas the proportion congruency of the Stroop task was varied between blocks. In one half of the experiment (i.e., four blocks), there was a low conflict proportion with 80% congruent Stroop trials (i.e., color word same as font color) and 20% incongruent Stroop trials (i.e., color word different than font color). In the other half of the experiment, the relation was reversed, with 20% congruent trials (high conflict proportion). A block featured 120 trials, that is, 60 (dis)honest and 60 Stroop trials. The sequence of the conflict proportion conditions was counterbalanced across participants. Between blocks and after each 40th trial, there was a self-paced break.

Results

Data Treatment

Data exclusion followed the same rules as in Experiment 1. Trials were excluded from further analyses when they featured the same task as the preceding trial with, also, either the same question (0.8%) or the same Stroop color word and/or Stroop font color to control for feature repetition effects (19.8%). Before analyzing PEs, we selected trials that were correct or entailed a commission error and followed a correct trial, and excluded all other trials (10.1% of (dis)honest trials, 9.6% of Stroop trials were excluded). For RT analyses, we excluded all erroneous trials and those trials that followed them (19.7% of (dis)honest trials, 14.0% of Stroop trials). For analyses on sustained effects (see below), 2.3% of (dis)honest trials and 2.7% of Stroop trials were identified as outliers and excluded. For analyses on transient effects, outlier exclusion amounted to 2.3% and 2.9%, respectively.

Sustained Effects

PEs and RTs were analyzed in a $2 \times 2 \times 2$ ANOVA with the within-subjects factors task ([dis]honest vs. Stroop), Stroop conflict proportion (low vs. high) and current congruency (congruent vs. incongruent). Sustained adaptation effects should produce a significant interaction between conflict proportion and current congruency. When such adaptation processes fully transferred from the Stroop to the (dis)honest task, there should be no three-way interaction between all three factors. A full ANOVA table, accompanied by BFs for each individual effect can be found in Table S3 in the online supplemental material. We scrutinized significant three-way interactions in separate 2×2 ANOVAs and two-way interactions in paired-samples *t*-tests and report BFs for these tests in the text.

Figure 2 shows the mean PEs (upper panels A and B) and RTs (lower panels C and D) for each combination of current congruency and conflict proportion for the (dis)honest task (left panels A and C) and the Stroop task (right panels B and D). On average each cell included 83 observations.

Critically, the analysis of PEs showed that neither the interaction between current congruency and conflict proportion ($F < 1$) nor

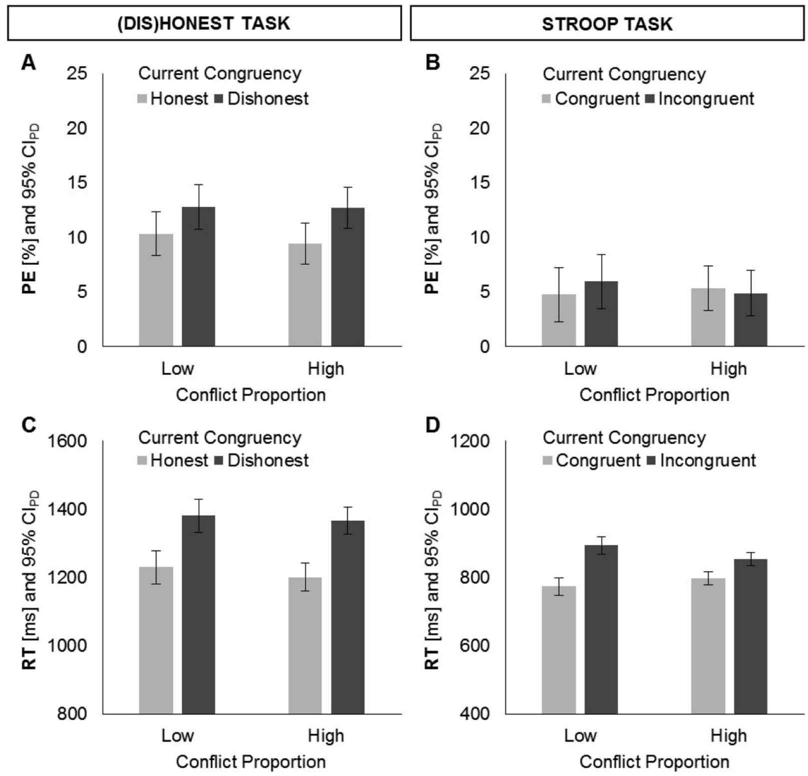

Figure 2. Sustained conflict adaptation effects on PEs (upper panels, A and B) and RTs (lower panels, C and D) in Experiment 2, plotted as function of current congruency and conflict proportion for the (dis)honest task (left panels, A and C) and the Stroop task (right panels, B and D). Note that the RT plots are scaled differently. Dishonest responses were more prone to error than honest responses but PEs in incongruent and congruent trials of the Stroop task were similar. The congruency effect in RTs was a bit larger in the Stroop than in the (dis)honest task. In the Stroop task, the congruency effect in RTs was smaller in the high than in the low conflict context but this effect did not transfer to the (dis)honest task. Error bars represent the 95% CI_{PD}, computed separately for low and high conflict proportion in each task.

the interaction between current congruency, conflict proportion and task was significant, $F(1, 28) = 1.44$, $p = .241$, $\eta_p^2 = .05$. Significant main effects of task, $F(1, 28) = 13.50$, $p = .001$, $\eta_p^2 = .33$, and current congruency, $F(1, 28) = 9.25$, $p = .005$, $\eta_p^2 = .25$ emerged. The (dis)honest task was more error-prone than the Stroop task as were dishonest/incongruent trials in comparison to honest/congruent trials. However, the two-way interaction of these factors was significant, $F(1, 28) = 4.43$, $p = .044$, $\eta_p^2 = .14$, as the difference in PEs was evident for the comparison of dishonest and honest trials, $t(28) = 3.24$, $p = .003$, $d = 0.60$, BF = 0.08, but not for the comparison of incongruent and congruent Stroop trials, $t(28) = 0.57$, $p = .574$, $d = 0.11$, BF = 4.36. None of the remaining effects were significant (Fs ≤ 0.28, ps $\geq .603$).

RTs showed a significant two-way interaction between current congruency and conflict proportion, $F(1, 28) = 5.42$, $p = .027$, $\eta_p^2 = .16$, and a significant three-way interaction of all factors, $F(1, 28) = 7.10$, $p = .013$, $\eta_p^2 = .20$. Furthermore, responses in the (dis)honest task were slower than in the Stroop task, $F(1, 28) = 144.25$, $p < .001$, $\eta_p^2 = .84$. Current congruency affected RTs, $F(1, 28) = 169.33$, $p < .001$, $\eta_p^2 = .86$, and was qualified by a significant interaction between task and current congruency, $F(1, 28) = 10.28$, $p = .003$, $\eta_p^2 = .27$. None of the remaining effects were significant (Fs ≤ 0.51, ps $\geq .479$).

Separate ANOVAs for the two tasks clarified the former three-way and two-way interactions in RTs and we only report effects that help to understand these interactions. Conflict proportion and current congruency did not interact in the (dis)honest task ($F < 1$, BF = 4.27), but it did in the Stroop task, $F(1, 28) = 33.88$, $p < .001$, $\eta_p^2 = .55$, BF = 4.27, with a larger congruency effect in low conflict proportion blocks, $t(28) = 9.77$, $p < .001$, $d = 1.81$, BF < 0.01, than in high conflict proportion blocks, $t(28) = 5.80$, $p < .001$, $d = 1.08$, BF < 0.01. The main effect of congruency was significant in both tasks, but smaller in the (dis)honest task, $F(1, 28) = 75.67$, $p < .001$, $\eta_p^2 = .73$, BF < 0.01, compared to the Stroop task, $F(1, 28) = 85.42$, $p < .001$, $\eta_p^2 = .75$, BF < 0.01.

Transient Effects

Second, a 2 × 2 × 2 × 2 ANOVA with the within-subjects factors task ([dis]honest vs. Stroop), task sequence (repetition vs. switch), current congruency (honest/congruent vs. dishonest/incongruent) and preceding congruency was conducted on PEs and RTs. The factor task sequence describes whether the preceding trial featured the same task as the current trial (repetition) or the other task (switch). Transient adaptation effects should produce a significant interaction between current and preceding congruency.

When such adaptation processes transfer between tasks, this two-way interaction should not be further qualified by task sequence. A full ANOVA table, accompanied by BFs for each individual effect can be found in Table S4 in the Supplementary Material. We scrutinized significant three-way and four-way interactions in separate planned ANOVAs and significant two-way interactions in planned paired-samples *t*-tests and report BFs for these tests in the text.

Figure 3 shows the mean PEs and Figure 4 depicts the mean RTs for each combination of current and preceding congruency for task repetitions (upper panels A and B) and task alternations (lower panels C and D) in the (dis)honest task (left panels A and C) and the Stroop task (right panels B and D). On average each cell included 42 observations.

For PEs, the two-way interaction of current and preceding congruency was significant, $F(1, 28) = 12.63$, $p = .001$, $\eta_p^2 = .31$, which was further modulated by task and task sequence, as indicated by the respective three-way interactions (Task × Current Congruency × Preceding Congruency: $F(1, 28) = 9.20$, $p = .005$, $\eta_p^2 = .25$; Task Sequence × Current Congruency × Preceding Congruency: $F(1, 28) = 16.62$, $p < .001$, $\eta_p^2 = .37$). Finally, the four-way interaction was significant, $F(1, 28) = 12.91$, $p = .001$, $\eta_p^2 = .32$. Furthermore, a switch between tasks resulted in more errors compared to task repetitions, $F(1, 28) = 9.38$, $p = .005$, $\eta_p^2 = .25$. Mirroring the PE analysis on sustained effects, the main effects of task, $F(1, 28) = 14.31$, $p = .001$, $\eta_p^2 = .34$, and current congruency, $F(1, 28) = 10.22$, $p = .003$, $\eta_p^2 = .27$, and the two-way interaction of both factors were significant, $F(1, 28) = 4.88$, $p = .036$, $\eta_p^2 = .15$. None of the remaining effects was significant (Fs ≤ 2.62, ps $\geq .117$).

Separate ANOVAs on PEs for the (dis)honest and the Stroop task were conducted to scrutinize the former interactions and, for the sake of brevity, we only report those effects that are informative to understand the former interactions. Stroop PEs showed no significant interaction of current and preceding congruency, $F(1, 28) = 1.91$, $p = .178$, $\eta_p^2 = .06$, BF = 2.15, or of current congruency, preceding congruency and task sequence, $F(1, 28) = 1.50$, $p = .231$, $\eta_p^2 = .05$, BF = 2.57. However, as the interaction of the initial ANOVA suggested, the two-way interaction between current and preceding congruency was significant for the (dis)honest task, $F(1, 28) = 15.50$, $p < .001$, $\eta_p^2 = .36$, BF = 0.02, and was also further modulated by task sequence, $F(1, 28) =$

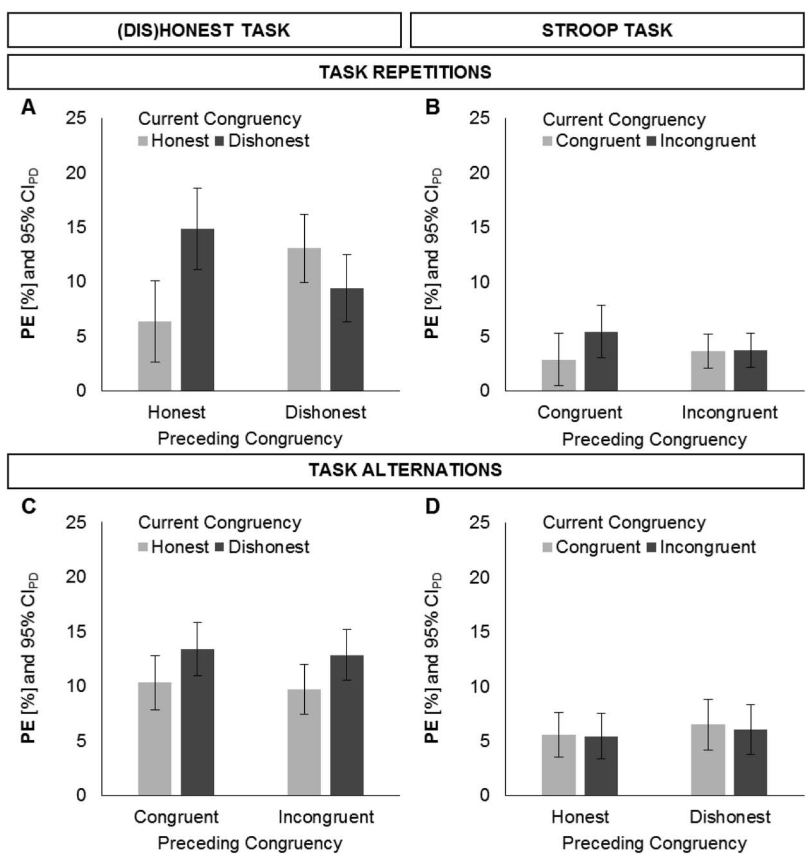

Figure 3. Transient conflict adaptation effects on PEs in Experiment 2, plotted as function of current and preceding congruency, separately for task repetitions (upper panels, A and B) and task alternations (lower panels, C and D) for the (dis)honest task (left panels, A and C) and the Stroop task (right panels, B and D). Errors only showed a modulation by current and preceding congruency when the (dis)honest task was repeated (A). After honest responses, dishonest responses were more error-prone whereas after dishonest responses honest responses were more error-prone. Error bars represent the 95% CI_{PD}, computed separately for the conditions of preceding congruency and task sequence in each task.

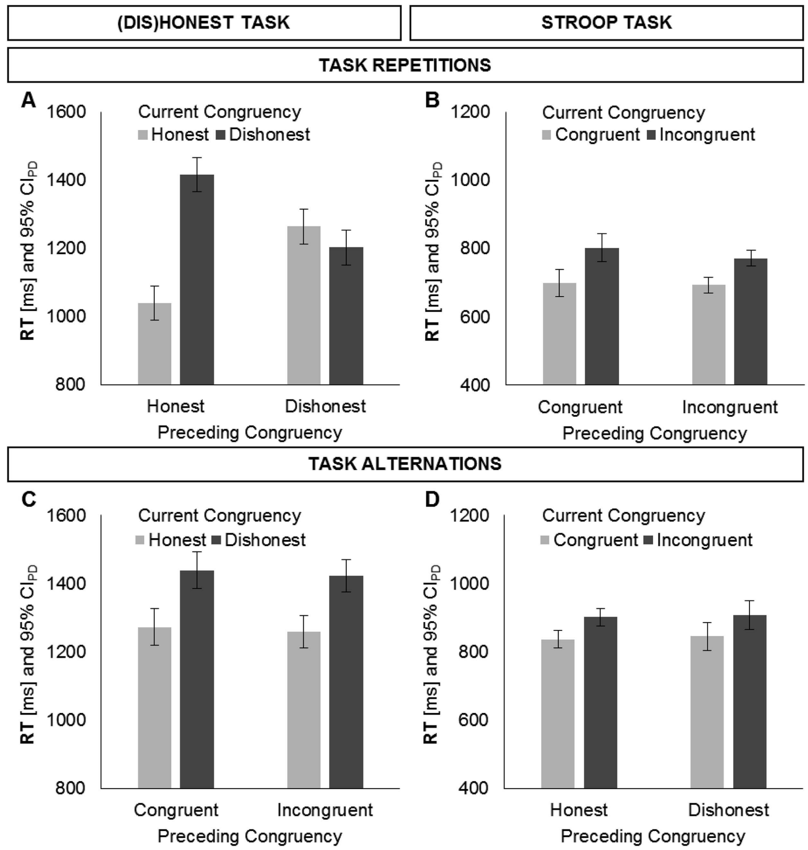

Figure 4. Transient conflict adaptation effects on RTs in Experiment 2, plotted as function of current and preceding congruency, separately for task repetitions (upper panels, A and B) and task alternations (lower panels, C and D) for the (dis)honest task (left panels, A and C) and the Stroop task (right panels, B and D). Note that the RT plots are scaled differently. RTs also only showed a modulation by current and preceding congruency when the (dis)honest task was repeated (A). After honest responses, dishonest responses were slower whereas after dishonest responses honest responses were slower. Error bars represent the 95% CI_{PD}, computed separately for the conditions of preceding congruency and task sequence in each task.

20.97, $p < .001$, $\eta^2_p = .43$, BF < 0.01. Separate ANOVAs on task repetition and switches for (dis)honest trials showed that a significant interaction of current and preceding congruency only emerged for task repetitions, $F(1, 28) = 21.99$, $p < .001$, $\eta^2_p = .44$, BF < 0.01, but not for switches, $F < 1$, BF = 5.04. When tasks repeated from the preceding to the current trial, dishonest responses were more error-prone than honest responses after honest responding, $t(28) = 4.67$, $p < .001$, $d = 0.87$, BF < 0.01, but the pattern of results was reversed after dishonest responding, $t(28) = 2.41$, $p = .023$, $d = 0.45$, BF = 0.44.

For RTs, current and preceding congruency interacted significantly, $F(1, 28) = 79.32$, $p < .001$, $\eta^2_p = .74$, however this interaction was again modulated. The three-way interactions between task, current and preceding congruency, $F(1, 28) = 171.11$, $p < .001$, $\eta^2_p = .86$, and between task sequence, current and preceding congruency, $F(1, 28) = 119.68$, $p < .001$, $\eta^2_p = .81$, as well as the four-way interaction of all factors, $F(1, 28) = 104.63$, $p < .001$, $\eta^2_p = .79$, were significant. Switches between tasks prolonged responses compared to task repetitions, $F(1, 28) = 141.41$, $p < .001$, $\eta^2_p = .84$. In line with the RT analysis on sustained effects, there were significant main effects of task, $F(1, 28) = 146.03$, $p < .001$, $\eta^2_p = .84$, and current congruency, $F(1, 28) = 153.91$, $p < .001$, $\eta^2_p = .85$, as well as a significant two-way interaction of these factors, $F(1, 28) = 13.91$, $p = .001$, $\eta^2_p = .33$. None of the remaining effects was significant (Fs $\leq$ 2.06, ps $\geq$ .162).

A separate statistical assessment of the RTs of the two tasks scrutinized the former interactions and again, we only report those effects here that are informative to understand the former interactions. Current and preceding congruency did not interact in Stroop trials, $F(1, 28) = 1.37$, $p = .251$, $\eta^2_p = .05$, BF = 2.73, and this interaction was also not qualified by task sequence, $F < 1$, BF = 3.29. In (dis)honest trials, however, this two-way interaction, $F(1, 28) = 149.85$, $p < .001$, $\eta^2_p = .84$, BF < 0.01, and the three-way interaction of task sequence, current and preceding congruency, $F(1, 28) = 167.04$, $p < .001$, $\eta^2_p = .86$, BF < 0.01, were significant. Separate ANOVAs for task repetitions and task switches in dishonest trials showed that preceding congruency did not affect current congruency when tasks switched from the preceding to the current trial, $F < 1$, BF = 5.02. The two-way interaction was significant for task repetitions, $F(1, 28) = 295.20$, $p < .001$, $\eta^2_p = .91$, BF < 0.01, as dishonest responses were slower than honest responses after honest responding, $t(28) = 15.59$, $p < .001$, $d = 2.89$, BF < 0.01, but an opposite effect was evident after dishonest responding, $t(28) = 2.46$, $p = .021$, $d = 0.46$, BF $=$ 0.40.

Discussion

Experiment 2 combined dishonest and Stroop conflict to explore potential transfer of transient and sustained adaptation processes and, thus, the specifity of conflict adaptation in lying. That is, the experiment was set up to show whether the difference between honest and dishonest responding would be smaller in a highly incongruent Stroop environment, or directly after an incongruent Stroop trial, compared to frequent or recent congruent Stroop conditions. Similarly, the experiment examined whether the congruency effect in the Stroop task would be modulated by recent dishonesty. Even though transfer of transient and sustained control adaptation between such different tasks is possible (e.g., Hazeltine et al., 2011; Kleiman et al., 2014; Wirth et al., 2016; Wühr et al., 2015), there was no transfer of transient control adaptation between both tasks in either direction and also no transfer of sustained effects from the Stroop task to the (dis)honest task.

It is important to note here, that Experiment 2 used separate response keys for the (dis)honest and the Stroop tasks. In contrast, studies that found transfer of transient control adaptation between different tasks with distinct conflict sources used same response keys for those tasks (Hazeltine et al., 2011; Kleiman et al., 2014). Using same response keys could lead to less salient task boundaries and thus to an enhanced probability of transferring control adaptation (Hazeltine et al., 2011). Transfer of sustained effects, however, was also observed when tasks, conflict sources and responses differed like in the present experiment (Experiment 3 of Wühr et al., 2015).

Like in Experiment 1, transient adaptation was again found within the (dis)honest task, that is, dishonest responding was slower and less accurate than honest responding after an honest response but this effect was absent or even reversed after dishonest responses (cf. Debey et al., 2014; Foerster et al., 2016). In contrast, transient adaptation processes did not emerge in the Stroop task. It is difficult to interpret that null effect. Increased error rates and RTs in the (dis)honest compared to the Stroop task indicate that the former task execution was more difficult than the latter. This might have increased saliency of the (dis)honest task with a prioritization of control adaptation in this task. Note also, that sustained effects within the Stroop task do not necessarily derive from control adaptation but could stem from learning frequent S–R pairings in the low conflict proportion condition.

In a nutshell, there was no evidence of a transient or sustained transfer of control adaptation from the Stroop task to the (dis)honest task or of a transient transfer in the opposite direction in the present experiment. However, this does not preclude the existence of general adaptation processes. The tasks of Experiment 2 featured different relevant stimulus dimensions, task rules, sources of conflict and responses. In Experiment 3, we therefore replaced the Stroop task with an S–R compatibility task that is more similar to dishonest responding (e.g., Davidson, Amso, Anderson, & Diamond, 2006) to render transfer of adaptation settings more likely. Most importantly, as in dishonest responding, the conflicting information in the conflict task was now relevant for task execution.

Experiment 3: Transfer Between Dishonesty and Conflict From Relevant Stimulus Dimensions

Experiment 3 combined the (dis)honest task with a location task in which participants responded to the left and right location of a square with a response on the same or on the opposite side. Four color cues indicated same or opposite responses in each trial and the proportion of conflict in the location task varied between blocks. Here, the conflict source in the location task was very similar to the one in dishonest responding. In both cases, conflict emerges from the automatic activation of the dominant response (truthful response vs. location-congruent response) and the required response that has to be derived from the dominant one. However, the dominant information in the location task is exogenously triggered from stimulus position, whereas it is endogenously triggered for dishonest responding, as the honest response is derived from question content and memory. In both cases, the conflict then emerges because an endogenous rule urges an opposing response.

A recent study suggests that both conflicts could share common adaptation mechanisms (Experiment 3 and 4 of Foerster et al., 2016). One group of participants in this study received honest and dishonest instructions to respond to simple yes/no questions while another group of participants received instructions that they should respond to questions from the perspective of two different agents. One of the agents supposedly shared the same experiences as the participants, whereas the other agent had opposite experiences. Notable, only the instructions but not the S–R rules of the experiment changed and we assumed that both, dishonest and opposite responses to follow a two-step process, where a dominant response has to be inhibited to allow for the opposite required response. In line with that assumption, transient effects were similar under both instructions. These results suggest that the (dis)honest and the location task could share adaptation mechanisms. In that case, preceding congruency should modulate the congruency effect when tasks switch, with a larger congruency effect in either task when the preceding other task was congruent relative to when it was incongruent. With a high proportion of congruent trials in the location task, the difference between honest and dishonest responding should be larger than with a low proportion of congruent trials.

Method

Participants

A new sample of 32 participants (age: $M = 28.4$, $SD = 8.21$; 22 female; 31 right-handed) took part in the experiment and received either monetary compensation or course credit. All participants gave written informed consent. The statistical analyses are based on 29 participants because three participants did not provide enough data according to the same criteria as used in Experiment 1 and 2.

Apparatus, Stimuli

For the sake of brevity, we only report details in which Experiment 3 deviated from Experiment 2. Instead of the Stroop task, Experiment 3 featured an S-R compatibility task where participants responded to the location of a square (location task). The edges of the square were 30 pixels long. The square appeared in one of four colors (blue, brown, yellow and purple) to control for feature integration effects. Participants were to respond according to the location and font color of the square with their right index and middle finger by pressing the keys *K* and *L*. Half of the participants were to respond in accordance with the square position (i.e., square left, left key press; square right, right key press) when the color of the square was blue or yellow (congruent). When the square was brown or purple, these participants responded with a left key press to right squares and with a right key press to left squares (incongruent). For the other half of participants, blue and yellow squares implied incongruent responding and brown and purple squares indicated congruent responding.

Procedure

Participants again started with a preexperimental procedure to select an equal amount of questions that asked about activities that had been performed and activities that had not been performed on the same day. Like in Experiment 1, 10 questions with affirmative and 10 questions with negative answers were selected from the question pool.

The square appeared either on the left side or the right side of the display. As in (dis)honest trials, participants had to respond within 3000 ms. Both tasks followed a random sequence. Again, responding honestly and dishonestly to questions was practiced in a first block of 16 trials. Then, participants practiced each combination of square color and position twice in a second block of 16 trials.

Like in Experiment 2, each question appeared equally often with honest and dishonest responses within a block whereas the proportion of conflict in the location task varied between blocks. Each of the 20 blocks featured 80 trials, that is, 40 (dis)honest and 40 location task trials. The sequence of the conflict proportion conditions was counterbalanced across participants. Participants were allowed self-paced breaks between blocks.

Results

Data Treatment

As in Experiment 1 and 2, trials were excluded from further analyses when they featured the same task as the preceding trials with also either same question (1.3%) or same square position and square color (6.5%). We only included correct trials or trials with commission errors which also followed a correct trial for PE analyses. This led to the exclusion of 7.8% of (dis)honest trials and 7.6% of location trials. For RT analyses, we excluded all error trials and those trials that followed them (15.8% of (dis)honest trials, 11.2% of location trials). For analyses on sustained and transient effects, 2.7% and 2.6% of (dis)honest trials and 2.8% and 3.0% of location trials, respectively, were identified as outliers and excluded.

Sustained Effects

PEs and RTs were analyzed in a 2 × 2 × 2 ANOVA with the within-subjects factors task ([dis]honest vs. location), location conflict proportion (low vs. high) and current congruency (congruent vs. incongruent). Sustained adaptation effects should produce a significant interaction between conflict proportion and current congruency. When such adaptation processes fully transferred from the location to the (dis)honest task, there should be no three-way interaction between all three factors. A full ANOVA table, accompanied by BFs for each individual effect can be found in Table S5 in the online supplemental material. We scrutinized significant three-way interactions in separate 2 × 2 ANOVAs and two-way interactions in paired-samples *t*-tests and report BFs for these tests in the text.

Figure 5 shows the mean PEs (upper panels A and B) and RTs (lower panels C and D) for each combination of current congruency and conflict proportion for the (dis)honest task (left panels A and C) and the location task (right panels B and D). On average each cell included 160 observations.

The two-way interaction of current congruency and conflict proportion was significant, $F(1, 28) = 26.33$, $p < .001$, $\eta_p^2 = .49$, as was the three-way interaction of all factors, $F(1, 28) = 30.94$, $p < .001$, $\eta_p^2 = .53$. Furthermore, errors were more frequent in the (dis)honest than in the location task, $F(1, 28) = 15.15$, $p = .001$, $\eta_p^2 = .35$, and in incongruent/dishonest compared to congruent/honest trials, $F(1, 28) = 4.80$, $p = .037$, $\eta_p^2 = .15$. There was also a significant two-way interaction of task and current congruency, $F(1, 28) = 15.24$, $p = .001$, $\eta_p^2 = .35$. None of the remaining effects were significant ($F\text{s} \leq 1.49$, $ps \geq .232$).

Separate analyses for the two tasks clarified the interactions. The main effect of current congruency, $F(1, 28) = 14.15$, $p = .001$, $\eta_p^2 = .34$, BF = 0.02, but not the two-way interaction of current congruency and conflict proportion, $F(1, 28) = 2.67$, $p = .114$, $\eta_p^2 = .09$, BF = 1.55, was significant for the (dis)honest task. For the location task, the main effect of current congruency was not significant ($F < 1$, BF = 3.47), but the interaction of current congruency and conflict proportion was, $F(1, 28) = 37.40$, $p < .001$, $\eta_p^2 = .57$, BF < 0.01, as participants committed more errors in incongruent than in congruent trials in low conflict proportion blocks, $t(28) = 3.50$, $p = .002$, $d = 0.65$, BF = 0.04, but showed the opposite pattern of results in high conflict proportion blocks, $t(28) = 5.55$, $p < .001$, $d = 1.03$, BF < 0.01.

The RT results were in line with the PE results. Again, the two-way interaction between current congruency and conflict proportion, $F(1, 28) = 70.94$, $p < .001$, $\eta_p^2 = .72$, and the three-way interaction, $F(1, 28) = 78.07$, $p < .001$, $\eta_p^2 = .74$, were significant. Furthermore, responses were considerably slower in the (dis)honest task than in the location task, $F(1, 28) = 740.30$, $p < .001$, $\eta_p^2 = .96$, and in incongruent/dishonest trials compared to congruent/honest trials, $F(1, 28) = 30.65$, $p < .001$, $\eta_p^2 = .52$. The two-way interaction between task and current congruency, $F(1, 28) = 22.62$, $p < .001$, $\eta_p^2 = .45$, was also significant. None of the remaining effects were significant ($F\text{s} \leq 1.45$, $ps \geq .239$).

Separate ANOVAs for the two tasks showed that dishonest responses were slower than honest responses, $F(1, 28) = 28.65$, $p < .001$, $\eta_p^2 = .51$, BF < 0.01, but the main effect of conflict proportion ($F < 1$, BF = 5.04), and the interaction of current congruency and conflict proportion were not significant in the (dis)honest task ($F < 1$, BF = 3.75). Responses in the location task did not differ with current congruency ($F < 1$, BF = 4.22), but with proportion, $F(1, 28) = 5.86$, $p = .022$, $\eta_p^2 = .17$, BF = 0.43, as responding took longer in high compared to low conflict pro-

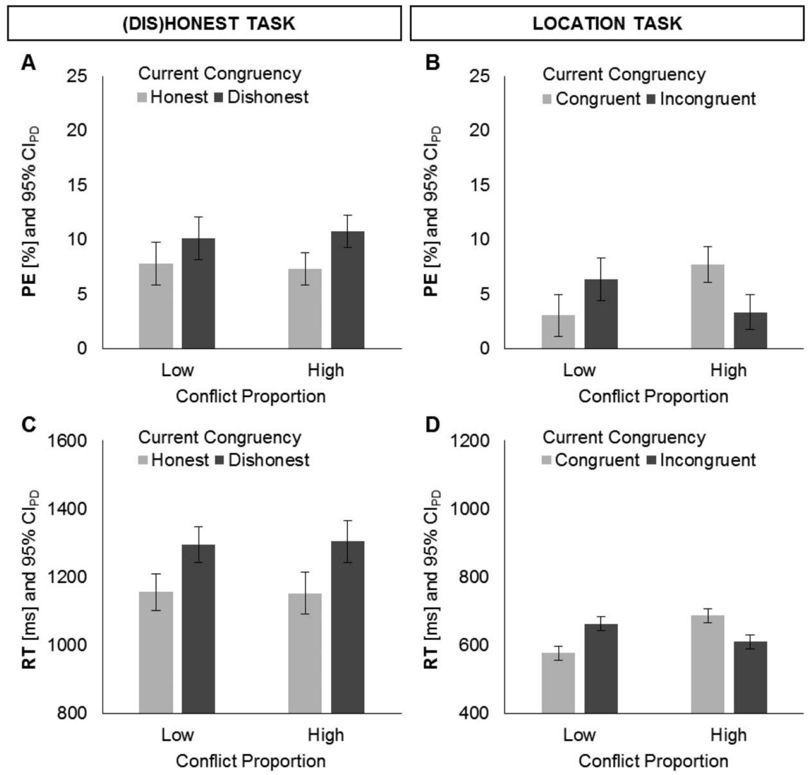

Figure 5. Sustained conflict adaptation effects on PEs (upper panels, A and B) and RTs (lower panels, C and D) in Experiment 3, plotted as function of current congruency and conflict proportion for the (dis)honest task (left panels, A and C) and the location task (right panels, B and D). Note that the RT plots are scaled differently. Dishonest responses were slower and less accurate than honest responses. The proportion manipulation of the location task did not affect performance in the (dis)honest task. Incongruent responses in the location task were slower and less accurate than congruent responses in low conflict proportion contexts but a reversed effect emerged for high conflict proportion blocks. Location responses were also slower in the high conflict proportion condition. Error bars represent the 95% CI_{PD}, computed separately for low and high conflict proportion in each task.

portion blocks. There was also a significant interaction between both factors, $F(1, 28) = 194.41$, $p < .001$, $\eta_p^2 = .87$, BF < 0.01, indicating that incongruent responses were slower than congruent responses with low conflict proportion, $t(28) = 8.58$, $p < .001$, $d = 1.59$, BF < 0.01, while a reversed congruency effect was evident with high conflict proportion, $t(28) = 7.58$, $p < .001$, $d = 1.41$, BF < 0.01.

Transient Effects

A 2 × 2 × 2 × 2 ANOVA with the within-subjects factors task ([dis]honest vs. location), task sequence (repetition vs. switch), current congruency (honest/congruent vs. dishonest/incongruent) and preceding congruency was conducted on PEs and RTs. Transient adaptation effects should produce a significant interaction between current and preceding congruency. When such adaptation processes transferred between tasks, this two-way interaction should not be further qualified by task sequence. A full ANOVA table, accompanied by BFs for each individual effect can be found in Table S6 in the online supplemental material. We scrutinized significant three-way and four-way interactions in separate planned ANOVAs and significant two-way interactions in planned paired-samples *t*-tests and report BFs for these tests in the text.

Figure 6 shows the mean PEs and Figure 7 the mean RTs for each combination of current and preceding congruency for task repetitions (upper panels A and B) and task alternations (lower panels C and D) in the (dis)honest task (left panels A and C) and the location task (right panels B and D). On average each cell included 80 observations.

PE analyses returned a bundle of significant interactions that we will cluster into two convenient groups with the first bundle capturing interactions without the factor preceding congruency and the second bundle comprising all interactions including this factor. First, the interactions between task and task sequence, $F(1, 28) = 5.70$, $p = .024$, $\eta_p^2 = .17$, between task and current congruency, $F(1, 28) = 9.83$, $p = .004$, $\eta_p^2 = .26$, between task sequence and current congruency, $F(1, 28) = 14.68$, $p = .001$, $\eta_p^2 = .34$, and between task, task sequence and current congruency, $F(1, 28) = 5.88$, $p = .022$, $\eta_p^2 = .17$, were significant. Second, the interactions between task sequence and preceding congruency, $F(1, 28) = 4.09$, $p = .053$, $\eta_p^2 = .13$, between current congruency and preceding congruency, $F(1, 28) = 50.97$, $p < .001$, $\eta_p^2 = .65$, and

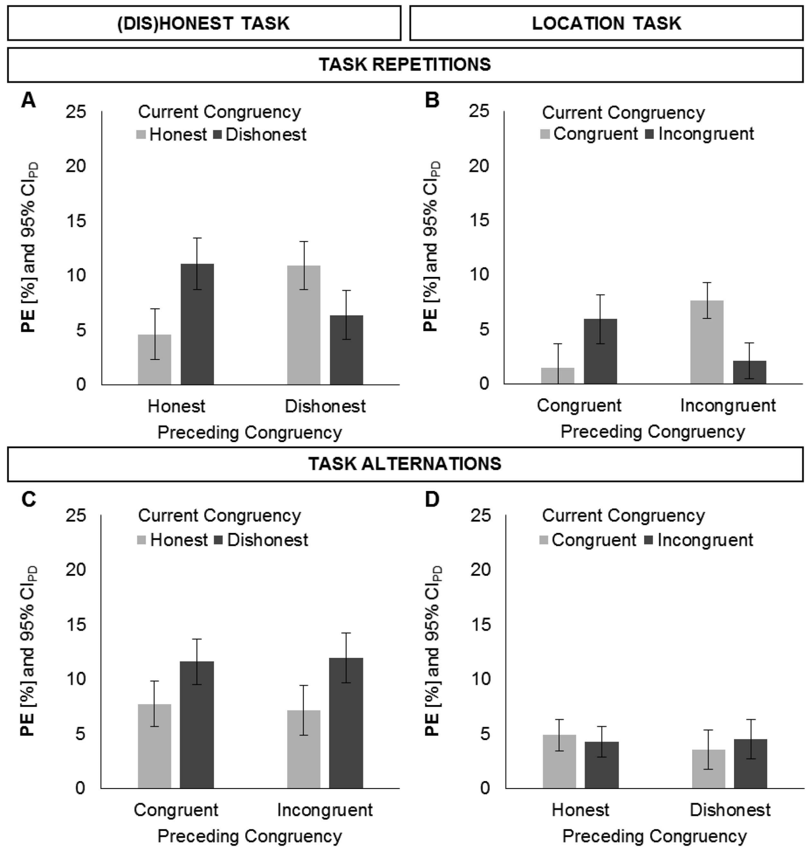

Figure 6. Transient conflict adaptation effects on PEs in Experiment 3, plotted as function of current and preceding congruency, separately for task repetitions (upper panels, A and B) and task alternations (lower panels, C and D) for the (dis)honest task (left panels, A and C) and the location task (right panels, B and D). Participants committed more errors in the (dis)honest task when tasks switched than when tasks repeated. In case of task repetitions, there were transient adaptation effects as congruency effects were reversed after dishonest (A) and incongruent trials (B). Responses were also less accurate after (dis)honest/incongruent trials when tasks repeated. A congruency effect was evident in task alternations (C and D). Error bars represent the 95% CI_{PD}, computed separately for the conditions of preceding congruency and task sequence in each task.

between task sequence, current congruency and preceding congruency, $F(1, 28) = 60.29$, $p < .001$, $\eta_p^2 = .68$, were also significant. In accordance with the analysis of sustained effects, the main effects of task, $F(1, 28) = 23.57$, $p < .001$, $\eta_p^2 = .46$, and current congruency, $F(1, 28) = 8.40$, $p = .007$, $\eta_p^2 = .23$, were significant. Furthermore, there was a nonsignificant trend toward more errors when tasks switched than when tasks repeated, $F(1, 28) = 3.77$, $p = .062$, $\eta_p^2 = .12$. None of the remaining effects were significant (*F*s ≤ 1.41, *p*s $\geq .244$).

For the first bundle of significant interactions, we decided to average data over preceding congruency and computed separate 2 × 2 ANOVAs for the two tasks with the factors task sequence and current congruency. Neither the main effects, nor the interaction were significant in the location task (*F*s ≤ 1.07, *p*s $\geq .309$, BFs ≥ 3.11), whereas all of them were significant in the (dis)honest task. In the (dis)honest task, participants committed more errors when tasks switched than when tasks repeated, $F(1, 28) = 7.56$, $p = .010$, $\eta_p^2 = .21$, BF = 0.23, and when they gave dishonest compared to honest responses, $F(1, 28) = 12.96$, $p = .001$, $\eta_p^2 = .32$, BF = 0.04. These main effects were qualified by their significant two-way interaction, $F(1, 28) = 15.45$, $p = .001$, $\eta_p^2 = .36$, BF = 0.02, as the difference between honest and dishonest responding was only evident when tasks switched, $t(28) = 4.50$, $p < .001$, $d = 0.84$, BF < 0.01, but not when tasks repeated, $t(28) = 1.33$, $p = .194$, $d = 0.25$, BF = 2.29 (note the sequential modulation of the congruency effect in the following analyses though).

For the second bundle of significant interactions, we decided to average data over the two tasks and then computed separate 2 × 2 ANOVAs for task repetitions and switches with the factors current and preceding congruency. Task repetitions did not show a main effect of current congruency, $F < 1$, BF = 4.43, but of previous congruency, $F(1, 28) = 4.44$, $p = .044$, $\eta_p^2 = .14$, BF = 0.75, as responses were more error-prone after incongruent/dishonest than after congruent/honest trials. However, the interaction of current and preceding congruency was also significant, $F(1, 28) = 72.53$, $p < .001$, $\eta_p^2 = .72$, BF < 0.01. A typical congruency effect emerged after congruent/honest responding, $t(28) = 6.21$, $p < .001$, $d = 1.15$, BF < 0.01, but a reversed effect was evident after incongruent/dishonest responding $t(28) = 8.24$, $p < .001$, $d = 1.53$, BF < 0.01. When tasks switched, a congruency effect was

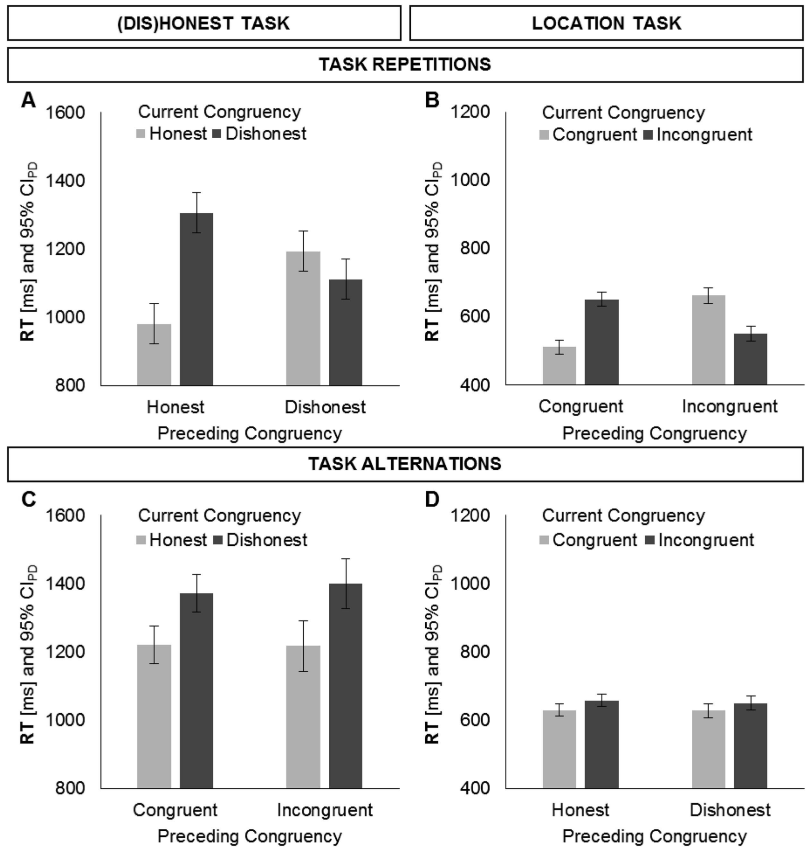

Figure 7. Transient conflict adaptation effects on RTs in Experiment 3, plotted as function of current and preceding congruency, separately for task repetitions (upper panels, A and B) and task alternations (lower panels, C and D) for the (dis)honest task (left panels, A and C) and the location task (right panels, B and D). Note that the RT plots are scaled differently. Current and preceding congruency interacted when tasks repeated as the congruency effect was in the expected direction after honest/congruent trials but reversed after dishonest/incongruent trials (A and B). This modulation was stronger in the location task. Furthermore, increased RTs emerged in repetition trials of the location task after incongruent than after congruent responses. When tasks alternated, preceding congruency did not affect congruency effects of both tasks. Error bars represent the 95% CI_{PD}, computed separately for the conditions of preceding congruency and task sequence in each task.

evident, $F(1, 28) = 16.09$, $p < .001$, $\eta_p^2 = .37$, BF = 0.01, whereas preceding congruency did not affect PEs, $F < 1$, BF = 3.69. There was a nonsignificant trend toward a two-way interaction of both factors, $F(1, 28) = 3.57$, $p = .069$, $\eta_p^2 = .11$, BF = 1.07, pointing toward a smaller congruency effect after congruent/honest than after incongruent/dishonest responding.

In RTs, there were significant interactions between task and task sequence, $F(1, 28) = 42.32$, $p < .001$, $\eta_p^2 = .60$, task sequence and current congruency, $F(1, 28) = 8.62$, $p = .007$, $\eta_p^2 = .24$, current and preceding congruency, $F(1, 28) = 205.72$, $p < .001$, $\eta_p^2 = .88$, task, current and preceding congruency, $F(1, 28) = 11.12$, $p = .002$, $\eta_p^2 = .28$, and task sequence, current and preceding congruency, $F(1, 28) = 258.08$, $p < .001$, $\eta_p^2 = .90$. There was also a nonsignificant trend toward a three-way interaction of task, task sequence, and current congruency, $F(1, 28) = 3.56$, $p = .070$, $\eta_p^2 = .11$. Finally, the analysis yielded a significant four-way interaction of all factors, $F(1, 28) = 27.27$, $p < .001$, $\eta_p^2 = .49$. Mirroring the analysis on sustained effects, the main effect of task, $F(1, 28) = 807.22$, $p < .001$, $\eta_p^2 = .97$, current congruency, $F(1, 28) = 35.18$, $p < .001$, $\eta_p^2 = .56$, and the two-way interaction of both factors were significant, $F(1, 28) = 23.20$, $p < .001$, $\eta_p^2 = .45$. Furthermore, task switches took longer than task repetitions, $F(1, 28) = 82.52$, $p < .001$, $\eta_p^2 = .75$, and there was a nonsignificant trend toward a main effect of preceding congruency, $F(1, 28) = 3.77$, $p = .062$, $\eta_p^2 = .12$. None of the remaining effects were significant (Fs ≤ 1.55, ps $\geq .224$).

For a better understanding of the data, separate 2 × 2 × 2 ANOVAs for the two tasks with the factors task sequence, current and preceding congruency were computed. As both ANOVAs returned significant three-way interactions of all factors ([dis]honest task: $F(1, 28) = 139.63$, $p < .001$, $\eta_p^2 = .83$, BF < 0.01; location task: $F(1, 28) = 275.21$, $p < .001$, $\eta_p^2 = .91$, BF < 0.01), separate 2 × 2 ANOVAs with the factors current and preceding congruency were computed for task repetitions and switches for each task, respectively.

Responding in task repetitions of the (dis)honest task was slower for dishonest than for honest trials, $F(1, 28) = 25.11$, $p < .001$, $\eta_p^2 =$

.47, BF < 0.01. Preceding congruency did not affect RTs, $F < 1$, BF $= 3.3$, but the interaction of both factors was significant, $F(1, 28) = 174.38$, $p < .001$, $\eta_p^2 = .86$, BF < 0.01, because of a typical congruency effect after honest trials, $t(28) = 11.25$, $p < .001$, $d = 2.09$, BF < 0.01, but a reversed effect after dishonest trials, $t(28) = 2.84$, $p = .008$, $d = 0.53$, BF $= 0.19$.

Task switches of the (dis)honest task showed prolonged dishonest responses in comparison to honest responses, $F(1, 28) = 31.09$, $p < .001$, $\eta_p^2 = .53$, BF < 0.01. The main effect of preceding congruency, $F < 1$, BF $= 4.12$, and the two-way interaction were not significant, $F(1, 28) = 2.60$, $p = .118$, $\eta_p^2 = .09$, BF $= 1.60$.

Task repetitions of the location task showed a nonsignificant trend toward longer incongruent than congruent responses, $F(1, 28) = 3.96$, $p = .056$, $\eta_p^2 = .12$, BF $= 0.91$, and responses took longer after incongruent than after congruent responding, $F(1, 28) = 15.32$, $p = .001$, $\eta_p^2 = .35$, BF $= 0.02$. These main effects were further qualified by a significant interaction, $F(1, 28) = 271.90$, $p < .001$, $\eta_p^2 = .91$, BF < 0.01, as incongruent responses were slower than congruent responses after congruent trials, $t(28) = 13.92$, $p < .001$, $d = 2.59$, BF < 0.01, but the opposite was evident after incongruent trials, $t(28) = 10.44$, $p < .001$, $d = 1.94$, BF < 0.01.

Task alternations of the location task showed longer RTs in incongruent than in congruent trials, $F(1, 28) = 7.78$, $p = .009$, $\eta_p^2 = .22$, BF $= 0.21$. The main effect of preceding congruency, $F(1, 28) = 2.39$, $p = .133$, $\eta_p^2 = .08$, BF $= 1.75$, and the two-way interaction of both factors were not significant, $F(1, 28) = 1.61$, $p = .215$, $\eta_p^2 = .05$, BF $= 2.45$.

Discussion

Experiment 3 examined transfer effects between dishonest responding on the one hand, and cognitive conflict that likely is due to similar mechanisms as in dishonesty on the other hand. Both conflicts in this experiment are based on the parallel activation of a dominant response and its appropriate counterpart. Still, the proportion of conflict trials in the location task affected only the location task but not the (dis)honest task. Accordingly, there was no transfer of sustained control adaptation. Transient adaptation effects emerged within both tasks but not between tasks. There was a surprising trend in error rates toward a modulation of the congruency effect by preceding congruency when tasks switched with smaller congruency effects after honest/congruent responses. This modulation was not significant though and did not replicate in RTs. As in Experiment 2, participants responded with different response keys in both tasks which could render transfer of transient control adaptation less likely (Hazeltine et al., 2011; Kleiman et al., 2014) but does not seem to prevent transfer of sustained control adaptation (Experiment 3 of Wühr et al., 2015). Together, Experiment 2 and 3 observed specific control adaptation in lying that is only triggered by (dis)honest responses but not by other conflicts.

General Discussion

Three experiments examined how dishonest responding changes with different conflicting contexts. Experiment 1 provided an integrated analysis of how dishonest responding adapts to transient and sustained dishonest contexts, that is, how dishonest responding changes when it succeeds an honest or a dishonest response and how it changes with the overall proportion of dishonest responses. Experiment 2 and 3 introduced two other types of conflict to establish whether control demands induced by another conflict task can generalize to dishonest responding or vice versa. Overall, the present results suggest that transient and sustained adaptation operate independently within dishonest responding and that transient adaptation effects could be mistaken for sustained adaptation effects (Experiment 1). Furthermore, we replicated strong transient effects in all experiments while neither transient nor sustained adaptation effects transfered across conflicts (Experiment 2 and 3). Together, the experiments paint a consistent picture of a surprisingly focused scope of the context that elicits control adaptation in dishonest responding.

Trading off Sustained for Transient Adaptation

Only transient adaptation to recent (dis)honesty but not sustained adaptation to the proportion of dishonesty emerged in the present analyses. In particular, dishonest responding was more difficult than honest responding after recent honest responses. However, after recent dishonest responses, honest responding was more difficult than dishonest responding even though this reversed effect did not have the same magnitude as the former effect, rendering dishonest responding still more difficult than honest responding overall. As such, the current findings corroborate the robust finding that dishonest responding is more difficult than honest responding (e.g., Suchotzki et al., 2017) but that the processing of preceding responses can shift that difficulty markedly (Debey et al., 2015; Foerster et al., 2016).

Preceding studies did not allow for disentangling whether effects of proportion dishonest manipulations reflected truly sustained adaptation processes or whether they instead reflected transient adaptation processes because information about preceding (dis)honesty were not included in statistical analyses (Van Bockstaele et al., 2012; Verschuere et al., 2011). The present experiment suggests that transient influences drive control adaptation even though, in theory, transient and sustained processes could operate at the same time (e.g., Botvinick et al., 2001).

Of course, these findings should not be taken as evidence against sustained adaptation to dishonesty in general, because such effects emerged when transient adaptation was held constant (Van Bockstaele et al., 2015). In this setting, smaller differences between honest and dishonest responding emerged in error rates but not in RTs when lying was frequent relative to when it was rare. In the present experiment, by contrast, the proportion manipulation did not even modulate the congruency effect in error rates when transient influences were not included in the analysis. The most important difference between both experiments is the fixed versus random arrangement of trials and the resulting constant versus varying nature of transient influences. The predictable trial sequences in Van Bockstaele et al. (2015) accentuated the proportion manipulation as they featured sequences of 10 honest/dishonest trials, followed by 10 trials with honesty and dishonesty in alternation. This accentuation could have rendered sustained control adaptation processes more likely. In contrast, the unpredictable sequence of the present experiment could have worked in favor of transient adaptation as the proportion manipulation might have been less salient. Adaptation to recent events also might have been easier than adaptation to a larger context in this situation. The sustained adaptation effect with predictable trial sequences was of medium size and transient adaptations effects here and elsewhere were of large size (Debey et al., 2015; Foerster et al., 2016). Possibly, a trade-off favors transient control adaptation over sustained control adaptation when both mechanisms could affect dishonest responding, whereas sustained influences are only considered when transient adaptation cannot come into action. A first hint for complex flexible trade-off mechanisms in the current data comes from the explanatory analyses of Experiment 1, which showed stronger transient adaptation in mostly dishonest than mostly honest contexts but only when participants started in the mostly dishonest context.

The present finding of absent sustained adaptation effects in the presence of transient adaptation effects taps into an important issue. With standard conflict tasks, it is difficult to simultaneously scrutinize sustained and transient control adaptation processes within a conflict without introducing confounding variables. For one, proportion manipulations should not introduce frequent S–R pairings which could produce a modulation of the congruency effect by bottom-up learning mechanisms (e.g., Wühr et al., 2015). Thus, inducer and probe stimuli are necessary like in the present study. Second, transitions from congruent/incongruent trials to subsequent congruent/incongruent trials should not be confounded with certain transitions of stimuli and responses between both trials (e.g., Hommel, 2004; Notebaert, Gevers, Verbruggen, & Liefooghe, 2006). In particular, it has to be ensured that all transitions of congruency are similar in regard to stimuli repetitions and switches. Implementing inducer and probe stimuli while at the same time controlling for stimuli repetitions/switches is difficult as those standard conflict tasks typically have a small stimulus set while it is easier with large stimulus sets as in typical (dis)honest tasks. Accordingly, it is more convenient to target common mechanisms of transient and sustained adaptation for standard conflicts via transfer between tasks. If transient and sustained effects dissociate when it comes to transfer, underlying mechanisms should differ as well (e.g., Wühr et al., 2015). Although this transfer situation is elegant, it could miss out on a possible trade-off mechanism between transient and sustained adaptation processes and the moderators of such a trade-off (e.g., Bugg, 2014).

As mentioned earlier, the conflict in dishonest responding differs markedly from standard conflicts which could also set different rules for transient and sustained adaptation processes. While it is theoretically possible to inhibit the irrelevant stimulus dimension completely in standard conflict tasks and still give a correct response, the dominant truthful response seems to be a prerequisite to generate an unrehearsed dishonest response (e.g., Debey et al., 2014). For rehearsed lies, by contrast, dishonest responses seem to be retrieved directly from stimuli (e.g., Hu, Chen, & Fu, 2012; Walczyk et al., 2012; Walczyk, Mahoney, Doverspike, & Griffith-Ross, 2009). For unrehearsed lies, such as in the present experiments, inhibition during dishonest responding might follow a specific time-course and, thus, only be beneficial if initiated early enough, but not too early. Such a time-critical process could be so demanding that agents are not able to additionally consider sustained information about dishonest responding. In a nutshell, it is not obvious from the current point of view whether dishonest responding truly differs from other conflicts in regard to sustained adaptation processes or whether the diverging evidence is based on methodological discrepancies.

A hint that a trade-off could exist for standard conflicts comes from a study where Simon and Stroop conflicts with same relevant stimulus dimension were presented randomly (Experiment 2 of Torres-Quesada et al., 2014). When the proportion of Stroop conflict was manipulated, sustained effects were also found for the Simon effect. However such a transfer of sustained adaptation was only evident when the conflict source had switched from the preceding to the current trial (Stroop → Simon). This is important as transient effects emerged for task repetitions but not for task switches. So when transient conflict adaptation in the Simon task took place after conflict experience in a preceding Simon trial, sustained adaptation was absent, but when there was no conflict adaptation in the Simon task after a Stroop task, sustained adaptation effects were present. In the same study, the authors wanted to assess whether the level of the proportion manipulation (e.g., 80/20 vs. 60/40) affected proportion effects. It did, but specifically for the conflict that featured the proportion manipulation. As such, the interaction of the level of proportion and the effect of proportion on congruency could stem from bottom-up learning mechanisms of probable associations of the irrelevant stimulus dimension and the response. With inducer and probe questions in a dishonest task, such bottom-up mechanisms can be controlled easily. It is plausible that a more extreme manipulation of the proportion shifts the trade-off toward sustained control adaptation.

To scrutinize a possible trade-off between transient and sustained adaptation, researchers could set up an experiment where a dishonest task (or any other conflict with a sufficiently large stimulus set) comes with a different temporal interval in between two successive responses. Previous evidence shows that despite an intermitting task and relatively long time intervals between (dis)honest responses, transient adaptation effects within dishonest responding are still evident (albeit reduced; Experiment 2 and 3 of Foerster et al., 2016). In addition, the proportion of dishonest trials should be manipulated across blocks via inducer questions (possibly by also varying the extent of the proportion manipulation, cf. Experiment 2 of Torres-Quesada et al., 2014). Such experiments could reveal if sustained influences increase with decreasing transient influences and/or with increasing extremity and if both control adaptation processes can also appear in parallel. This is not only relevant for dishonest responding but also for other behavioral conflicts and our general understanding of cognitive control processes. Furthermore, this insight could also prove valuable considering applied efforts to render honest responding as easy as possible and lying as hard as possible to improve lie detection methods (Van Bockstaele et al., 2012, 2015; Verschuere et al., 2011).

Specific Adaptation for Dishonesty

The present study yielded no signs of transfer of transient control adaptation in either direction between the conflicts in dishonest responding and other tasks, nor did sustained adaptation transfer from other conflict tasks to dishonest responding. Whereas conflict sources highly differed in Experiment 2, they were similar in Experiment 3 and previous evidence showed transfer of transient and sustained control adaptation between very different conflict sources and with different relevant stimulus dimensions (e.g., Kleiman et al., 2014; Wühr et al., 2015).

Another conflict that appears to be very similar to dishonest responding in its basic processing is a rule violation. When agents break a rule, the dominant rule-based response has to be inhibited and this rule-based response is necessary to derive the rule violation (Jusyte et al., 2016; Pfister, Wirth, Schwarz, Steinhauser, & Kunde, 2016; Pfister et al., 2016). Rule violation instructions in the present Experiment 3 would result in exactly the same S–R rules and correct actions. However, preceding evidence suggests that the explicit instruction of rule violations would produce larger congruency effects but similar adaptation processes than more neutral rule inversion instructions (Experiment 3 of Wirth, Pfister, Foerster, Huestegge, & Kunde, 2016). The current study makes the counterintuitive suggestion that despite both tasks being similar, control adaptation should not transfer between dishonest responding and rule violations. However, a possibly shared negative connotation of both behavioral tendencies could promote transfer (Wirth, Foerster, Rendel, Kunde, & Pfister, 2017). A close examination of common mechanisms underlying dishonest and violation behavior should be the aim of future research.

The present comparison of different cognitive conflicts and dishonest responding also highlights critical methodological issues. For example, as mentioned earlier, the examination of dishonesty comes with a considerably larger stimulus set than standard conflicts. In the present location task, participants responded to two locations depending on four colors with two responses. In the dishonest task, participants responded to 20 questions depending on two colors with two responses. A large stimulus set is a prerequisite to make sure that participants do not simply learn S–R associations during dishonest responding (e.g., question A in color B affords response C). Of course, simple responding based on S-R associations is an aspect of dishonest responding as specific dishonest responses can be learned when used frequently (e.g., Hu et al., 2012; Walczyk et al., 2009, 2012), however, as people do not lie as frequently as they tell the truth, dominance of the truthful response should be the default scenario (e.g., Debey et al., 2015; DePaulo et al., 1996; Halevy et al., 2014; Hilbig & Hessler, 2013; Serota, Levine, & Boster, 2010). Besides the larger stimulus set, the current question stimuli also differ in complexity from those employed in most common conflict tasks (including the current Stroop and location task). To the best of our knowledge, there are no theoretical assumptions or empirical evidence on how stimulus set size or stimulus complexity could interact with control adaptation or its transfer. The current evidence in the literature does not provide clear rules which differences between stimuli, conflict sources and responses or their constellation are negligible and which have the power to eliminate any transfer of control adaptation. In the light of the large transient adaptation effects that are found within dishonest responding, it is safe to conclude that similar adaptation effects between dishonesty and other tasks do not seem to emerge easily. This does not, however, preclude that there could be specific situations that set the right conditions for such a transfer.

Transient Effects: The Role of Conflict Adaptation and Task Switching

Whereas the current study examined control processes in dishonesty from the perspective of conflict adaptation, recent studies focused on the involvement of task switching processes (Debey et al., 2015; Foerster et al., 2016). Both of these theoretical perspectives converge on the notion that the difficulty of dishonest responding can vary due to transient factors. This raises the question of whether or not the transient changes explored in the present experiments might be fully explained in terms of task-switching mechanisms. This does not seem to be the case though, as suggested by several observations. First and foremost, task switching accounts would predict asymmetric switch costs with larger switch costs for the transition from a difficult to an easier task as for the transition from a relatively easy to a more difficult task (e.g., Allport, Styles, & Hsieh, 1994). Executing a less dominant task requires enhanced activation of the relevant task set, and enhanced inhibition of the irrelevant but more dominant task set, rendering a subsequent switch to the dominiant task especially effortful (e.g., Koch, Prinz, & Allport, 2005; Leboe, Whittlesea, & Milliken, 2005; Schneider, & Anderson, 2010). This view makes direct predictions for the analysis of transient effects for honest and dishonest responses because responding honestly is dominant and easier than dishonest responding (e.g., Debey et al., 2014). However, five out of six recent experiments (Debey et al., 2015; Foerster et al., 2016) and all of the current experiments showed symmetrical switch costs.[3] Symmetrical switch costs have previously been attributed to the inherent activation of the honest response in dishonest responding (Debey et al., 2015), thus emphasizing the role of conflict for control adaptation in dishonest responding. As such, the transient effects explored in the present experiments cannot be fully accounted for by a task switching perspective whereas they are well in line with conflict adaptation theories. To further corroborate this assessment, we reanalyzed the data of a recently published experiment from our lab (Exp. 4 of Foerster et al., 2016). In this experiment, truth distractors (i.e., distractors that are compatible with an honest response) and lie distractors (i.e., distractors that are incompatible with an honest response) accompanied each question. Truth distractors facilitated honest and dishonest responses in comparison to lie distractors, revealing the initial honest response activation when responding dishonestly (cf., Debey et al., 2014). Control adaptation should diminish the impact of subsequent conflicting responses. In particular, distractor effects should be reduced after dishonest compared to after honest responses and such a finding could be explained by conflict adaptation but not by task switching. Distractor effects were indeed smaller after dishonest than after honest responses in the error rates of currently dishonest trials, $F(1, 42) = 6.27$, $p = .016$, $\eta_p^2 = .13$ (see Experiment 4 of Foerster et al., 2017). This finding provides additional support to a control adaptation perspective on transient effects in dishonesty as put forward by the current study.

[3] A statistical comparison of switch costs for honest ([dishonest → honest] – [honest → honest]) and dishonest ([honest → dishonest] – [dishonest → dishonest]) responses for each of the current experiments revealed no significant difference in error rates (ps $\geq .342$) or RTs (ps $\geq .114$).

Connecting Control Adaptation to Other Processes of Dishonesty

The current design deliberately limited the experimental design to the cognitive processes involved in the two-step process of an initial activation and inhibition of the truth, and how these processes can be regulated by transient and sustained control adaptation. Based on the current results, future studies could examine how other components of lying affect the control of these cognitive processes. Motivational tendencies suggest themselves as moderators when considering previous evidence of the conflict and lying literature. For example, reward modulated control adaptation depending on the kind and rules of reward (e.g., Braem, Verguts, Roggeman, & Notebaert, 2012; Van Steenbergen, Band, & Hommel, 2009; but see Stürmer, Nigbur, Schacht, & Sommer, 2011). Similarly, gain and loss affected action control (Wirth, Dignath, Pfister, Kunde, & Eder, 2016) and conflicts were more likely and more easily avoided than approached (e.g., Dignath, & Eder, 2015; Dignath, Kiesel, & Eder, 2015). In cheating paradigms, lying was more frequent when it averted loss than when it led to gain (Schindler, & Pfattheicher, 2017). In this vein, it would be interesting to establish in future research whether loss compared to gain triggers more control over the activation of the truth and its inhibition, rendering lying not only more frequent but also less challenging.

Conclusion

The present experiments examined whether transient and sustained control adaptation elicited by dishonest and standard cognitive conflicts can affect the two-step process of initial honest response activation and its inhibition in dishonest processing. Adaptation processes did not transfer between dishonest responding and other conflicts in any of the experiments. Transient control adaptation to recent experiences of dishonesty, by contrast, improved dishonest responding substantially in all experiments while sustained control adaptation to frequent dishonest responding was absent. On the basis of previous evidence, we therefore propose that sustained adaptation to dishonesty only comes into play when transient adaptation is not possible in a given context; in all remaining contexts, sustained adaptation is traded for transient adaptation instead. Because transient adaptation is likely to be possible in a huge variety of settings, and will therefore override more sustained effects, the present experiments document flexible but surprisingly focused control adaptation in dishonest responding.

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(*Appendix follows*)

Appendix

Question Set With German Originals and English Translations

1. Warst du Joggen?
Did you go for a run?
2. Bist du eine Treppe herunter gegangen?
Did you go down a staircase?
3. Bist du eine Treppe hoch gegangen?
Did you go up a staircase?
4. Hast du getankt?
Did you buy petrol?
5. Hast du Schokolade gegessen?
Did you eat chocolate?
6. Bist du Bus gefahren?
Did you take a bus?
7. Bist du Zug gefahren?
Did you take a train?
8. Hast du einen Mülleimer benutzt?
Did you use a dustbin?
9. Hast du ein Bad genommen?
Did you take a bath?
10. Hast du ein Toast zubereitet?
Did you make a sandwich?
11. Hast du einen Brief geschrieben?
Did you post a letter?
12. Hast du eine Tür geschlossen?
Did you close a door?
13. Warst du duschen?
Did you take a shower?
14. Hast du eine Zeitung gekauft?
Did you buy a newspaper?
15. Hast du eine Zeitschrift gekauft?
Did you buy a magazine?
16. Hast du ein Messer benutzt?
Did you use a knife?
17. Hast du einen Regenschirm benutzt?
Did you use an umbrella?
18. Hast du ein Medikament genommen?
Did you take a pill?
19. Hast du mit einem Polizisten gesprochen?
Did you speak to a police officer?
20. Hast du einen Apfel gegessen?
Did you eat an apple?
21. Hast du ein Fenster zerstört?
Did you break a window?
22. Hast du telefoniert?
Did you use a telephone?
23. Hast du eine SMS erhalten?
Did you receive a text?
24. Hast du einen Saft getrunken?
Did you drink fruit juice?
25. Hast du Radio gehört?
Did you listen to the radio?
26. Warst du im Internet?
Did you use the internet?
27. Hast du in einer Schlange angestanden?
Did you stand in a queue?
37. Hast du an einer Ampel gehalten?
Did you stop at a traffic light?
38. Warst du im Supermarkt?
Did you go to a supermarket?
39. Hast du Blumen gekauft?
Did you buy some flowers?
40. Hast du abgewaschen?
Did you do the dishes?
41. Bist du Fahrstuhl gefahren?
Did you take an elevator?
42. Hast du ein Fenster geputzt?
Did you clean a window?
43. Hast du eine Verabredung verschoben?
Did you reschedule an appointment?
44. Hast du ein Buch gelesen?
Did you read a book?
45. Hast du ein Moped abgestellt?
Did you park a moped?
46. Hast du eine Zitrone ausgepresst?
Did you squeeze a lemon?
47. Hast du eine Email verschickt?
Did you send an e-mail?
48. Ha*st du ein Tier gestreichelt?*
Did you stroke a pet?
49. Hast du einen Mantel getragen?
Did you wear a coat?
50. Hast du einen Kühlschrank geöffnet?
Did you open a fridge?
51. Hast du einen Computer eingeschaltet?
Did you switch on a computer?
52. Hast du eine Zigarette geraucht?
Did you smoke a cigarette?
53. Hast du auf eine Uhr geschaut?
Did you look at a watch?
54. Hast du einen Wasserhahn geöffnet?
Did you open a water tap?
55. Hast du einen Toilettendeckel geöffnet?
Did you lift a toilet seat?
56. Bist du über einen Zebrastreifen gelaufen?
Did you use a pedestrian crossing?
57. Hast du einen Geldautomaten benutzt?
Did you use an ATM?
58. Hast du Geld gewechselt?
Did you change money?
59. Hast du einen Teppich abgesaugt?
Did you vacuum a carpet?
60. Hast du Hustensaft getrunken?
Did you drink cough syrup?
61. Hast du jemanden gegrüßt?
Did you greet someone?
62. Hast du geputzt?
Did you clean the house?
63. Hast du in deinen Briefkasten geschaut?
Did you check your mailbox?

(*Appendix continues*)

Appendix (*continued*)

28. Hast du in einem Warteraum gesessen?
Did you sit in a waiting room?

29. Hast du dein Bett gemacht?
Did you make your bed?

30. Hast du deine Hände gewaschen?
Did you wash your hands?

31. Hast du ein Dokument unterzeichnet?
Did you sign a document?

32. Hast du Kaffee getrunken?
Did you drink coffee?

33. Hast du mit einem Kind gesprochen?
Did you speak to a child?

34. Hast du Fernsehen geschaut?
Did you watch television?

35. Hast du Zwiebeln gegessen?
Did you eat onions?

36. Hast du Wasser getrunken?
Did you drink water?

64. Hast du deine Zähne geputzt?
Did you brush your teeth?

65. Hast du Musik gehört?
Did you listen to music?

66. Bist du Fahrrad gefahren?
Did you ride on a bicycle?

67. Hast du auf einer Leiter gestanden?
Did you stand on a ladder?

68. Hast du auf einem Stuhl gesessen?
Did you sit on a chair?

69. Hast du ein Stück Papier abgerissen?
Did you rip a piece of paper?

70. Hast du Blumen gegossen?
Did you water the plants?

71. Hast du deine Schlüssel benutzt?
Did you use your keys?

72. Hast du Wasser gekocht?
Did you boil some water?

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