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The dishonest mind set in sequence

Anna Foerster[1] · Robert Wirth[1] · Wilfried Kunde[1] · Roland Pfister[1]

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Abstract Dishonest responding is an important part of the behavioral repertoire and perfectly integrated in communication and daily actions. Thus, previous research aimed at uncovering the cognitive mechanisms underlying dishonest responding by studying its immediate behavioral effects. A comprehensive account of the aftereffects of this type of behavior has not been presented to date, however. Based on the methods and theories from research on task switching, we, therefore, explored the notion of honest and dishonest responding as two distinct intentional sets. In four experiments, participants responded either honestly or dishonestly to simple yes/no questions. Crucially, robust switch costs were found between honest and dishonest responding when questions succeeded promptly (Exp. 1) but also when an unrelated task intervened between questions (Exp. 2). Surprisingly, responding dishonestly to a question also affected responses in the subsequent intervening task in terms of a more liberal response criterion. Time to prepare for the upcoming intentional set further induced asymmetrical switch costs (Exp. 3). Finally, a novel control condition (Exp. 4) allowed us to pinpoint most of the observed effects to negation processing as an inherent mechanism of dishonesty. The experiments shed new light on the cognitive mechanisms underlying dishonesty by providing strong support for the concept of distinct mental sets for honest and dishonest responding. The experiments further reveal that these mental sets are notably stable and are not disturbed by intervening task performance. The observed aftereffects of dishonest responding might also provide a potent extension to applied protocols for lie detection.

✉ Anna Foerster
anna.foerster@uni-wuerzburg.de

[1] Department of Psychology III, University of Würzburg, Röntgenring 11, 97070 Würzburg, Germany

Introduction

Imagine two co-workers chatting about their activities during the weekend. While one of them boasts about an exciting event, verbosely exaggerating the occasional detail, the other had rarely left the house. As she is not inclined to admit that she enjoyed staying in, she tells her colleague that she mostly stayed at home because she felt sick. The chat ends and both return to work. In this conversation, both of them had made honest and dishonest statements, and they had switched between both types of statements. This example illustrates that dishonest behavior is not an encapsulated action but rather it is always embedded in conversations and other unrelated actions, making it necessary to switch between honest and dishonest responding, and to perform other actions in the meantime. In the present experiments, we aimed at investigating exactly this switch between honest and dishonest responding for one, and how such switches are affected by unrelated actions for another.

Research on the cognitive mechanisms underlying dishonest behavior has indeed concentrated on the immediate effects of dishonesty on different neurophysiological and behavioral measures (e.g., Debey, Verschuere, & Crombez, 2012; Duran, Dale, & McNamara, 2010; Pfister, Foerster, & Kunde, 2014; Spence et al., 2001; Vrij, Fisher, Mann, & Leal, 2008a ; Walczyk, Roper, Seemann, & Humphrey, 2003), whereas only little is known about the aftereffects of dishonest behavior (Debey, Liefooghe, Houwer, & Verschuere, 2014a; Johnson, Barnhardt, & Zhu, 2003). A methodological tool to approach such aftereffects are sequential analyses that specifically target the interplay of current and preceding behavior. In the following, we first give an overview of current theories about the cognitive underpinnings of dishonest behavior, followed by methodological considerations for studying the aftereffects of such behavior. We then present four experiments that employ these methods to scrutinize the aftereffects of dishonest responding.

The dishonest mind set

Lies and dishonest responses are an integral part of everyday communication, as indicated by diary and survey studies on the frequency of such events (DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996; Serota, 2014; Serota, Levine, & Boster, 2010). In these studies, a considerable proportion of participants reported dishonest behavior on a daily basis. Participants also claimed to be quite successful liars. And indeed, even robbery investigators, judges, psychiatrists, and polygraphers struggle to detect dishonest responses with above-chance precision (Ekman & O'Sullivan, 1991).

These findings suggest that most humans are able to produce lies efficiently and without second thought. Still, previous research has uncovered a range of subtle effects of dishonest responding in behavioral, hemodynamic, and electrophysiological measures. To investigate these effects, participants are typically instructed to respond honestly or dishonestly to a stimulus, e.g., to simple, autobiographical yes/no questions. In investigations like these, dishonest responses were consistently slower than honest responses (e.g., Debey et al., 2012; Walczyk et al., 2003). Furthermore, areas associated with cognitive control showed enhanced hemodynamic activity during dishonest as compared to honest responding (e.g., Bhatt et al., 2009; Spence et al., 2001), and electrophysiological investigations indicated a less direct retrieval of the appropriate response when intending to respond dishonestly (e.g., Johnson, Barnhardt, & Zhu, 2003, 2004; Pfister et al., 2014; Suchotzki, Crombez, Smulders, Meijer, & Verschuere, 2015).

Such differences between honest and dishonest responding are typically explained in terms of increased cognitive demand when people are dishonest (Zuckerman, DePaulo, & Rosenthal, 1981). In other words: Even if dishonesty may not feel particularly effortful for the acting agent in most situations, dishonest behavior still comes with some difficulty. The cause of this difficulty has been explained in different theoretical frameworks for understanding the cognitive mechanisms underlying honesty and dishonesty. According to an influential suggestion (Vrij et al., 2008a, 2008b), dishonesty entails the following processes: (1) generation of the dishonest response, (2) enhanced monitoring and control of own behavior to appear honest, (3) enhanced monitoring of the other's reaction to evaluate the success of dishonesty, (4) deliberate acting, (5) suppression of the honest response and (6) deliberate and intentional activation of the dishonest response compared to mostly automatic activation of the truth (see also, e.g., Debey, De Houwer, & Verschuere, 2014b; Duran et al., 2010; Foerster, Pfister, Schmidts, Dignath, & Kunde, 2013; Levine, 2014; Shalvi, Eldar, & Bereby-Meyer, 2012; Verschuere, & Shalvi, 2014).

Automatic activation of the honest response is also an integral part of a second prominent approach, the Activation-Decision-Construction-Action Theory (ADCAT; Walczyk, Harris, Duck, & Mulay, 2014; Walczyk et al., 2003). According to this theory, a decision to lie leads to inhibition of the honest response and a lie is constructed. The ADCAT further highlights an expectancy-value mechanism behind the decision component, and assumes the honest response to remain represented throughout the process of actually giving the dishonest response, which necessitates continued inhibition until the action component is completed.

Even though the two models differ in several aspects, they converge on the notion that dishonest responding is a qualitatively different task than responding honestly. Hence, honest and dishonest behavior are characterized as two distinct intentional sets. Both models further describe dishonest responding as drawing on increased cognitive control and inhibition of automatic action tendencies, whereas they describe honest responding as giving into these automatic tendencies. For the present argument, we will focus on this common assumption of an automatic activation of the honest response, followed by effortful inhibition.

Notably, both models tacitly assume that the processes of activation and inhibition can be characterized sufficiently when focusing on single instances of honest or dishonest behavior. That is, even though other processes are explicitly described to be affected by contextual variables, for instance current goals and expectations, such contextual variables are not sufficiently discussed with relation to the automatic activation of the honest response and its inhibition. The only exception to this rule seem to be rehearsed lies as they are explicitly examined and discussed in the context of ADCAT (Walczyk et al., 2005, 2012; Walczyk, Mahoney, Doverspike, & Griffith-Ross, 2009). In this case, however, rehearsal seems to lead to the formation of S-R association that are easily retrieved, rendering the automatic activation and inhibition of the honest response obsolete for dishonest responding. The otherwise unconditional treatment of these processes seems warranted in light of the robustness and ubiquity of the effects of dishonest as compared to honest responding. On the other hand, viewing honest and dishonest responding as distinct intentional sets does indeed render contextual factors such as aftereffects of previous behavior likely (see the next section for a thorough discussion). Whether or not theoretical frameworks for understanding the cognitive mechanisms of dishonesty would benefit from including such contextual factors is the central question of the experiments reported here.

Preliminary support for this consideration comes from studies on moderators of the effects of dishonest responding on behavioral and physiological measures. Such moderators include cognitive load, exercise and the proportion of dishonest trials (e.g., van Bockstaele et al., 2012; van't Veer, Stel, & van Beest, 2013; Verschuere, Spruyt, Meijer, Otgaar, 2011; Walczyk et al., 2012). Because these moderators were rarely discussed directly with regard to the theoretical frameworks described above, we decided to analyze a ubiquitous contextual factor which is the modulation of honest and dishonest behavior by immediately preceding intentional sets.

To isolate the activation of the honest response and its subsequent inhibition from other processes such as deciding to tell a lie, constructing a plausible lie that is appropriate for the audience, and monitoring another's reactions (Vrij et al., 2008a; Walczyk et al., 2014), we deliberately restricted our analysis to simple yes/no questions. For these questions, the dishonest mind set comes down to a limited range of processes (see Fig. 1): Participants read the question which triggers rather automatically the honest response. Being honest simply means to answer with the true value whereas being dishonest implies to answer with the exact opposite of that true value. To further remove a possible decision component, we cued whether or not a question had to be answered honestly or dishonestly. This experimental approach allows studying the processes of activation and inhibition as an acid test of whether or not they are subject to contextual modulation by preceding instances of honest or dishonest responding.

Studying the aftereffects of behavior

Aftereffects of any type of behavior can be addressed in terms of sequential analyses that have been developed in the extensive literature on task switching (e.g., Allport, Styles, & Hsieh, 1994; Rogers & Monsell, 1995; for reviews, see Kiesel et al., 2010; Monsell, 2003; Vandierendonck, Liefooghe, & Verbruggen, 2010). Holding two tasks in working memory not only yields general costs as evidenced by overall decreased performance as compared to single-task settings (mixing costs), but also local costs that arise when switching from one task to another (switch costs).

Overall mixing costs emerge also for honest and dishonest tasks (Johnson et al., 2003, 2004, 2005). In these experiments, participants had to memorize a list of words in a training session and they had to indicate if they saw a new or an old word in the following experimental session. They had to categorize the words honestly throughout one block and dishonestly throughout another block. In a last block, participants had to decide in each trial whether they wanted to respond honestly or dishonestly but they had to balance the amount of honest and dishonest trials across the block. Participants gave fastest responses in the consistent honest condition, followed by responses in the consistent dishonest condition. Both, honest and dishonest responses were slower in the random condition than in the single-task condition.

More important for the present experiments, however, are switch costs, i.e., longer RTs and higher error rates on task switch trials as compared to task repetition trials. Different theoretical assumptions exist about the origin of these switch costs, e.g., switch costs were proposed to derive from switch-specific reconfiguration processes (e.g., Rogers & Monsell, 1995) or from interference between the preceding activated task set and the currently relevant task

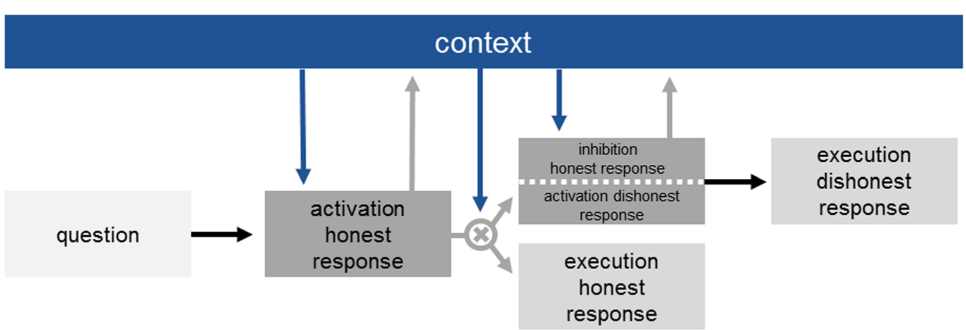

Fig. 1 Possible interactions of contextual variables with the processes that guide honest and dishonest behavior when responding to simple yes/no questions. Following the question, we assume that the honest response is activated automatically as soon as the question is read and understood. Whether the honest response can be executed depends on the current intention. When participants are to respond dishonestly, the honest response has to be inhibited to give way for the dishonest response. Contextual information, for the present experiments, is the preceding intentional set that might affect these processing steps. The results of the present series of experiments do indeed favor this possibility

set (e.g., Allport et al., 1994). Switch costs, i.e., local costs that occur when tasks switch from the preceding to the current trial, were observed when participants follow a fixed task sequence (e.g., Rogers, & Monsell, 1995), when they voluntarily choose the task in each trial (e.g., Arrington & Logan, 2004, 2005) and also when a cue signals the task in each trial (e.g., Meiran, 1996). If the production of a lie constitutes a distinct intentional set, behavioral costs should arise for a voluntary switch to dishonest or honest responding (as in daily communication) but also for cued switches (as in the current study).

In addition to switch costs between those two intentional sets, analyses of trial sequences can further inform about aftereffects on unrelated behavior. A useful method to investigate such aftereffects was proposed for studies on cognitive conflicts (Verguts, Notebaert, Kunde, & Wühr 2011). The authors of this study examined if cognitive conflict causes more cautious behavior and thus a general increase of response times (RTs) in subsequent behavior. To distinguish this slow-down from the effect of task focusing, i.e., a focus on relevant stimulus dimensions, the authors first induced conflict in terms of a Simon task (responding to stimulus color while ignoring stimulus location) and assessed the aftereffects of this task on a univalent probe task (location discrimination). With the help of this experimental paradigm, post-conflict slowing in the probe task was observed when the probe task followed an incongruent rather than a congruent Simon trial, indicating aftereffects of conflict processing on an unrelated task. Similar to cognitive conflict in the Simon task, dishonest responding requires the inhibition of an automatic response tendency and the generation and execution of an alternative response. Hence aftereffects of dishonesty might resemble aftereffects of cognitive conflict.

The present experiments

In four experiments, we scrutinized the aftereffects of dishonest responding by borrowing methodological tools from research on task-switching and conflict processing. In Experiment 1, we aimed at validating our paradigm by replicating the established RT difference when answering honestly or dishonestly to simple yes/no questions. Participants answered simple yes/no questions about activities they may have experienced on that day. A cue signaled participants whether to answer honestly or dishonestly in each trial. Regarding the immediate effects of dishonesty in the literature (e.g., Pfister et al., 2014; Spence et al., 2001; Walczyk et al., 2003), we expected dishonest responses to take longer and to be more error-prone than honest responses. Crucially, sequence analyses should reveal switch costs, i.e., higher RTs and error rates when participants respond honestly in one trial and dishonestly in the next trial or vice versa compared to repeating intentions. Recently, a very similar study was published by Debey et al. (2014a). They found reliable symmetrical switch costs in RTs and error rates and proposed that the results support the assumption that when people are dishonest, the honest response is activated in a first step. Furthermore, we explored the notion that the modulation of the intention effect through the proportion of dishonest trials might be driven by an unequal ratio of switches in honest and dishonest trials (e.g., van Bockstaele et al., 2012; Verschuere et al., 2011). Following this logic, a high proportion of dishonest trials features mainly dishonest repetition trials and honest switch trials (i.e., switches from dishonest to honest responding). Accordingly, dishonest responding is facilitated as it mostly entails a repetition of the same intention whereas honest responding is more difficult as it mostly entails a switch of intentions. This should result in a relatively small intention effect. In contrast, a low proportion of dishonest trials features mainly dishonest switch trials (i.e., switches from honest to dishonest responding) and honest repetition trials. In this case, dishonest responding is more difficult as it mostly entails a switch of intentions whereas honest responding is facilitated as it mostly entails a repetition of the same intention. This should result in a relatively large intention effect. We scrutinized this assumption in a design with a balanced ratio of honest and dishonest trials by separately analyzing the differences between those trial combinations. In a nutshell, Experiment 1 provided information about the immediate intention effect, i.e., the difference between honest and dishonest responses, and the sequence effect of intentions, i.e., the difference between intention repetitions and switches from one trial to the next.

Experiment 2 then targeted aftereffects of honest and dishonest responding on unrelated behavior. To this end, we employed a design similar to those used to address post-conflict slowing and added an unrelated intervening task in between the sequence of honest and dishonest responses (Verguts et al., 2011). We, therefore, expected slower responses in this intervening task following dishonest as compared to honest responses, indicating that participants might indeed adopt a more cautious response criterion after having responded dishonestly. The introduction of the intervening task was also expected to affect sequence effects of honest and dishonest responding. This task not only made participants switch to an unrelated task in between two questions, but it also prolonged the time that passed between response to the question in the current trial and question onset in the next trial substantially. Increased time intervals between a response and subsequent task cue empirically lead to reduced switch costs (e.g., Meiran, Chorev, & Sapir, 2000). A second question of Experiment 2 thus was whether the switch costs observed in Experiment 1 would still be evident when two question responses were separated by the intervening task.

Experiments 3 finally investigated the impact of task preparation for honest and dishonest responding. Whereas in Experiment 1 and 2, participants did not know beforehand whether they had to respond honestly or dishonestly until the question was presented on screen, we now presented the intention cue shortly before the question. In traditional task switching investigations, overall performance typically improves and switch costs decrease with increasing preparation time (e.g., Meiran, 1996; Koch, 2003; Sudevan, & Taylor, 1987). Accordingly, we expected preparation to improve overall performance in the honest and dishonest task, and to reduce sequence effects. Furthermore, knowledge of the intention of the upcoming question could reduce behavioral costs of dishonest compared to honest responding as the appropriate intentional set may be prepared beforehand.

To assess how specific the observed effects are for the setting of honest and dishonest responding, Experiment 4 introduced a novel control condition: Whereas participants in Experiment 3 received an explicit instruction to respond either honestly or dishonestly (as in Exp. 1–2), participants in Experiment 4 were asked to answer the questions either from the perspective of another agent who had experienced the same events as the participants or from the perspective of another agent who had experienced the exact opposite events.

Experiment 1

The experiment was set up to get a grasp on sequence effects of honest and dishonest responding. Participants answered simple yes/no questions about activities to indicate whether or not they had already experienced this activity on the same day. The font color of the response labels indicated if they were to answer honestly or dishonestly in the current trial. We expected an intention effect in terms of higher RTs and error rates for dishonest responses compared to honest responses. More importantly, we further expected a switch from honest to dishonest responding (or vice versa) from one trial to the next to impair performance in terms of increased RTs and more errors compared to repetition sequences.

Methods

Participants, stimuli and apparatus

The sample size was based on a power analysis with the following input parameters: $d = 0.5$, $\alpha = .05$, and a power of .8. A sample size of 32 met these criteria; to compensate for potential exclusions, however, we recruited thirty-four participants for Experiment 1 (6 male, all right-handed, mean age = 21.0 years) who received either monetary compensation or course credit. All participants gave informed consent. Participants sat in front of a 17″ CRT display. Questions about daily activities (see the "Appendix") were adapted from previous work (van Bockstaele et al., 2012), translated to German and modified slightly. For instance, one question asked participants if they had ridden a bicycle. The response keys *D* and *K* of a standard QWERTZ keyboard were assigned to *yes* and *no*, counterbalanced across participants. Participants used their left and right index finger to press the keys.

Procedure

Prior to the actual experiment, participants answered a random selection from the question pool to generate a balanced stimulus set of activities they either had or had not performed. For this pretest, participants indicated whether they had already experienced the questioned activities during the same day. The experimenter advised participants to answer carefully and at leisure and emphasized that they had to let her know if they made a mistake. Participants answered the questions until they had given ten affirmative and ten negative answers, respectively. In the actual experiment, these 20 questions were used again. If more than ten affirmative (or negative) answers had been given before the tenth negative (or affirmative) answer, the program discarded the surplus questions.

The question was presented in the horizontal center of the screen, 35 % below the top of the screen in white font against a black background (Fig. 2). The labels *yes* and *no* appeared below the question, 25 % from the left and right side of the screen, respectively. Participants were to answer the questions with one of two different intentions, i.e., honestly or dishonestly. Therefore, the font color (yellow and blue) of *yes* and *no* indicated the appropriate intention of the current trial. Each question had to be answered honestly and dishonestly equally often in each block. The assignment of the colors to intentions was counterbalanced across participants. Participants had to respond as quickly as possible (response deadline: 3000 ms). The next trial started after an inter-trial interval (ITI) of 400 ms. Error feedback was provided for 500 ms if participants did not follow the instructed intention, did not respond within 3000 ms or pressed any other key than *D* or *K*.

Each block featured 60 trials, with one half of the questions being presented two times and the other half being presented four times.[1] Participants worked through 10 blocks in total.

[1] The differing frequencies of the questions resulted from a bug in the program. Yet, question frequency did not alter the observed pattern, as indicated by follow-up ANOVAs with the within-subjects factors intention (honest vs. dishonest), intention sequence (repetition vs. switch) and question frequency (rare vs. frequent) on RTs and error rates. Neither the main effect of question frequency nor any interaction of question frequency with the other factors approached significance in these analyses, $ps \geq .108$. Equally, Exp. 2 fixed the presentation frequencies and replicated the results of Exp. 1.

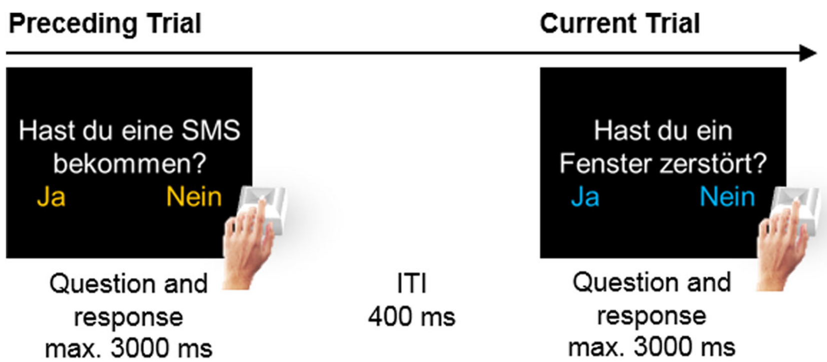

Fig. 2 Trial procedure and sequence of Exp. 1. Each trial featured one of the twenty questions (here: "Did you receive a text?" and "Did you break a window?"). Participants answered with *yes* or *no* either honestly or dishonestly, according to the font color of the response labels. The analyses focused on the impact of intention (honest vs. dishonest) on response times (RTs) and *error rates*, and its sequential modulation by the immediately preceding answer (intention repetition vs. switch)

Data treatment and analyses

We ran separate 2 $\times$ 2 analysis of variance (ANOVA) with the within-subjects factors intention (honest vs. dishonest) and intention sequence (repetition vs. switch) on RTs and error rates. A repetition sequence means that intentions were the same in the preceding and current trial (e.g., honest responses in both trials). A switch sequence means that intentions switched between the preceding and current trial (e.g., honest response in the preceding trial and dishonest response in the current trial). Additionally, honest repetition trials were directly pitted against dishonest switch trials (i.e., switches from honest to dishonest responding), and honest switch trials (i.e., switches from dishonest to honest responding) were pitted against dishonest repetition trials in separate two-tailed paired-samples *t*-tests to explore the notion that differing switch ratios in honest and dishonest responding might have driven the effects of proportion dishonesty manipulations (van Bockstaele et al., 2012; Verschuere et al., 2011). Follow-up analyses, i.e., two-tailed paired-samples *t*-tests, were used in case of significant interactions.

The first block served as practice and was thus excluded from all analyses. We further excluded the first trial of each block as there was no preceding trial that could be considered for the sequence analysis.

Results

One participant was excluded based on the number of valid trials remaining for the RT analysis. The participant deviated more than 2.5 standard deviations (SDs) from the other participants in at least on cell mean. Consequently, results are based on a sample of 33 participants.

RTs

All error trials (14.6 %) and trials following errors were excluded from analysis. The same was done with trials that used the same question as the trial before to avoid confounds due to retrieval of item-specific S-R associations (4.1 %). For the remaining trials, *z*-scores of the RTs were separately computed for each design cell. Trials with RTs that deviated more than 2.5 SDs from the respective cell mean were eliminated as outliers (2.2 %).

Mean RTs for all conditions of interest are plotted in Fig. 3a. As expected, dishonest responses took longer than honest responses, giving rise to a substantial intention effect ($\Delta = 146$ ms), $F(1, 32) = 90.59$, $p < .001$, $\eta_p^2 = .74$. Likewise, responses were slower when intentions switched from one trial to the next as compared to intention repetitions ($\Delta = 214$ ms), $F(1, 32) = 271.81$, $p < .001$, $\eta_p^2 = .90$. The interaction of the two factors was not significant, $F < 1$. The planned comparisons confirmed dishonest switch trials to be slower than honest repetition trials, $t(32) = 17.0$, $p < .001$, $d = 2.96$ ($\Delta = 360$ ms), whereas dishonest repetition trials were faster than honest switch trials ($\Delta = 68$ ms), $t(32) = 3.56$, $p = .001$, $d = 0.62$.

Error rates

Omissions (1.8 %) and trials where any other key than *D* or *K* was pressed (irrelevant commissions; 0.1 %) were excluded. Then, we computed the relative commission error rate.

Mean error rates for all conditions of interest are plotted in Fig. 4a. Participants committed more errors when they had to respond dishonestly than when they had to respond

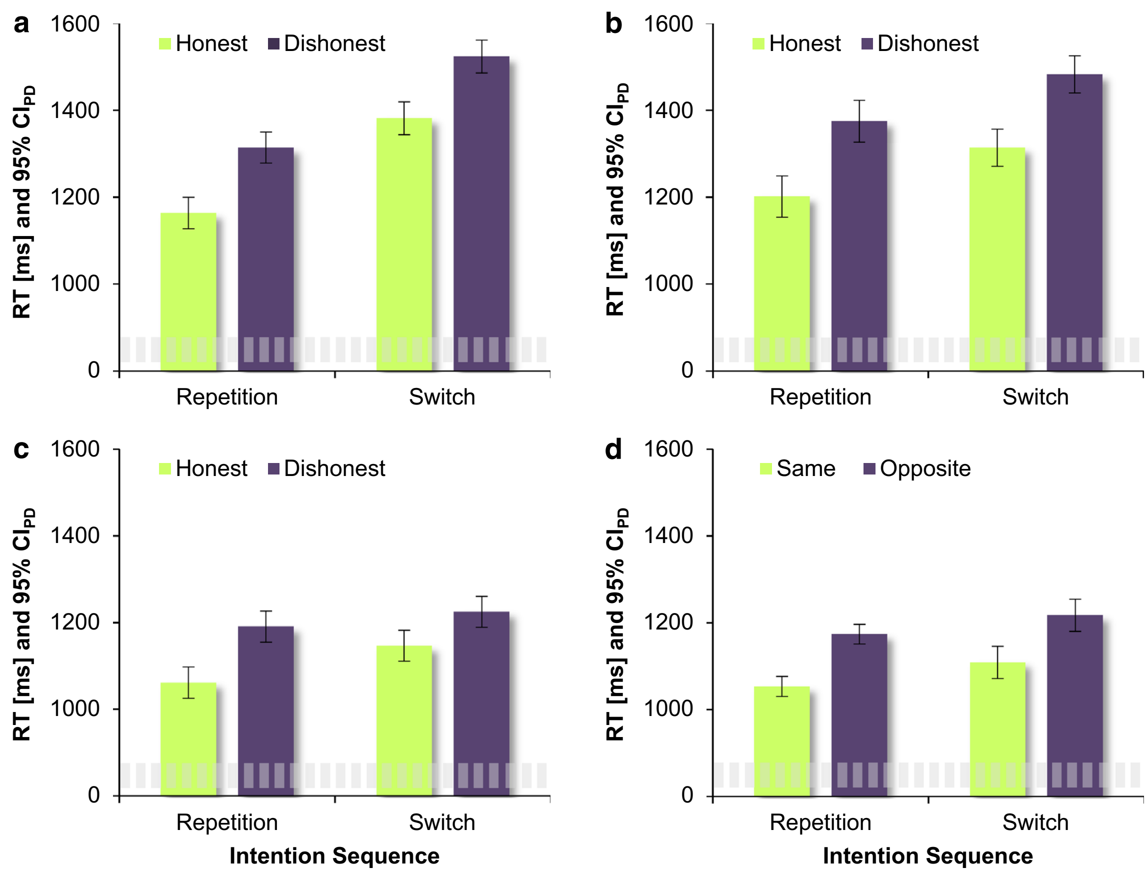

Fig. 3 Main results for the question responses. RTs are plotted as a function of intention in the current trial and the intention sequence for Exp. 1 (**a**), Exp. 2 (**b**), Exp. 3 (**c**) and Exp. 4 (**d**). Participants responded slower in dishonest than in honest trials and when intentions switched than when intentions repeated in all experiments. In Exp. 3 (**c**), the intention effect was more pronounced for intention repetition trials than for switch trials and switch costs were higher in honest than in dishonest trials. *Error bars* represent the 95 % confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013), computed separately for repetition trials and for switch trials

honestly ($\Delta = 4.6$ %), $F(1, 32) = 34.87$, $p < .001$, $\eta_p^2 = .52$, and when intentions switched than when intentions repeated from the preceding to the current trial ($\Delta = 6.3$ %), $F(1, 32) = 62.00$, $p < .001$, $\eta_p^2 = .66$. The interaction did not approach significance, $F < 1$. More errors were committed in dishonest switch trials than honest repetition trials ($\Delta = 10.9$ %), $t(32) = 8.79$, $p < .001$, $d = 1.53$, whereas participants tended to be more accurate in dishonest repetition trials than in honest switch trials ($\Delta = 1.7$ %), $t(32) = 1.78$, $p = .085$, $d = 0.31$.

Discussion

The experiment was conducted to examine aftereffects of honest and dishonest behavior in terms of repetition benefits and switch costs. As expected, it was more difficult for participants to switch from being honest to being dishonest and vice versa than to answer questions with the same intention in two subsequent trials. Symmetrical switch costs not only emerged in terms of prolonged RTs but also in terms of higher error rates. Hence, the results of the experiment are in line with previous findings on aftereffects of dishonesty (Debey et al., 2014a). Furthermore, dishonest responses were generally more error-prone and took longer than honest responses (see also, Pfister et al., 2014; Spence et al., 2001; Walczyk et al. 2003). These findings confirm that the present design allows to capture both, the immediate behavioral signature as well as the aftereffects of dishonest responding that we will scrutinize in the following experiments.

Experiment 2

Whereas Experiment 1 targeted aftereffects of dishonesty on subsequent honest and dishonest behavior, Experiment 2 introduced a new intervening task to study aftereffects of dishonesty on unrelated behavior. Each trial now featured a question and the corresponding response, followed by an unrelated choice reaction target and its response.

This intervening task allowed us to approach two questions. First, it allowed us to examine whether the switch

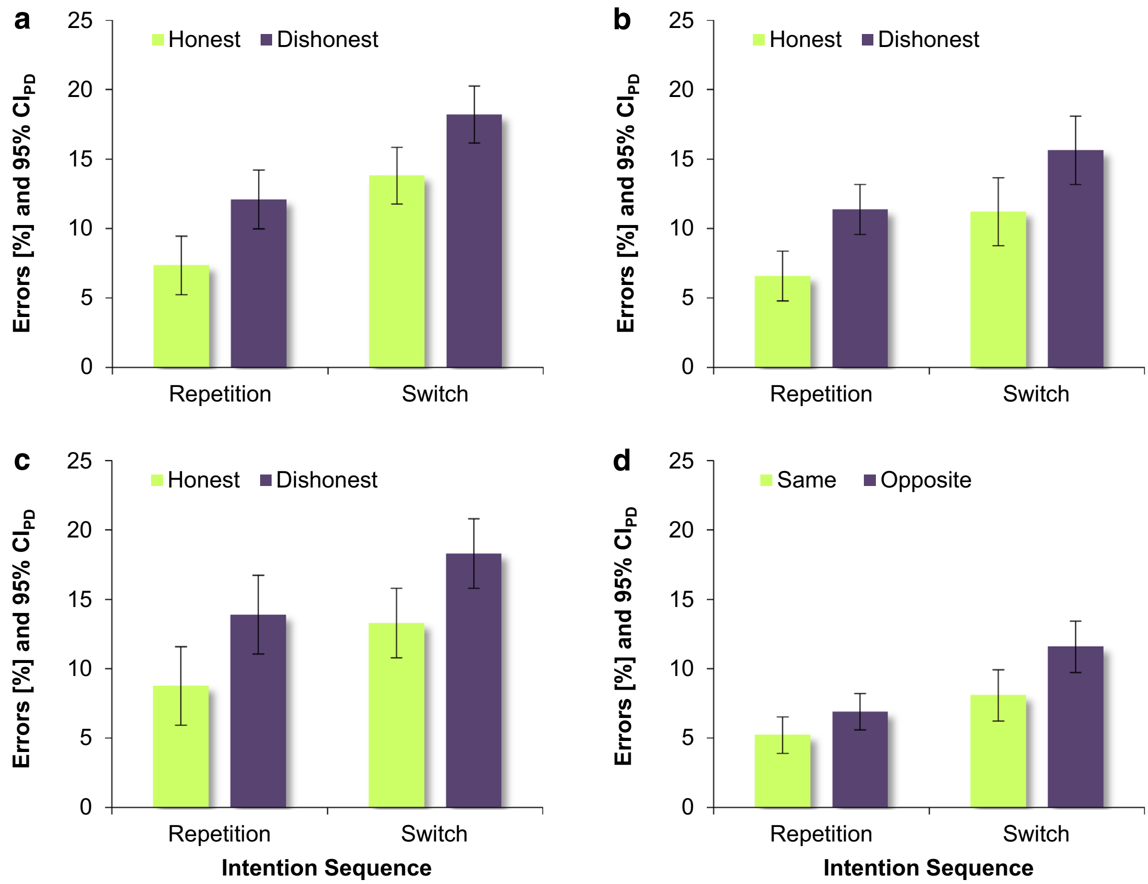

Fig. 4 Main results for the question responses. Error rates are plotted as a function of intention of the current trial and the intention sequence for Exp. 1 (**a**), Exp. 2 (**b**), Exp. 3 (**c**) and Exp. 4 (**d**). Participants committed more errors in dishonest than in honest trials and when intentions switched than when intentions repeated in all experiments. In Exp. 4 (**d**), the interaction between intention and intention sequence just failed to reach significance. *Error bars* represent the 95 % confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013), computed separately for repetition trials and for switch trials

costs observed in Experiment 1 would persist even when the questions did not follow in a rapid, uninterrupted succession. Following typical findings in task-switching experiments (Meiran et al., 2000), we assumed that the longer interval between questions should reduce activation of the preceding intentional set in the subsequent trial. This should result in a smaller sequence effect than in Exp. 1. Second, the intervening task allowed us to study aftereffects of dishonest responding on unrelated behavior. Following experiments on post-conflict slowing (Verguts et al., 2011), we expected prolonged RTs in the intervening task after dishonest responses compared to honest responses.

Methods

Participants, stimuli and apparatus

A new sample of thirty-two participants was recruited (4 male, 2 left-handed, mean age = 23.9 years) and received either monetary compensation or course credit. All participants gave informed consent. Stimuli and apparatus were as in Exp. 1 but an additional task followed directly after each question response. In this intervening task, participants saw three squares arranged in a horizontal line (cf. Verguts et al., 2011, for a similar setup). Either the left or the right square was filled white and participants were to press the left or right key accordingly.

Procedure

While the initial procedure, i.e., the assessment of the participant's true activities, was exactly the same as in Exp. 1, the procedure of the actual experiment was slightly adapted. In contrast to Experiment 1, the font color (yellow and blue) of the question (instead of the response options) indicated the current intention (Fig. 5). The question was followed by a 400 ms response–stimulus interval (RSI). In the intervening task, participants indicated the position of a target, i.e., a white filled square. Three squares appeared in a vertical distance of 40 % from the top of the screen. The

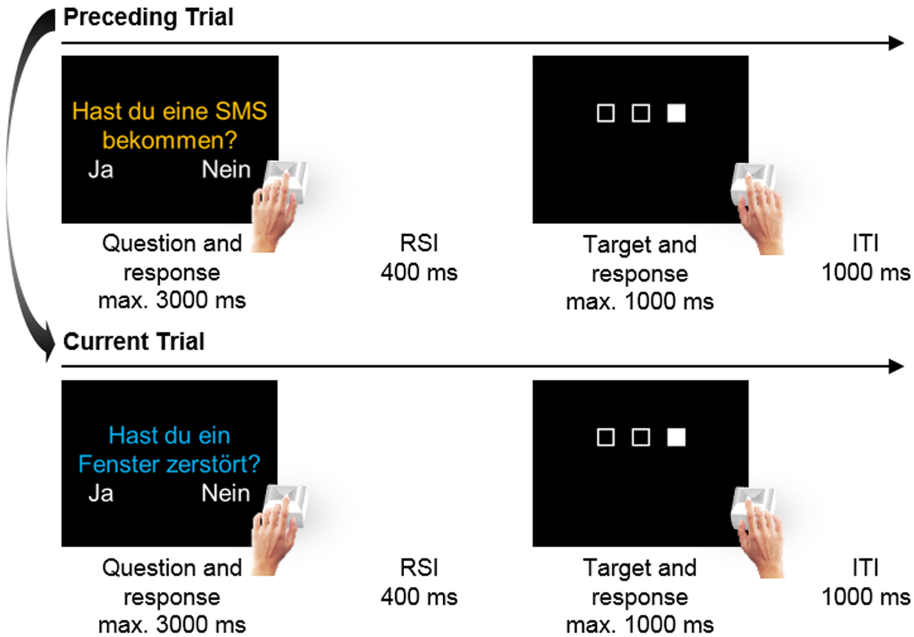

Fig. 5 Trial procedure of Experiment 2. Each trial featured one of the twenty questions (here: "Did you receive a text?" and "Did you break a window?"). Participants answered with *yes* or *no* either honestly or dishonestly, according to the font color of the question. Afterward, participants had to indicate the position of the target *square* (here: *right square*). For the question, the analyses focused on the impact of intention (honest vs. dishonest) on response times (RTs) and error rates and its sequential modulation by the preceding answer (intention repetition vs. switch). For the target, the analyses focused on the impact of intention on RTs and error rates

square in the middle was centrally positioned and the two target squares in a distance of 13 % to the left and right, respectively. Within 1000 ms, participants had to press the left key (*D*) if the left square was filled white or the right key (*K*) if the right square was filled white. The square in the middle served as reference point and did not change nor require any response. The next trial started after an ITI of 1000 ms. An error display was presented for 1000 ms if participants did not respond to the question or to the target within the according time frame, did not follow the instructed intention, chose the wrong square or pressed any other key than *D* or *K* in any of the tasks. Each question was presented twice in each block, i.e., once with honest and once with dishonest intention, respectively. There were 10 blocks à 40 trials.

Data treatment and analyses

Data treatment and question analyses were as in Exp. 1. In addition, RTs and error rates of the target responses were compared between honest and dishonest trials via two-tailed paired-samples *t*-tests. We also conducted between-experiment analyses to compare the results of Exp. 1 and 2. Therefore, question RTs and error rates were analyzed in a $2 \times 2 \times 2$ ANOVA with the within-subjects factors intention (honest vs. dishonest), intention sequence (repetition vs. switch) and the between-subjects factor experiment (Exp. 1 vs. Exp. 2). Please note that these between-experiment analyses do not represent the main objective of Experiment 2; rather the main objectives of Experiment 2 were (1) to address whether switch costs between honest and dishonest behavior would remain even with an unrelated task in between two question responses, and (2) to study aftereffects of dishonesty on this unrelated task.

Results

One participant was excluded based on the number of valid trials, which were considered for the RT analyses of both tasks. The participant deviated more than 2.5 standard deviations (SDs) from the other participants in at least on cell mean. Consequently, results are based on a sample of 31 participants.

Question RTs and error rates

Trials with erroneous question responses (12.5 %) and questions following error trials were excluded from RT analysis. The same was done with trials that used the same question as the preceding trial (2.3 %) and outliers (2.0 %). Prior to the analysis of error rates, omissions (1.3 %) and irrelevant commissions (<0.1 %) of the question in the current trial were eliminated.

Mean RTs for all conditions of interest are plotted in Fig. 3b. Participants were again slower in dishonest trials than in honest trials ($\Delta = 171$ ms), $F(1, 30) = 78.29$,

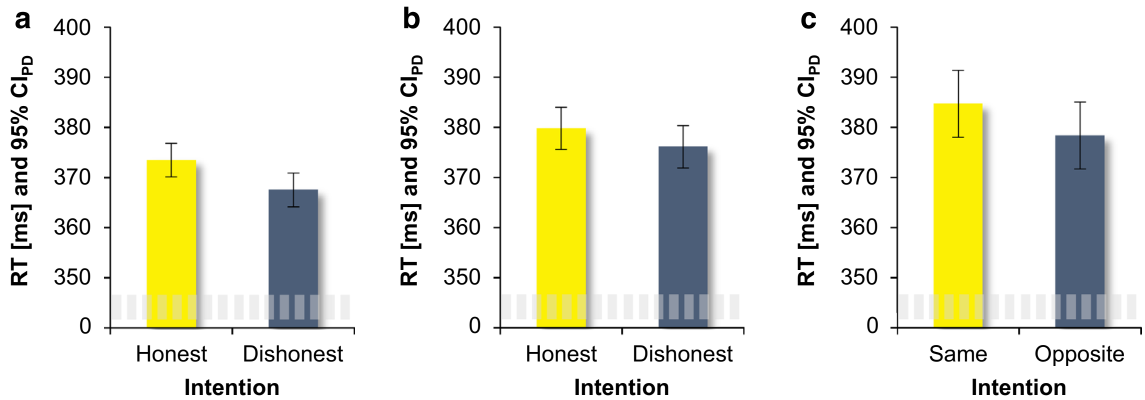

Fig. 6 RTs of the target responses as a function of the preceding intention of the question for Exp. 2–4. Participants were faster after dishonest than after honest question responses in Exp. 2 (**a**). This intention effect was evident as a non-significant trend in Exp. 3 (**b**) and 4 (**c**). *Error bars* represent the 95 % confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013)

$p < .001$, $\eta_p^2 = .72$. Participants were also slower when intentions switched from one trial to the next than when intentions repeated ($\Delta = 110$ ms), $F(1, 30) = 47.74$, $p < .001$, $\eta_p^2 = .61$. The interaction was not significant, $F < 1$. Dishonest switch trials were slower than honest repetition trials ($\Delta = 281$ ms), $t(30) = 10.41$, $p < .001$, $d = 1.87$, and dishonest repetition trials were slower than honest switch trials but the effect was much smaller than for the former comparison ($\Delta = 61$ ms), $t(30) = 2.67$, $p = .012$, $d = 0.48$.

A similar pattern was found for error rates (Fig. 4b). Participants were less accurate in dishonest than in honest trials ($\Delta = 4.6$ %), $F(1, 30) = 26.94$, $p < .001$, $\eta_p^2 = .47$, and when intentions switched than when intentions repeated ($\Delta = 4.5$ %), $F(1, 30) = 43.62$, $p < .001$, $\eta_p^2 = .59$. The interaction was not significant, $F < 1$. More errors were committed in dishonest switch trials than honest repetition trials ($\Delta = 9$ %), $t(30) = 7.51$, $p < .001$, $d = 1.35$, whereas no difference was evident between dishonest repetition trials and honest switch trials ($\Delta = 0$ %), $t(30) = 0.16$, $p = .878$, $d = 0.03$.

Between-experiment analyses: question RTs and error rates

Across both experiments, participants were slower when responding dishonestly than honestly, $F(1, 62) = 167.18$, $p < .001$, $\eta_p^2 = .73$, and when intentions switched than when intentions repeated from one trial to the next, $F(1, 62) = 251.59$, $p < .001$, $\eta_p^2 = .80$. However, the two-way interaction between intention sequence and experiment was significant, $F(1, 62) = 25.97$, $p < .001$, $\eta_p^2 = .30$, indicative of a larger effect of intention sequence in Exp. 1 compared to Exp. 2 ($\Delta = 104$ ms). Neither the main effect of experiment nor any of the remaining interactions were significant, $ps \geq .319$.

A similar pattern emerged for question error rates. Participants committed more errors in dishonest than in honest trials, $F(1, 62) = 61.12$, $p < .001$, $\eta_p^2 = .50$, and in intention switch trials than in intention repetition trials, $F(1, 62) = 104.31$, $p < .001$, $\eta_p^2 = .63$. The two-way interaction between intention sequence and experiment just failed to reach significance, $F(1, 62) = 3.04$, $p = .086$, $\eta_p^2 = .05$, indicative of a descriptively larger effect of intention sequence in Exp. 1 compared to Exp. 2 ($\Delta = 1.8$ %). Neither the main effect of experiment nor any of the remaining interactions were significant, $Fs < 1$.

Target RTs and error rates

All error trials (14.2 %) and outliers (2.3 %) were excluded prior to RT analysis. To analyze error rates, omissions (0.5 %) and irrelevant commissions (0.1 %) of the target and trials with any error in the question task (12.5 %), were excluded beforehand.

The resulting mean RTs and error rates are plotted in Figs. 6a and 7a, respectively. Contrary to our predictions, participants indicated the position of the white filled square *faster* after dishonest than after honest responses ($\Delta = 6$ ms), $t(30) = 3.61$, $p < .001$, $d = 0.65$. The analysis of error rates yielded a non-significant trend toward higher error rates after dishonest than after honest responses ($\Delta = 0.5$ %), $t(30) = 2.02$, $p = .053$, $d = 0.36$.[2]

[2] Following the suggestions of an anonymous reviewer, we reanalyzed target RTs and error rates in a 2 × 2 ANOVA with the within-subjects factor intention (honest vs. dishonest) and response sequence (repetition vs. switch). The latter factor describes whether the key press response repeated or switched from question to target. Target RTs in Exp. 2 yielded a marginally significant interaction of intention and response sequence for Exp. 2, $F(1, 30) = 3.34$, $p = .078$, $\eta_p^2 = .10$ which suggested a trend toward a more pronounced effect of intention for response switches. The same interaction was not significant for error rates, $F < 1$. We also did not find any significant interaction of the two factors in Exp. 3 and 4, $ps \geq .107$.

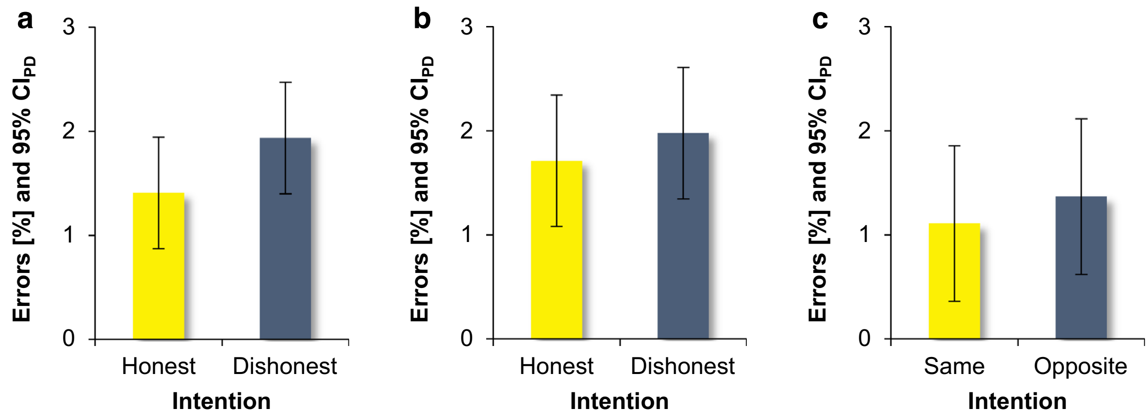

Fig. 7 Error rates of the target responses as a function of the preceding intention of the question for Exp. 2–4. Participants showed a non-significant trend toward committing more errors after dishonest than after honest question responses in Exp. 2 (**a**). No difference between the intention conditions was found in Exp. 3 (**b**) and 4 (**c**). *Error bars* represent the 95 % confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013)

Discussion

Experiment 2 featured a univalent task that followed honest and dishonest responses to simple yes/no questions. Despite the intervening task, the results replicated the pattern observed in Experiment 1 in terms of robust effects for intention and intention sequence. However, the introduction of the intervening task between honest and dishonest responding and extension of the ITI reduced the sequence effect considerably on RTs and descriptively on error rates, suggesting that the previous mind set decayed over time. Furthermore, aftereffects of dishonesty on the performance in the univalent task were observed. Surprisingly, and contrarily to our hypothesis, participants were faster but also tended to commit more errors after dishonest compared to honest responses. Rather than slowing down the participants' responses as observed after cognitive conflict (Verguts et al., 2011), dishonesty seems to cause a shift of the current speed-accuracy criterion. We will come back to this observation in the "General discussion".

Experiment 3

In the preceding experiments, participants were not able to predict how they would be instructed to answer the next question, i.e., honestly or dishonestly, and could therefore not prepare either intentional set. Exp. 3 therefore targeted the impact of preparation on honest and dishonest responding as well as the corresponding aftereffects. We introduced a cue that announced intention shortly before question onset, and expected the preparation interval between cue and question to reduce intention effects, sequence effects and improve overall performance of participants' responding to questions as compared to Exp. 2.

Methods

Participants, stimuli and apparatus

Thirty-two new volunteers were invited for participation (6 male, all right-handed, mean age = 24.5 years). They received either monetary compensation or course credit. All participants gave informed consent. The experiment used the same stimuli and apparatus as Exp. 2 but a cue was introduced in form of a colored frame.

Procedure

Participants went through the same tasks as in Exp. 2 but the trial procedure was slightly adapted again to allow for preparation of the current intentional set (Fig. 8). Now, a cue preceded the question to indicate if the following question should be answered honestly or dishonestly. The cue was a colored frame (yellow or blue) that was shown together with the answers *yes* and *no* in white font on a black screen for 1000 ms. Afterward, the question appeared in white font in the frame and participants had to answer it according to the intention that was instructed by the colored frame within 3000 ms. Like in Exp. 2, the participant's response was followed by a 400 ms RSI and the same target display. Error feedback was shown for 1000 ms if participants responded during the presentation of the cue.

Data treatment and analyses

Our main analyses were similar to Exp. 2. In addition, we conducted between-experiment analyses to compare the results of Exp. 2 and 3. Therefore, question RTs and error rates were analyzed in a 2 × 2 × 2 ANOVA with the

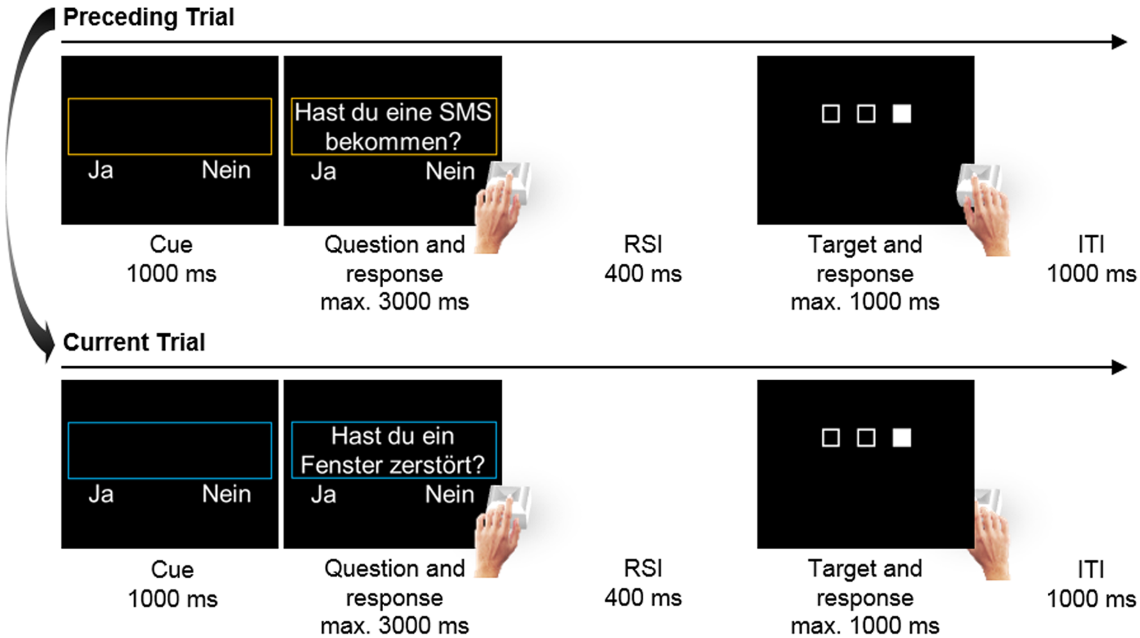

Fig. 8 Trial procedure of Experiment 3 and 4. A cue, i.e., a *colored frame*, indicated the intention (Exp. 3: honest vs. dishonest; Exp. 4: same vs. opposite). The cue stayed on the screen when the question appeared (here: "Did you receive a text?" and "Did you break a window?"). Participants answered with *yes* or *no*, according to the instructed intention. Afterward, participants had to indicate the position of the target *square* (here: *right square*). For the question responses, the analyses focused on the impact of intention (honest vs. dishonest) on RTs and error rates and its sequential modulation by the preceding answer (intention repetition vs. switch). For the target, the analyses focused on the impact of intention on RTs and error rates

within-subjects factors intention (honest vs. dishonest), intention sequence (repetition vs. switch) and the between-subjects factor experiment (Exp. 2 vs. Exp. 3). Furthermore, target RTs and error rates were examined in a 2 × 2 ANOVA with the within-subjects factor intention (honest vs. dishonest) and the between-subjects factor experiment (Exp. 2 vs. Exp. 3).

Results

In application of the same rules as in Exp. 2, one participant was identified as outlier which left 31 participants for statistical analyses.

Question RTs and error rates

Trials with erroneous question responses (17.8 %) and questions following errors were excluded from analysis prior to question RT analysis. The same was done with trials that used the same question as the trial before (2.4 %) and outliers (1.4 %). Prior to error rates analysis, omissions (4.8 %) and irrelevant commissions (<0.1 %) of the question in the current trial were eliminated, as well as trials where participants responded during the cue display (0.1 %).

Mean RTs for all conditions of interest are plotted in Fig. 3c. Participants gave slower dishonest responses than honest responses ($\Delta = 104$ ms), $F(1, 30) = 44.09$, $p < .001$, $n_p^2 = .60$. They were also slower when intentions switched than when intentions repeated ($\Delta = 60$ ms), $F(1, 30) = 31.19$, $p < .001$, $n_p^2 = .51$. The main effects were qualified by a significant interaction, $F(1, 30) = 10.88$, $p = .003$, $n_p^2 = .27$. Follow-up analyses revealed that intention switch costs were larger in honest ($\Delta = 85$ ms), $t(30) = 6.31$, $p < .001$, $d = 1.13$, than in dishonest trials ($\Delta = 34$ ms), $t(30) = 2.65$, $p = .013$, $d = 0.48$. Furthermore, the intention effect was larger when intentions repeated ($\Delta = 129$ ms), $t(30) = 7.39$, $p < .001$, $d = 1.33$, than when intentions switched between trials ($\Delta = 78$ ms), $t(30) = 4.51$, $p < .001$, $d = 0.81$. Dishonest switch trials were slower than honest repetition trials ($\Delta = 164$ ms), $t(30) = 8.67$, $p < .001$, $d = 1.56$, and dishonest repetition trials were slower than honest switch trials but the effect was much smaller than for the former comparison ($\Delta = 44$ ms), $t(30) = 2.33$, $p = .027$, $d = 0.42$.

Mean error rates for all conditions of interest are plotted in Fig. 4c. Error rates were higher in dishonest compared to honest trials ($\Delta = 5.1$ %), $F(1, 30) = 21.04$, $p < .001$, $n_p^2 = .41$, and when intentions switched than when intentions repeated ($\Delta = 4.5$ %), $F(1, 30) = 31.54$, $p < .001$, $n_p^2 = .51$. The interaction was not significant, $F < 1$. More errors were committed in dishonest switch trials than in honest repetition trials ($\Delta = 9.6$ %), $t(30) = 6.72$,

$p < .001$, $d = 1.21$, whereas no difference was evident between dishonest repetition trials and honest switch trials ($\Delta = 0.6$ %), $t(30) = 0.47$, $p = .643$, $d = 0.08$.

Between-experiment analyses: question RTs and error rates

In Exp. 2 and Exp. 3, dishonest responses took longer than honest responses, $F(1, 60) = 122.21$, $p < .001$, $\eta_p^2 = .67$, and RTs in intention switch trials were prolonged compared to intention repetition trials, $F(1, 60) = 78.31$, $p < .001$, $\eta_p^2 = .57$. In general, RTs were faster in Exp. 3 compared to Exp. 2. ($\Delta = 187$ ms), $F(1, 60) = 17.00$, $p < .001$, $\eta_p^2 = .22$. There was a significant two-way interaction between intention and experiment, $F(1, 60) = 7.26$, $p = .009$, $\eta_p^2 = .11$, indicative of a larger effect of intention in Exp. 2 than in Exp. 3 ($\Delta = 67$ ms). Furthermore, there was a significant two-way interaction between intention sequence and experiment, $F(1, 60) = 6.89$, $p = .011$, $\eta_p^2 = .10$, indicative of a larger effect of intention sequence in Exp. 2 than in Exp. 3 ($\Delta = 50$ ms). The three-way interaction just failed to reach significance, $F(1, 60) = 2.96$, $p = .091$, $\eta_p^2 = .05$, driven by the significant interaction between intention and intention sequence in Exp. 3 but no such interaction in Exp. 2.

Participants committed more errors in dishonest than in honest trials, $F(1, 60) = 46.57$, $p < .001$, $\eta_p^2 = .44$, and in intention switch trials than in intention repetition trials, $F(1, 60) = 73.20$, $p < .001$, $\eta_p^2 = .55$. Neither the main effect of experiment nor any of the remaining interactions were significant, $ps \geq .226$.

Target RTs and error rates

All error trials (19.5 %) and outliers (2.7 %) were excluded prior to the RT analysis. To analyze error rates, omissions (0 .7 %) and irrelevant commissions (0.1 %) of the target and trials with any error in the question task (17.8 %) were beforehand excluded.

Mean RTs and error rates for all conditions of interest are plotted in Figs. 6b and 7b. Participants showed a non-significant trend toward faster responses after dishonest compared to honest responses ($\Delta = 4$ ms), $t(30) = 1.77$, $p = .086$, $d = 0.32$ whereas error rates were similar after honest and dishonest responses ($\Delta = 0.3$ %), $t(30) = 0.86$, $p = .398$, $d = 0.15$.

Between-experiment analyses: Target RTs and error rates

Participants were faster, $F(1, 60) = 13.27$, $p = .001$, $\eta_p^2 = .18$, and committed more errors, $F(1, 60) = 3.84$, $p = .005$, $\eta_p^2 = .06$, after dishonest than after honest responding, whereas neither the main effect of experiment nor the interaction was significant in both analyses, $Fs < 1$.

Discussion

The results of Experiment 3 indicate that participants were able to prepare the appropriate intentional set for honest or dishonest responding in advance. Participants gave much faster responses to questions and showed considerably reduced but still robust intention and sequence effects in RTs as compared to Exp. 2 whereas they were not able to improve accuracy. Regarding RTs, preparation reduced the performance deficit that is caused by dishonesty and switches. Noteworthy, switch costs were now considerably higher when participants switched from dishonest to honest trials than vice versa. This observation points toward a critical role of preparation for the occurrence of asymmetrical switch costs (e.g., Allport et al., 1994; Meiran, 1996). Participants may have engaged in pronounced preparation when dishonesty was announced as when honesty was announced.

Experiment 4

The three experiments presented so far successfully disclosed a range of immediate effects and aftereffects of dishonest responding. However, it is not clear yet whether the observed effects are specific for dishonest behavior or if the same pattern of results can be found with different instructions that require nevertheless the same behavior as the dishonesty instruction. Exp. 4, therefore, provided a novel control condition that was designed to test for potentially distinct effects of the dishonesty instruction on intention and sequence effects. Accordingly, the trial procedure was exactly the same as in Exp. 3 but the instructions were changed. Participants were not instructed to answer honestly or dishonestly, but to take the perspective of two different agents (akin to common procedures in research on perspective taking, e.g., Batson, Early, & Salvarani, 1997). The first agent was supposed to have had the same experiences as the participant whereas the second agent had the opposite experiences of the participant. The cue color signaled for which person they had to respond in the upcoming question.

Methods

Participants, stimuli and apparatus

A new sample of thirty-two volunteers was invited for participation (14 male, 3 left-handed, mean age = 30.0 - years). They received either monetary compensation or course credit. All participants gave informed consent. Stimuli and the apparatus were exactly as in Exp. 3.

Procedure, data treatment and analyses

Exp. 4 only differed from Exp. 3 in terms of the instruction (Fig. 8). Now, participants were instructed that the experiment tested their ability to put themselves in the position of others. Accordingly, they had to answer the questions on either for a person that had had the same experiences as them or for another person that had had opposite experiences on that day. The color of the frame indicated for which person they had to answer the question in the current trial.

The same analyses as in Exp. 2 and Exp. 3 were applied, except that the levels of the factor intention were redefined as “same” vs. “opposite”. Between-experiment analyses compared the results of Exp. 3 and Exp. 4.

Results

In application of the same rules as in Exp. 2, one participant was identified as outlier which left 31 participants for statistical analyses.

Question RTs and error rates

Trials with erroneous question responses (11.8 %) and questions following errors were excluded from analysis prior to question RT analysis. The same was done with trials that used the same question as the trial before (2.6 %) and outliers (1.3 %). Prior to error rates analysis, omissions (4.2 %) and irrelevant commissions (.1 %) of the question in the current trial and trials were participants responded during the cue display (<0.1 %) were eliminated.

Mean RTs for all conditions of interest are plotted in Fig. 3d. Participants gave slower responses in opposite than in same intention trials ($\Delta = 115$ ms), $F(1, 30) = 80.14$, $p < .001$, $n_p^2 = .73$, and in intention switch compared to repetition trials ($\Delta = 50$ ms), $F(1, 30) = 20.83$, $p < .001$, $n_p^2 = .41$. The interaction was not significant, $F < 1$. Opposite switch trials were slower than same repetition trials ($\Delta = 165$ ms), $t(30) = 9.83$, $p < .001$, $d = 1.77$, and opposite repetition trials were slower than same switch trials but the effect was much smaller than for the former comparison ($\Delta = 65$ ms), $t(30) = 3.85$, $p = .001$, $d = 0.69$.

Mean error rates for all conditions of interest are plotted in Fig. 4d. Participants were less accurate in opposite compared to same intention trials ($\Delta = 2.6$ %), $F(1, 30) = 17.32$, $p < .001$, $n_p^2 = .37$, and when intentions switched than when intentions repeated from one trial to the next ($\Delta = 3.8$ %), $F(1, 30) = 43.88$, $p < .001$, $n_p^2 = .59$. The interaction just failed to reach significance $F(1, 30) = 3.72$, $p = .063$, $n_p^2 = .11$. Follow-up analyses showed that the intention effect was smaller in intention repetition trials ($\Delta = 1.7$ %), $t(30) = 2.61$, $p = .014$, $d = 0.47$, than in intention switch trials ($\Delta = 3.5$ %), $t(30) = 3.88$, $p = .001$, $d = 0.70$. Furthermore, intention switch costs were larger in opposite intention trials ($\Delta = 4.7$ %), $t(30) = 6.31$, $p < .001$, $d = 1.13$, than in same intention trials ($\Delta = 2.9$ %), $t(30) = 3.87$, $p = .001$, $d = 0.69$. More errors were committed in dishonest switch trials than honest repetition trials ($\Delta = 6.4$ %), $t(30) = 7.43$, $p < .001$, $d = 1.33$, whereas no difference was evident between dishonest repetition trials and honest switch trials ($\Delta = 1.2$ %), $t(30) = 1.43$, $p = .164$, $d = 0.26$.

Between-experiment analyses: question RTs and error rates

In Exp. 3 and Exp. 4, participants were slower in dishonest/opposite compared to honest/same trials, $F(1, 60) = 116.84$, $p < .001$, $n_p^2 = .66$, and when intentions switched as when intentions repeated from one trial to the next, $F(1, 60) = 51.40$, $p < .001$, $n_p^2 = .46$. These main effects were qualified by a significant two-way interaction between intention and intention sequence, $F(1, 60) = 7.96$, $p = .006$, $n_p^2 = .12$. The three-way interaction just failed to reach significance, $F(1, 60) = 3.00$, $p = .089$, $n_p^2 = .05$, indicative of a significant interaction between intention and intention sequence in Exp. 3 but no such interaction in Exp. 4. Neither the main effect of experiment nor any of the remaining interactions were significant, Fs < 1.

More errors occurred for dishonest/opposite compared to honest/same responses, $F(1, 60) = 36.43$, $p < .001$, $n_p^2 = .38$, and for intention switch trials than for intention repetition trials, $F(1, 60) = 70.93$, $p < .001$, $n_p^2 = .54$. Responses were more error-prone in Exp. 3 compared to Exp. 4 ($\Delta = 5.7$ %), $F(1, 60) = 9.29$, $p = .003$, $n_p^2 = .13$. The two-way interaction between intention and experiment just failed to reach significance, $F(1, 60) = 3.84$, $p = .055$, $n_p^2 = .06$, indicative of a descriptively larger effect of intention in Exp. 3 than in Exp. 4 ($\Delta = 2.5$ %). None of the remaining interactions approached significance, $ps \geq .225$.

Target RTs and error rates

All error trials (13.0 %) and outliers (2.4 %) were excluded prior to the RT analysis. To analyze error rates, omissions (0.4 %) and irrelevant commissions (<0.1 %) of the target and trials with any error in the question task (11.8 %), were beforehand excluded.

Mean RTs and error rates for all conditions of interest are plotted in Figs. 6c and 7c. Again, there was a non-significant trend toward faster responses after opposite than after same responses ($\Delta = 6$ ms), $t(30) = 1.94$, $p = .062$, $d = 0.35$ whereas error rates did not differ between same and opposite trials ($\Delta = 0.3$ %), $t(30) = 0.71$, $p = .483$, $d = 0.13$.

Between-experiment analyses: target RTs and error rates

Across Exp. 3 and Exp. 4, participants responded faster after dishonest compared to honest responding, $F(1, 60) = 6.68$, $p = .012$, $n_p^2 = .10$. The main effect of experiment and the interaction did not approach significance, Fs < 1. Neither the main effects, nor the interaction were significant in the analysis of the target error rates, ps $\geq .132$.

Discussion

The experiment was conducted to identify potentially distinct intention and sequence effects of the dishonesty instruction. With the new instruction, participants answered the questions more accurately and produced a somewhat smaller intention effect on error rates compared to Exp. 3. In contrast to Exp. 3, symmetrical switch costs in RTs but asymmetrical switch costs in error rates emerged. Furthermore, the asymmetrical patterns differed considerably between the two experiments as switch costs in error rates were more pronounced for opposite than for same intention trials whereas participants needed more time to switch from dishonest to honest responding in Exp. 3. Likewise, the intention effect was larger for intention repetitions than switches in RTs in Exp. 3, whereas the opposite was true for error rates in Exp. 4. Despite these differences regarding the symmetry of switches, the results are generally very similar to those that were obtained under dishonesty instructions, with the notable difference that participants seem to have engaged in less preparation under the opposite instruction than under the dishonest instruction.

General discussion

Four experiments examined effects and aftereffects of honest and dishonest responding from a perspective of changing intentional sets. Therefore, participants answered simple yes/no questions honestly and dishonestly in sequence. An intervening task and an intention cue were subsequently introduced to examine interference and preparation effects as well as aftereffects of dishonesty on unrelated behavior. We will begin by discussing the aftereffects on honest and dishonest behavior, as well as the corresponding preparation effects. Afterward, we will discuss the aftereffects of dishonest responding on unrelated behavior. The last section gives a prospect of the documented sequence effects as a cue to detect dishonest responses.

Aftereffects on honest and dishonest responding

In accordance with recent evidence (Debey et al., 2014a) and traditional task switch observations (e.g., Arrington & Logan, 2004; Meiran, 1996; Rogers & Monsell, 1995), switches from honest to dishonest responding and vice versa produced robust behavioral costs, i.e., higher RTs and error rates, compared to intention repetitions in Exp. 1 to Exp. 3. The introduction of the intervening task and the prolonged ITI reduced but, crucially, did not eliminate switch costs from Exp. 1 to Exp. 2. Hence, the preceding honest and dishonest mind set seemed to have decayed over time, which was disadvantageous in repetition trials and beneficial in switch trials, but also survived the extra amount of time as well as the implementation of another task set in between two questions.

In several investigations, intention effects decreased or reversed when the proportion of dishonest trials was higher than that of honest trials compared to a balanced condition (van Bockstaele et al., 2012; Verschuere et al., 2011). Likewise, the intention effect was pronounced when the proportion of honest trials was higher than the proportion of dishonest trials. The current experiments offer a re-interpretation of these results. Comparisons of dishonest switch trials (i.e., switches from honest to dishonest responding) with honest repetition trials revealed much larger intention effects than the comparison of dishonest repetition trials with honest switch trials (i.e., switches from dishonest to honest responding). The latter comparison even produced a reversed intention effect in Exp. 1. Hence, it is likely that different proportions of switch and repetition trials for the two intentions contributed to the effects of proportion of dishonesty and recent evidence supports this assumption (van Bockstaele, Wilhelm, Meijer, Debey, & Verschuere, 2015). Accordingly, future manipulations of proportion of dishonest trials should be accompanied by sequential analyses to scrutinize the contribution of switches and repetitions to the data pattern.

Even though switches triggered robust effects on RTs and error rates, they did not modulate the typically observed effect of slower and more error-prone dishonest responses in switch trials compared to honest responses in switch trials when participants were not informed about whether they were to answer honestly or dishonestly prior to question onset (e.g., Pfister et al., 2014; Spence et al., 2001; Walczyk et al., 2005). However, the intention effect varied with regard to repetitions or switches when a cue announced intention for the upcoming question shortly before question onset. In particular, the intention effect was larger in repetition than in switch trials and switch costs from dishonest to honest responding were larger than switches from honest to dishonest responding whereas quite the opposite was found when participants were instructed to respond from the perspective of another person.

Typically, asymmetrical switch costs emerge when one task is more dominant, e.g., a switch from color-naming to word-reading in a Stroop task appears to be more difficult than the reversed switch (Allport et al., 1994). The authors explained the results as an indicator of an easier, more dominant word-reading task. The less dominant color-naming task thus affords a stronger activation than the word-reading task, rendering it more difficult for participants to switch from color-naming to word-reading because word-reading was strongly inhibited and color-naming strongly activated before (Allport et al., 1994). Likewise, honest responses are claimed to be activated automatically whereas dishonest responses need to overcome this automatic tendency and are, thus, rather deliberate and more difficult (e.g., Vrij et al., 2008a; Walczyk et al., 2014). Because the honest response is typically seen as more dominant than the dishonest response, switches could be expected to be more difficult from dishonest to honest responding than in the reverse direction. However, we only found such an asymmetry in Exp. 3, whereas Exp. 1 and 2 and the two experiments reported by Debey et al. (2014a) found symmetrical switch costs.

In comparison to other findings on the asymmetry of switch costs, the moderating role of the preparation interval in our experiments is rather surprising (Allport et al., 1994; Kiesel et al., 2010; Meuter & Allport, 1999; Monsell, Yeung, & Azuma, 2000; Yeung & Monsell, 2003). In particular, a typical asymmetrical data pattern with increased switch costs for the more dominant task was observed but disappeared or even reversed with enhanced time for preparation although the interference of the two tasks remained evident (Yeung & Monsell, 2003). Contrarily, allowing participants to prepare for the upcoming honest or dishonest task in Exp. 3, gave rise to the typical pattern of asymmetrical switch costs. There is a notable difference between the two studies that could account for the diverging results. In the experiment of Yeung and Monsell (2003; Exp. 1, Exp. 1A) participants had to execute the tasks in a predictable and fixed sequence. In Exp. 1 and 2 of the current study, the intention cue and the question were presented simultaneously and the intention varied randomly across the experiment. Presumably, time was too short to strategically enhance activation and/or inhibition of a particular intentional set to compensate for different levels of dominance or difficulty between both intentional sets. In contrast, the announcement of the upcoming intention prior to question onset in Exp. 3 resulted in generally improved performance and gave rise to asymmetrical switch costs with larger switch costs for honest responding as participants were able engage in preparation.

The occurrence of asymmetrical switch costs when the intentional set was cued in advance, suggests that preparation time prompts more inhibition of the honest intentional set when intending to be dishonest than of the dishonest intentional set when intending to be honest. Accordingly, a switch back to the honest mind set is more difficult than a switch from honest to dishonest responding. A non-significant trend towards a reversed pattern was found in error rates in Exp. 4 when participants were asked to respond for an agent that experienced the same activities and for an agent that had opposite experiences as them. In fact, the instruction resulted in same response rules as the honest and dishonest instruction. However, symmetrical instead of asymmetrical switch costs in RTs and reversed asymmetrical switch costs in error rates emerged for the new instructions. Following the argument above, both task sets in Exp. 4 appear to be more equally dominant and difficult and in consequence, trigger preparation processes of more similar intensity.

The results of the current experiments and of Yeung and Monsell (2003) clearly demonstrate that the strength of between task interference in terms of dominance or difficulty and the ability for preparation interact dynamically so that diverging patterns of (a)symmetry emerge even for the same combination of tasks. Speculatively, increasing the time for preparation even more when responding honestly or dishonestly might diminish and even reverse the observed asymmetric pattern. In a nutshell, symmetrical switch costs for honest and dishonest responding likely occur when participants cannot prepare for the appropriate intentional set prior to question onset. However, asymmetrical switch costs likely occur when a cue announces the intention before question onset. Then, enhanced preparation for dishonest responding leads to more costs for switches from dishonest to honest responding.

Aftereffects on unrelated behavior

Contrary to our predictions based on previous research on aftereffects of cognitive conflict, dishonest responding did not prolong responses in the intervening task compared to honest responding (Verguts et al., 2011). Instead performance in the intervening task was faster but tended to be less accurate after dishonest responses in Exp. 2. In Exp. 3 and 4, there were similar trends in RTs but no differences in error rates. Tentatively, these results indicate that participants were faster after dishonest responding but to some extent with losses in accuracy, indicative of a more liberal response criterion.

Closer inspection of the current paradigm and the paradigm used to investigate post-conflict slowing points toward several differences. Most importantly, the present questions seem to be more demanding than the Simon task used to trigger cognitive conflict (Verguts et al., 2011) by a substantial margin. The difference in cognitive demand becomes evident when comparing the mean RT of about 550 ms in the study by Verguts et al. (2011) to the fastest condition mean of the current study (>1000 ms). In addition to being overall more demanding, the intention effect of the current study (134 ms) also exceeded the congruency effect that triggered post-conflict slowing (44 ms). These two factors might at least partly explain the observed results. Furthermore, when participants wanted to respond dishonestly or oppositely, they had to derive their response from the honest/same response value (cf. Debey et al., 2014b; Vrij et al., 2008a; Walczyk et al., 2014). In the Simon task, by contrast, the relevant stimulus dimension sufficiently indicated the appropriate response. As the use of the irrelevant dimension was mandatory in our experiments, a strategic shift to more cautious responding in the intervening task may not have been attractive and beneficial for participants. Instead, participants may have tried to compensate worse performance in dishonest/opposite responding with better performance in the intervening task. Apart from such top–down control, the complexity of dishonest/opposite responding may have induced an increase in arousal and thus may have altered participants' readiness to respond (Cohen, 2011a, 2011b, see also Yerkes & Dodson, 1908).

The exact contributions of these explanations to the surprising data pattern of the current study certainly calls for further experimental investigation. In any case, the observed aftereffects of dishonest responding on performance in an unrelated task offer an innovative and unique window on the cognitive processes at work when responding dishonestly.

Methodological implications

Direct comparisons of honest and dishonest responding on measures such as response times come with a long-lasting tradition in research on the cognitive processes underlying dishonesty and lies (e.g., Debey et al., 2012, 2014; Pfister et al., 2014; van Bockstaele et al., 2012; Verschuere et al., 2011; Walczyk et al., 2003). Still, it has rarely been asked to which degree the resulting intention effects are specific to dishonest responding. This question is important because typical operationalizations of dishonesty do not include a true communicative setting in which the agent tries to deceive another human being. Rather, most paradigms revolve around yes/no answers to questions presented on the computer screen. As outlined in the introduction, this restriction is thought to come with the virtue of isolating the processes of activation and inhibition of the honest response option, but this assumption apparently has never been put to test.

To address this open issue, the current Experiment 4 adopted and modified a procedure that has been used to study the effects and aftereffects of rule violation behavior (Pfister, 2013; Pfister, Wirth, Schwarz, Steinhauser, & Kunde, 2016; Wirth, Pfister, Foerster, Huestegge, & Kunde, 2015). In these studies, participants were asked to follow an S-R mapping rule in a stimulus classification task, or they were asked to violate this rule by giving the wrong response. Akin to the intention effect in studies on dishonesty, rule violation took longer than rule-based responding (in addition to spatial attraction of movement trajectories toward the rule-conform target during rule violation). In one type of control condition, however, participants of these studies were instructed with two opposing task rules, with Task 1 featuring the original mapping rule and Task 2 featuring the reversed rule. In another type of control condition, they were asked to either use the original mapping rule or to invert the rule. This latter control condition thus defines an original task set (rule-based responding) and a derived task set (rule inversion), much like the instructions of Experiment 4.

Similar results for the behavior of interest (rule violation or dishonest responding) and the inversion condition suggest that the behavior in question is mainly driven by inversion or negation processing (for general findings regarding negation processing, see Clark & Chase, 1972; Gilbert, 1991; Strack & Deutsch, 2004). Indeed, rule violation behavior seems to be based on negation processing to a considerable degree (Wirth et al., 2015), even though negation processing alone cannot sufficiently explain the observed results. A similar conclusion can be drawn for the current paradigm of honest and dishonest responses to yes/no questions, as suggested by a comparison of Exp. 3 and 4. More precisely, similar main effects of intention and intention sequence emerged for the two experiments, suggesting that the use of simple yes/no question does indeed isolate the processes of inhibiting the automatically activated truth and negating it to arrive at the correct response option. At the same time, Exp. 3 and 4 differed in terms of the symmetry of switch costs. Merely labeling a response as "dishonest" might thus induce a tendency toward more pronounced preparation for the upcoming response than with the more neutral label of responding for a person who had had opposite experiences as oneself. This difference certainly calls for further investigation with control conditions as presented in the present Exp. 4.

Lie detection via sequential effects

The present experiments identified aftereffects of honest and dishonest responding on other honest and dishonest responses and on unrelated behavior. Especially, switch costs between honest and dishonest responding were of considerable size. Research on lie detection could take advantage of this robust and stable pattern of results as very similar experimental paradigms are already examined as a potential method for lie detection.[3] Switch costs could prove as an important cue to detect dishonesty in those designs. In particular, switch costs between honest and dishonest responding would be unique for liars because, by contrast, honest persons would not switch between intentions. Thus, switch costs may provide a unique grasp on the classification of truth-tellers and liars in procedures such as the Time Restricted Integrity-Confirmation (TRI-Con; Walczyk et al., 2005, 2009, 2012) and may improve detection rates of this procedure.

Initial versions of the TRI-Con procedure aimed at classifying truth-tellers and liars based solely on the difference in RTs between honest and dishonest responding as it is a robust finding in the literature (Debey et al., 2012; van Bockstaele et al., 2012; Pfister et al., 2014; Spence et al., 2001; Walczyk et al., 2003)—even though a robust group effect does not guarantee sufficient accuracy in lie detection for individual participants (Franz & von Luxburg, 2014). In the first two studies, the authors used simple and highly controlled question/response designs on the PC. Walczyk et al. (2005, 2009) instructed a group of participants to answer yes/no and open-ended biographical questions of different content domains honestly and another group was asked to answer these questions dishonestly. They were able to distinguish unrehearsed and rehearsed liars from truth-tellers better than chance in some of the content domains. Adjusting RT data for individual differences (i.e., computing difference values of honest responses to control questions and to be suspected dishonest responses of liars), and identifying inconsistencies in responding, improved classification rates considerably. A more recent study asked participants to take the role of a crime witness to set up a paradigm that is comparable to a criminal context (Walczyk et al., 2012). Participants watched videos of actual crimes and afterward, they answered questions of an interviewer about the crime honestly or dishonestly with unrehearsed or rehearsed lies. Besides RTs and inconsistencies, participants' eye movements and pupil dilation were assessed as well. Again, participants were classified as truth-tellers, unrehearsed and rehearsed liars better than chance with the help of adjusted response times, inconsistency in responding and eye movements. In the three studies, false positive classifications were low but not negligible as they are especially fatal in the criminal context when it comes to convictions. Furthermore, participants did not have to fear any consequences whereas suspects of a crime have to fear even life-changing consequences (Walczyk et al., 2012).

Walczyk and colleagues, thus, presented a very simple and elegant method that may be used to detect liars in the future. The current experiments featured a forced-choice design and every participant responded honestly and dishonestly. However, switch costs are also observed even when participants voluntarily switch between tasks (e.g., Arrington & Logan, 2004, 2005). Thus, it is reasonable to assume that switch costs are observed when participants decide on their own whether to respond honestly or dishonestly in the current trial. So in a slightly adjusted experimental design, the contribution of switch costs to the detection of dishonesty could be easily examined. Verifiable control questions may be mixed with critical questions that are expected to be answered dishonestly. Mixing costs could be considered within participants as well when participants answer the same control questions in absence as well as in presence of the critical questions in an experimental block.

Conclusion

Inspired by theories and methods developed in research on task switching, the current study took a new perspective on dishonesty. Whereas previous research examined immediate effects of dishonesty, the current investigation sets dishonest behavior in context by showing lasting aftereffects of honest and dishonest behavior and by providing a model that can account for these effects. Large and robust switch costs between honest and dishonest responding were observed. Asymmetrical switch costs, i.e., larger switch costs for honest responding, emerged when participants were able to prepare for the upcoming honest or dishonest task before question onset, pointing toward enhanced preparation when responding dishonestly with stronger activation of the difficult and more complex dishonest mind set. Even more, the present experiments are the first to demonstrate aftereffects of dishonest responding on unrelated behavior. The results thus support the assumption of distinct honest and dishonest mind sets which proved to be impressively stable. In addition to these theoretical

[3] Note that lie detection is usually achieved via physiological measures (e.g., Ben-Shakhar & Elaad, 2003; Gamer, Verschuere, Crombez, & Vossel, 2008; van't Veer, Gallucci, Stel, & van Beest, 2015; Vandenbosch, Verschuere, Crombez, & De Clercq, 2009). Approaches to lie detection via RTs could prove as a fruitful extension to these methods.

contributions, the current findings provide a basis for improving applied procedures for lie detection by drawing on the observed sequence effects.

Compliance with ethical standards

Conflict of Interest The authors declare that there is no conflict of interest.

Ethical approval All procedures were in accordance with the ethical standards of the institutional research committee and with the 1964 Helsinki declaration and its later amendments. Informed consent: Informed consent was obtained from all individual participants included in the study.

Appendix

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?
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?
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. Has*t 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?
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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