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Lying Upside-Down: Alibis Reverse Cognitive Burdens of Dishonesty

Anna Foerster, Robert Wirth, Oliver Herbort, Wilfried Kunde, and Roland Pfister
University of Würzburg

The cognitive processes underlying dishonesty, especially the inhibition of automatic honest response tendencies, are reflected in response times and other behavioral measures. Here we suggest that explicit false alibis might have a considerable impact on these cognitive operations. We tested this hypothesis in a controlled experimental setup. Participants first performed several tasks in a preexperimental mission (akin to common mock crime procedures) and received a false alibi afterward. The false alibi stated alternative actions that the participants had to pretend to have performed instead of the actually performed actions. In a computer-based inquiry, the false alibi did not only reduce, but it even reversed the typical behavioral effects of dishonesty on response initiation (Experiment1) and response execution (Experiment 2). Follow-up investigations of response activation via distractor stimuli suggest that false alibis automatize either dishonest response retrieval, the inhibition of the honest response, or both (Experiments 3 and 4). This profound impact suggests that false alibis can override actually performed activities entirely and, thus, documents a severe limitation for cognitive approaches to lie detection.

Keywords: dishonesty, lying, deception, lie detection

Responding truthfully on each and every occasion can yield negative consequences at times, and being dishonest may come as a convenient alternative in this case. People may thus withhold information that might be harmful if revealed, or they might even present incorrect but plausible information as true facts. Lies can be further told about different topics, and among these topics, autobiographical events are particularly relevant.

Lying about autobiographical events often comes with a *false alibi*, when incorrect information is provided to conceal or deny actual events and actions. Here, we understand false alibis as giving a false impression about which activities were performed and which were not performed. Such false alibis are especially important in criminal contexts, where guilty subjects are likely motivated to present such alibis.

In the current experiments, we examined the impact of false alibis on the behavioral traces of lying in a controlled experimental design. Understanding the effects of false alibis on dishonest processing is essential to assess whether such alibis constrain the potential of behavioral measures for forensic application (i.e., lie detection). In the following, we first review theoretical models and empirical findings on the cognitive basis of dishonesty, followed by recent observations that point toward factors that moderate the behavioral effects of dishonesty. These moderators also pave the way for an empirical approach to the effects of false alibis that motivated our experimental design.

Anna Foerster, Robert Wirth, Oliver Herbort, Wilfried Kunde, and Roland Pfister, Department of Psychology III, University of Würzburg.

Correspondence concerning this article should be addressed to Anna Foerster, Department of Psychology III, University of Würzburg, Röntgenring 11, 97070 Würzburg, Germany. E-mail: anna.foerster@uni-wuerzburg.de

The Cognitive Basis of Dishonesty

An influential approach to describing the cognitive processes underlying dishonest behavior is the Activation-Decision-Construction-Action Theory (ADCAT; Walczyk, Harris, Duck, & Mulay, 2014; for a former version of the theory, see Walczyk, Roper, Seemann, & Humphrey, 2003). The theory assumes that respondents usually activate a representation of the truth first. However, once the respondent decides to lie, based on the social context and previous decisions, the activated truthful response needs to be inhibited to construct and deliver a plausible lie. An action component also considers that the agent can control and monitor own behavior and monitor the behavior of the receiver of the lie. ADCAT further holds that the proposed processes can in principle operate simultaneously and automatically (Walczyk et al., 2014).

The assumption that dishonest responding requires the inhibition of the initially activated truthful response is supported by *instructed intention paradigms*. In these paradigms, participants respond, for example, to simple autobiographical questions and are instructed to respond with a particular intention, that is, honestly or dishonestly. Intention effects in terms of differences between honest and dishonest responding were observed in behavioral, electrophysiological and hemodynamical data. In particular, dishonest responding prolongs response times (RTs) and increases error rates, leads to an enhanced recruitment of brain regions that are associated with cognitive control, and alters electrophysiological signatures in a way that points toward less direct response retrieval (e.g., Bhatt et al., 2008; Johnson, Barnhardt, & Zhu, 2003, 2004; Pfister, Foerster, & Kunde, 2014; Spence et al., 2001; Suchotzki, Crombez, Smulders, Meijer, & Verschuere, 2015; Walczyk et al., 2003). When true and false responses are collected as continuous movements toward certain spatial target locations, movement trajectories steer toward the honest response option when responding dishonestly, revealing a continued influence of the truthful response option during response execution (Duran, Dale, & McNamara, 2010). A recent study further yielded direct evidence for the assumed cognitive detour from the honest to the dishonest response (Debey, de Houwer, & Verschuere, 2014). Participants saw questions together with truth or lie distractors, that is, the honest or dishonest response. Accordingly, the last word of the question appeared in a random position on the screen with "yes" or "no" written above and below the word. Honest and dishonest responding alike were facilitated in the presence of truth distractors compared to lie distractors, even though a truth distractors corresponded to the very opposite of a correct response in dishonest trials. In a nutshell, the available evidence reveals that the honest response has to be overcome for each act of dishonest responding.

The observation of longer RTs and higher error rates for lying also encouraged researchers to study the success of these measures in lie detection. In some of these studies, participants were tested as either truth-tellers or liars throughout the experiment, or they lied in specific domains while telling the truth in others (e.g., Walczyk, Mahoney, Doverspike, & Griffith-Ross, 2009; Walczyk et al., 2005, 2012). Accordingly, each question required either an honest or dishonest response. Even though these approaches yielded several promising findings, the resulting classification success is currently insufficient to use this method for lie detection outside the laboratory.

In the instructed intention paradigm, by contrast, cues inform participants in each trial whether to respond honestly or dishonestly, and each question is answered equally often with both intentions (e.g., Furedy, Davis, & Gurevich, 1988; Spence et al., 2001). Hence, the instructed intention paradigm maximizes effect sizes and, thus, provides a promising basis for lie detection. Using the instructed intention effect for lie detection, however, also requires a firm understanding of potential moderating factors, and we will therefore describe some of these factors in the following section.

Just How Basic Is the Basis?

The size of the intention effect in instructed intention paradigms is a direct function of differences in cognitive processing between honest and dishonest responding, and several factors modulate this difference. Rehearsing specific lies, for instance, facilitates lying up to a level where lying can become easier than responding honestly, provided that each question was responded to with only one intention throughout an experimental session (Hu, Chen, & Fu, 2012; Walczyk et al., 2009, 2012). Clearly, participants learn stimulus–response associations in this setting, where the automatically activated response seems to be the dishonest one instead of the honest (akin to storing "instances" of stimulus-response episodes; Logan, 1988).

Besides such item-specific learning, general changes in cognitive control settings influence dishonest processing in a sustained as well as transient manner: Having responded honestly or dishonestly changes future honest and dishonest responding. Sustained influences describe how lying is modulated by the frequency of dishonest behavior. To differentiate between stimulus-response learning and changes in control settings, the frequency of both intentions can be manipulated in inducer questions, while the frequency is held constant in test questions (Van Bockstaele et al., 2012; Verschuere, Spruyt, Meijer, & Otgaar, 2011). Typically, inducer questions had to be answered only honestly, only dishonestly, or both across different conditions. Test questions afforded an equal number of responses with both intentions irrespective of condition, and both question types appeared in a random sequence. The intention effect became smaller with a larger proportion of dishonest trials for inducer questions and, importantly, also for test questions. When responses were given in an environment with a balanced proportion of honest and dishonest trials afterward, this modulation only held for the inducer questions, which again indicates acquired stimulus–response associations (Van Bockstaele et al., 2012).

Similarly, control settings can change transiently, that is, from trial to trial. Therefore, honest and dishonest responses were analyzed as a function of the intention in the preceding trial, akin to methods of the literature on task switching (Debey, Liefooghe, de Houwer, & Verschuere, 2015; Foerster, Wirth, Kunde, & Pfister, 2016; for reviews on task switching, see Kiesel et al., 2010; Monsell, 2003). Repeated honest or dishonest responding was easier than switching between both intentions. Performance differences between honest and dishonest responding, however, were mostly unaffected by these switch costs, except when the upcoming intention was announced shortly before question onset (Foerster et al., 2016).

Switch costs between honest and dishonest responding also provide a new perspective on the studies on sustained influences described above. Because these studies manipulated the frequency of honest and dishonest trials, they also introduced varying ratios for repetition and switch trials. For example, a highly dishonest environment featured mostly repetitions of dishonest trials and rarely switches to dishonest responding, whereas honest trials were mostly intention switches and rarely repetitions. The observed effects of frequency manipulations could, thus, stem from transient instead of sustained changes in control settings or from a combination of both (Van Bockstaele et al., 2012; Debey et al., 2015; Foerster et al., 2016, 2017).

Another factor that affects dishonest responding in a sustained manner is instruction of deliberate response strategies (Hu, Chen, et al., 2012). Participants in this study first were naïve about the intention effect and responded honestly and dishonestly to information in separate blocks. Then they learned about the usual intention effect in performance and their own mean performance in the two blocks. They were asked to diminish the intention effect by speeding up responding in the dishonest block and indeed, participants were able to do so. Although the intention effect became smaller as dishonest RTs were decreased, it did not vanish entirely. This was only the case when participants went through an additional dishonest training block with the same information in which they conceivably acquired stimulus–response associations.

Overall, these results suggest that a dishonest response is retrieved directly by means of a stimulus when this association has been learned sufficiently well before, which can render dishonest responding as easy as honest responding. Otherwise, an initially activated honest response has to be overcome when responding dishonestly. This is easier when dishonest processing already took place recently or very frequently, diminishing the intention effect, and it is harder when previous or frequent responding was honest, enhancing the intention effect.

Interestingly, the effect of false alibis as a frequent companion of dishonest behavior has not yet been addressed in research on the behavioral consequences of dishonesty and in the evaluation of lie detection methods. The modulating influences described above indeed suggest that false alibis may alter the way dishonest behavior is processed. Following the existing evidence, preparation of false alibis could render the process of inhibiting the truth more efficient or change dishonest responding even more drastically to a process of directly retrieving the appropriate dishonest response. Investigating precisely this effect of false alibis is the goal of the present experiments. If false alibis change the way dishonest responses are processed, this would be relevant for the development of lie detection methods as for example the autobiographical implicit association test (Sartori, Agosta, Zogmaister, Ferrara, & Castiello, 2008), the concealed information test (e.g., Ben-Shakhar & Elaad, 2003) or tests that rely on the cognitive load induced by dishonesty (e.g., Walczyk et al., 2005).

Experiment 1

In Experiment 1, we aimed at examining the effects of a false alibi on (dis-)honesty in an instructed intention paradigm. To establish a situation that resembles applied forensic settings, the experiment was divided in two separate parts: a mission and an inquiry. In the mission, participants engaged in allegedly secret activities. After performing these activities, they received a false alibi that detailed a series of alternative actions. The inquiry took place on a computer with discrete yes/no responses via key press, and participants were to pretend to have had engaged only in the alibi activities and not in the activities they actually had performed. Accordingly, they were to respond dishonestly during the inquiry when asked to respond honestly, and they were to respond honestly when asked to respond oppositely about the mission. Note that the intention instructions in the inquiry were honest and “opposite” instead of honest and “dishonest.” This change in the instructed intention paradigm was introduced to make the instructions more applicable in forensic settings, as it would seem rather odd to ask actual suspects to respond honestly or dishonestly. As a baseline, to gauge potential alibi effects, we further included routine questions in the inquiry (relating to daily activities and unrelated to the mission) in addition to the mission questions.

In a nutshell, the experimental design established conditions in which participants responded in correspondence with their actually experienced activities, and conditions in which their responses and activities were noncorresponding. We hypothesized that responding would be easier when a response corresponded with the experiences of participants, for example, when participants gave an affirmative response when asked about activities they actually engaged in and negated questions about activities they did not engage in. Accordingly, the manipulation of activity-response correspondence should affect our behavioral measures, that is, error percentages and RTs. Participants should respond slower and less accurately in noncorresponding trials than in corresponding trials. The critical question was whether the false alibi would reduce this correspondence effect. Such an effect would be evident in reduced correspondence effects for mission questions (for which participants had an alibi) relative to routine questions (for which there was no alibi).

Method

Participants and overall procedure. A sample size of 44 participants was determined with a power analysis based on an effect size of $d = 0.5$, $\alpha = .05$ and a power of $1 - \beta = .90$ (calculated with the power.t.test function in R version 3.1.1). We used a generic medium effect size as a conservative estimate, because effects of dishonesty and their modulation are usually large in RTs and error rates (e.g., Foerster et al., 2016; Van Bockstaele et al., 2012). Participants gave written informed consent and received either monetary compensation or course credit (age: mean (*M*) = 20.6, standard deviation (*SD*) = 3.07; 39 female; 40 right-handed).

The experiment was divided in two separate parts as shown in Figure 1. In the first part, participants went through a mission in which they performed certain actions (e.g., drawing a triangle and a circle on a sheet of paper). By the end of the mission, they were informed about an upcoming inquiry regarding their activities and were instructed about plausible activities that they should pretend to have performed in this inquiry. In the second part, participants worked on the computerized inquiry and were asked to respond to a number of yes/no questions with button presses. Both parts are described in more detail in the following.

Mission. The experiment began with a mission that required participants to perform activities alone in a room. The following items in the room were important for the mission: a desk with a chair, a box on the desk containing a stack of empty sheets of paper and a pen, a box under the table with a slit, an USB stick on the desk next to the box, a computer with a screen, a keyboard and a mouse.

All participants received the same instructions through a letter (see Appendix A) in an envelope and were asked to strictly follow these instructions. Participants had to (a) sit down at the desk, (b) take a sheet of paper and the pen from a box on the desk, (c) draw a triangle and a circle on this sheet of paper, (d) put the pen back in the box on the table, (e) tear the sheet of paper in half, and (f) hide one piece under the stack of paper in the box on the desk and the other one in the box below the desk. Participants then learned from the letter that they would be questioned about these activities in a computerized inquiry afterward. Crucially, participants were led to believe that the majority of participants received different instructions. Namely, these other participants allegedly engaged in different activities with the remaining objects, and that the other participants had to be honest about these activities in the following inquiry. In contrast, the letter stated that the current participant was chosen for a special mission in this experiment to learn more about lie detection. This mission required hiding their true activities and pretending to have performed plausible alibi actions. Alibi actions were (a) to switch on the computer, (b) use the USB stick, (c) open a file called “table,” (d) write an e-mail, and (e) send the file via this e-mail. Participants were instructed to not actually engage in any of these activities.

The letter now explained the following inquiry in detail. Participants would not only be asked questions about their activities in the room—referred to as mission questions in the following—but also about activities they could have had experienced or not experienced on the same day—referred to as routine questions in the following. They also learned that, on each trial, they would either be asked to respond honestly or to give the opposite re-

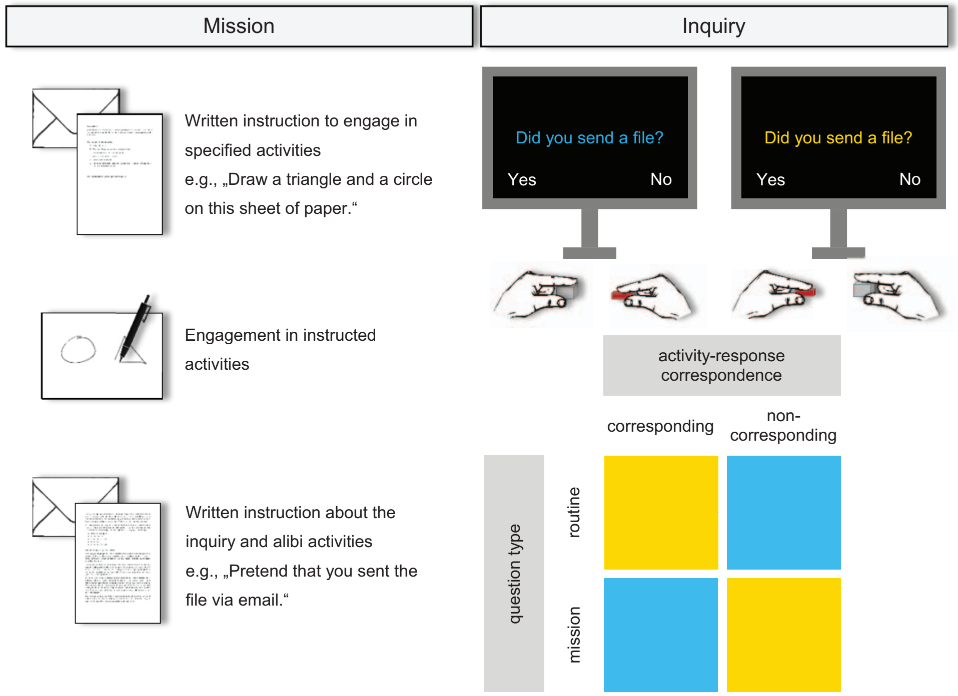

Figure 1. Design of Experiment 1. Participants responded to routine and mission questions with yes or no. The font color of the question cued whether participants should give an honest (here: yellow [light gray]) or an opposite to honest response (here: blue [dark gray] type; for legibility, question and response labels are not drawn to scale). Participants had to follow these cues when they responded to routine questions but adapt their responses for mission questions. Participants gave responses that corresponded or did not correspond with their actual activities for routine and mission questions. For mission questions, participants were supplied with an explicit false alibi that stated a series of alternative actions that they were to pretend to have performed instead of their actual activities. For example, participants had to affirm all alibi actions and deny performed actions when the question appeared in yellow (light gray) font (indicating an honest response), whereas they had to deny the alibi actions and to affirm the performed actions when the question appeared in blue font (indicating an opposite response). See the online article for the color version of this figure.

sponse. How to respond would be indicated by the color of the question (the exact assignment of color to honest or opposite was provided in the inquiry).

When participants had had engaged in a routine activity, they would need to respond with “yes” when the color indicated to give an honest response and with “no” when the color cue indicated to give an opposite response. When participants had not had engaged in a routine activity, however, they would need to respond with “no” when the color indicated to give an honest response and with “yes” when the color indicated to give an opposite response.

Importantly, participants were to respond differently when confronted with mission questions to give the impression that the false alibi reflected true events. So when confronted with mission questions, participants would always have to lie when the color instructed an honest response, similar to a guilty suspect who would respond to the police. Accordingly, questions about their actually performed activities in the mission (e.g., hiding a piece of paper) would have to be negated when the color instructed an honest response and affirmed when the color instructed an opposite response. In contrast, questions about alibi activities (e.g., sending an e-mail) would need to be affirmed when the color cued an honest response and negated when the color cued an opposite response. Participants were encouraged to remember these instructions well. Afterward they were asked to insert the letter in the box below the table and to go to another room for the inquiry. Accordingly, responses could be obtained that either corresponded or did not correspond with the actual activities of the participants for routine and mission questions (see Figure 1).

Inquiry. Ten routine and 10 mission questions were prepared (see Appendix B). Routine questions were picked carefully to ask about five activities that were very unlikely experienced and five activities that were very likely or even surely experienced (through the participation in the experiment) on that day. Five mission questions asked about the activities that participants engaged in alone in the room and the other five asked about the activities that participants did not engage in. The questions were matched for length: All questions featured five words, and the average number of characters per question was either 25 or 26 for each condition.

Participants sat in front of a 22-inch TFT screen. They saw all questions on the screen, grouped in routine and mission questions, before the inquiry started and were informed that these questions would be presented randomly. Participants responded “yes” and “no” with their index fingers via the keys D and K of a standard QWERTZ keyboard. The assignment of “yes” and “no” responses to the response keys was counterbalanced across participants, but constant for each participant. The font color of the question (yellow vs. blue) indicated whether participants were to respond honestly or to give an opposite response. To be sure that participants understood the meaning of the cues, they were asked, "Did you understand the instructions?" and the font color of this question first cued an honest response and then an opposite response. Only a correct response prompted the next screen. The assignment of cue meaning to the colors yellow and blue was counterbalanced across participants. From this point onward, participants were no longer reminded that they had to respond differently to mission questions, because participants were led to believe that they were on a special mission and that the experimenter did not know about their true activities in the room. Hence, the presentation of error feedback in case of a false response was not possible. To prevent participants from random responding, however, they were told that the computer would monitor whether they responded inconsistently, that is, gave different answers when the same question was presented repeatedly in the same color. Participants were encouraged to respond as fast and accurately as possible.

Each trial of the inquiry started with a centrally presented white fixation cross on black background for 500 ms. In case of a response during fixation, error feedback was provided for 1,500 ms ("Zu früh!"—German for "too early!"). Then a question appeared in yellow or blue font in the center of the screen. The labels for "yes" and "no" (German: "ja" and "nein") were written in white font in the bottom left and right half of the screen as a reminder of the key-response assignment. The question and response labels stayed on screen until a response was given or a time limit of 3,000 ms was exceeded. In the latter case, appropriate error feedback was provided for 1,500 ms ("Zu langsam!"—"too slow!"). The next trial started after 500 ms.

The combination of 2 question types (routine vs. mission; with 10 questions each) $\times$ 2 activity-response correspondence (corresponding vs. noncorresponding) resulted in 40 trial combinations. Participants went through 11 blocks with 40 trials each, where each combination was presented once. Participants could take self-paced breaks in between blocks.

Data treatment and analyses. The first block served as practice and was thus excluded from all statistical analyses, as was the first trial of each block. Error rates were computed as the number of trials in which participants gave a response that was inappropriate for the given combination of question and cue, relative to the number of trials without any other errors (i.e., commission errors plus correct trials). Accordingly, less than 50% correct trials would mean that participants guessed or wrongly memorized the instructions. Eight participants were excluded because they had error rates of 50% or above in at least one of the design cells. Thirty-six participants remained for statistical analyses and we did not replace the removed participants because the sample size was computed based on conservative estimates for possible effect sizes. Trials that used the same question as the trial before were excluded from all statistical analyses to avoid confounds due to retrieval of short-term stimulus–response bindings (2.5%).

Trials in which participants gave an early response during fixation, or did not respond, or responded with any other key than D or K (6.2%), were excluded prior to computing and analyzing error rates. All erroneous trials were excluded before analyzing RTs. Trials with RTs that deviated more than 2.5 *SD*s from the respective cell mean were eliminated as outliers (1.7%).

Error rates and RTs were examined in separate 2 $\times$ 2 analyses of variance (ANOVAs) with the within-subjects factors question type (routine vs. mission) and activity-response correspondence (corresponding vs. noncorresponding). In case of a significant interaction, we used two-tailed paired *t* tests to scrutinize the size of the correspondence effect for each question type.

Results

Error rate. Responses to routine questions were less accurate than responses to mission questions (see Figure 2A), $F(1, 35) = 4.21$, $p = .048$, $\eta_p^2 = .11$. Surprisingly, the main effect of activity-response correspondence was not significant, $F < 1$, whereas the interaction between both factors was significant, $F(1, 35) = 115.08$, $p < .001$, $\eta_p^2 = .77$. Noncorresponding responses to

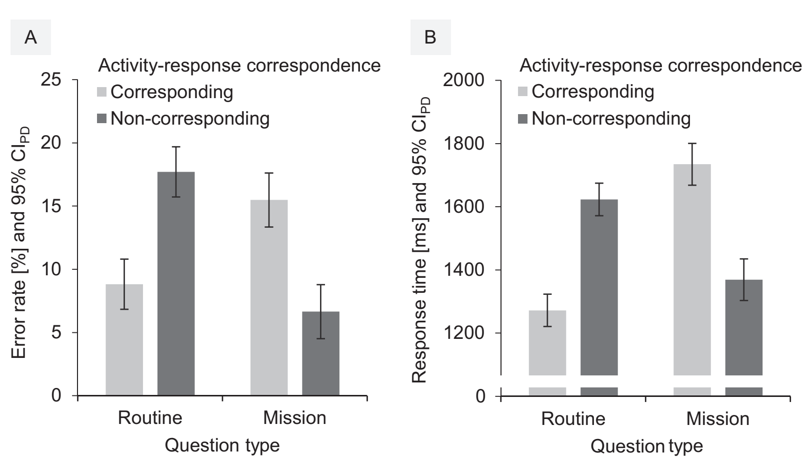

Figure 2. Mean error rates (A) and response times (B) of Experiment 1, plotted as function of activity-response correspondence and question type. Error bars represent the 95% confidence interval of paired differences (CI_{PD}; Pfister & Janczyk, 2013), computed separately for routine and mission questions.

routine questions were more error-prone than corresponding responses to routine questions, $t(35) = 9.10$, $p < .001$, $d = 1.52$, reflecting the hypothesized correspondence effect. A reversed correspondence effect with less accurate corresponding than noncorresponding responses, however, emerged for mission questions, $t(35) = -8.40$, $p < .001$, $d = -1.40$.

Response time. Responses to mission questions were slower than responses to routine questions (see Figure 2B), $F(1, 35) = 45.44$, $p < .001$, $\eta_p^2 = .57$. Again, the main effect of activity-response correspondence was not significant, $F < 1$, whereas the interaction between both factors was significant, $F(1, 35) = 172.81$, $p < .001$, $\eta_p^2 = .83$. Responses to routine questions showed a sizeable correspondence effect, $t(35) = 13.86$, $p < .001$, $d = 2.31$, and a reversed effect was evident for mission questions, $t(35) = -11.24$, $p < .001$, $d = -1.87$.

Discussion

In Experiment 1 we provided participants with a false alibi which entailed to give a false impression about which activities they had performed and which they had not performed in a mission. In a computerized instructed intention paradigm, cues prompted either honest or opposite yes and no responses to routine and mission questions. As participants were instructed to stick to the false alibi, they had to answer mission questions honestly in the presence of the opposite cue and dishonestly in the presence of the honest cue. By contrast, routine questions had to be answered exactly as the cues instructed.

RTs and error rates were analyzed as a function of whether the required response corresponded to what the participants had actually done. Responses to routine questions replicated common findings on dishonesty, as noncorresponding responses took longer and were more error-prone than corresponding responses (e.g., Debey, Verschuere, & Crombez, 2012; Foerster et al., 2016; Spence et al., 2001). This effect was further moderated by the false alibi instruction in mission questions; the extent of this manipulation came unexpected, however: Responses in accordance with the false alibi were, in fact, faster and more accurate than responses based on the participants' actual activities (i.e., activity-response corresponding responses). Hence, it was easier for participants to negate activities they had actually experienced in the mission and affirm activities they had not performed. This pattern of results indicates that participants internalized the false alibi to an extent where the noncorresponding, dishonest response became the default. As such, internalizing false alibis seems to change dishonest responding in a similar way as rehearsal of dishonest responses does (Hu, Chen, et al., 2012; Walczyk et al., 2009, 2012), even though responses in accordance with the false alibi and opposite responses are delivered equally often. Participants might have formed explicit intentions for how to respond to the mission questions. Such explicit intentions have been shown to counter automatic retrieval of spontaneous action tendencies (Waszak, Pfister, & Kiesel, 2013), and might therefore represent a plausible mechanism to explain the observed effects. To follow up on these findings, we examined the impact of false alibis again in a more fine-grained procedure in Experiment 2 that captures not only response initiation but also response execution.

Experiment 2

Without false alibis, a reliable signature of dishonest responding has also been reported in a study that measured continuous movements to capture response initiation and execution (Duran et al., 2010). Participants in this study had to move a mouse cursor from a start area to response labels on the top left and top right of a screen. These movements were initiated later, executed more slowly and their trajectory was more strongly contorted toward the alternative response label when responding dishonestly than when responding honestly. Similar observations were made for rule violations, which also show a continued influence of the rule-based response (e.g., Pfister, Wirth, Schwarz, Steinhauser, & Kunde, 2016; Wirth, Pfister, Foerster, Huestegge, & Kunde, 2016). So for rule violations and lies, there is a conflict between the appropriate response and an automatically activated default response. As such, capturing continuous movements could provide a more detailed picture about the impact of conflicting response tendencies in honest and dishonest responding. In Experiment 2, participants went through the same mission as in the preceding experiment but conducted the inquiry on an iPad to capture continuous finger sweeping movements. This allowed us to study effects of activity-response correspondence and its modulation by false alibis on response initiation and execution.

Method

Participants and overall procedure. A new sample of 44 participants was recruited for either monetary compensation or course credit (age: $M = 26.0$, $SD = 7.14$; 27 female; 41 right-handed). As effects of dishonesty are similarly large for discrete and continuous performance measures, the same sample size as in Experiment 1 was used (cf. Duran et al., 2010). All participants gave written informed consent. The experiment was again divided in two separate parts. The first part of the experiment, that is, the mission, was the same as in Experiment 1 with minor changes in the procedure. The inquiry was conceptually similar but required finger-sweeping responses on an iPad.

Inquiry. The experiment began in the room where the mission took place. First, participants learned how to respond to questions on an iPad. Participants were asked whether they understood the instruction, whether they were sitting on a chair, whether they were awake and whether they were currently lying on a beach. When participants gave a wrong answer to any of these practice questions, the question was repeated until participants gave the correct answer.

Participants used the index finger of their dominant hand to respond to questions. A question appeared in each trial in the center of the screen (see Figure 3; notice that the font color of the practice questions was black and participants had not learned about the upcoming cues yet). When participants touched the starting area at the bottom of the display, the question disappeared and the response labels for "yes" and "no" appeared randomly on the left and right side at the top of the screen. If participants touched the starting area later than 1,500 ms after question onset or left the starting area later than 500 ms after touching it, error feedback was provided in red font in the center of the screen until participants stopped touching the iPad ("Bitte schneller reagieren!"—German for "please respond faster!"). This procedure stressed fast responses to maximize the effects of the independent variables. The next question was presented after 400 ms. After each practice question had been answered correctly, the experimenter left the

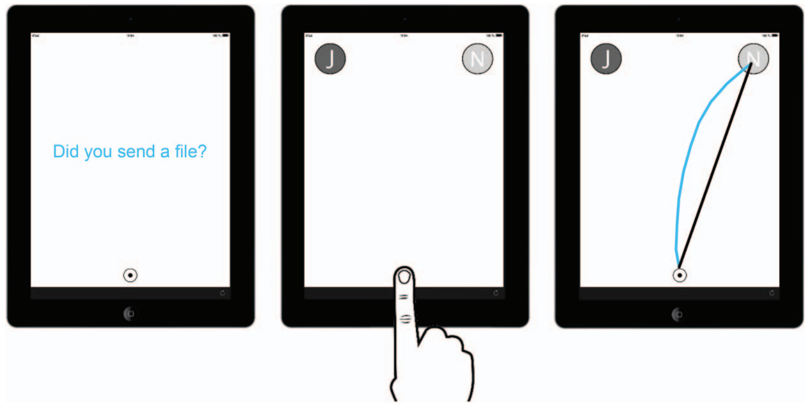

Figure 3. Setup of the inquiry in Experiment 2 (left and center display), and an exemplary movement trajectory (right display). Trials started with a colored question (not drawn to scale to improve legibility). Touching the starting area in the bottom center of the screen made the question disappear whereas the response labels for "yes" ("J"—German "Ja") and "no" ("N"—German "Nein") appeared. The time that passed between question onset and touching the starting area was the reading duration. Initiation time captures the time that passed from touching the starting area until leaving it. The time that passed from that moment until the finger stopped touching the iPad was the movement time. The area under the curve is the area between the actual trajectory (blue [light] line) and a virtual direct line from the start to the end point of the movement (black [dark] line). Areas under the curve were computed after time-normalizing the individual trajectories to 101 data points via linear interpolation. The larger the area under the curve, the stronger was the attraction of the movement toward the alternative, incorrect response area. See the online article for the color version of this figure.

room with the iPad and took it to the inquiry room. The participant stayed and went through the same mission that was used in Experiment 1. Accordingly, the routine and mission questions were the same as in Experiment 1.

In the inquiry, routine and mission questions appeared in yellow and blue font to indicate honest and opposite responding. Participants learned about the honest cue first and responded to each of the routine and mission questions in that color once in a random order. Afterward, the 20 questions were presented again in the color for opposite responding (practice block). In the following 10 blocks, honest and opposite cues appeared in a random sequence. The combination of 2 question types (routine vs. mission; with 10 questions each) × 2 activity-response correspondence (corresponding vs. noncorresponding) × 2 response positions (yes/left and no/right vs. no/left and yes/right) resulted in 80 trial combinations, presented once in each block.

Data treatment and analyses. All practice trials and the first trial of each block were excluded from all statistical analyses. Error rates were computed as in Experiment 1 and six participants were excluded because they had error rates of 50% or more in at least one of the design cells. 38 participants remained for statistical analyses. Trials that used the same question as the trial before were excluded from all statistical analyses as well (3.9%).

Trials in which participants failed to touch the starting area within 1,500 ms after question onset, failed to leave the starting area within 500 ms after touching it, or did not finish their movement in one of the response areas, were excluded prior to computing and analyzing error rates (18.6%; note that this rather high number reflects the emphasis on speeded responding that we sought to stress in this experiment). We selected reading duration, initiation time, movement time, and area under the curve as dependent variables to get a grasp on response initiation (reading duration, initiation time) and execution (movement time, area under the curve; for a detailed description of these variables, see Figure 3). The selection of those four variables was motivated by their high sensitivity to similar experimental manipulations in previous studies (e.g., Pfister et al., 2016; Wirth, Dignath, Pfister, Kunde, & Eder, 2016; Wirth, Pfister, et al., 2016). All error trials were excluded before analyzing those variables. Trials where at least one of the dependent values deviated more than 2.5 *SD*s from the respective cell mean were eliminated as outliers (7.8%).

All dependent variables were examined in separate 2 × 2 ANOVAs with the within-subjects factors question type (routine vs. mission) and activity-response correspondence (corresponding vs. noncorresponding). In case of significant interactions, we used two-tailed paired *t* tests to scrutinize the size of the correspondence effect for each question type.

Results

Error rate. There was a nonsignificant trend toward more accurate responses to mission questions compared to routine questions (see Figure 4A), $F(1, 37) = 3.79$, $p = .059$, $\eta_p^2 = .09$. The main effect of activity-response correspondence was not significant, $F < 1$, whereas the interaction between both factors was significant, $F(1, 37) = 48.51$, $p < .001$, $\eta_p^2 = .57$. Noncorresponding responses were more error-prone than corresponding responses to routine questions, $t(37) = 6.01$, $p < .001$, $d = 0.97$, whereas noncorresponding responses were more accurate than corresponding responses to mission questions, $t(37) = -5.43$, $p < .001$, $d = -0.88$.

Reading duration. Participants spent more time before touching the starting area in mission than in routine trials (see Figure

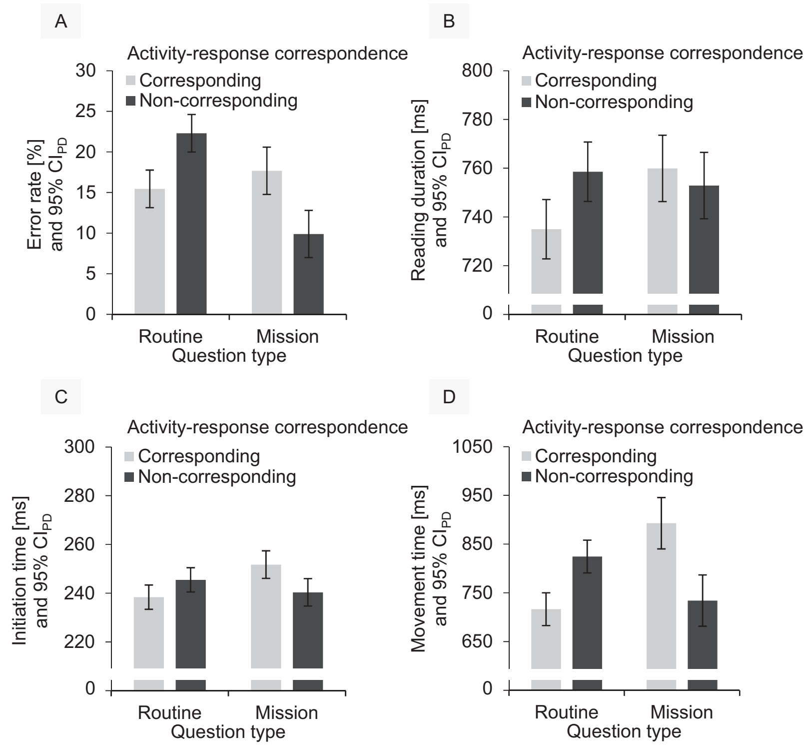

Figure 4. Mean error rates (A), reading durations (B), initiation times (C) and movement times (D) of Experiment 2, plotted as function of activity-response correspondence and question type. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions.

4B), $F(1, 37) = 4.68$, $p = .037$, $\eta_p^2 = .11$. Moreover, noncorresponding trials increased reading durations compared to corresponding trials, $F(1, 37) = 4.17$, $p = .048$, $\eta_p^2 = .10$. A significant interaction qualified the main effects, $F(1, 37) = 9.63$, $p = .004$, $\eta_p^2 = .21$. Whereas noncorresponding trials increased reading durations compared to corresponding trials for routine questions, $t(37) = 3.92$, $p < .001$, $d = 0.64$, there was no effect of correspondence on reading durations for mission questions, $t(37) = -1.05$, $p = .301$, $d = -0.17$.

Initiation time. Participants took longer to leave the starting area when they responded to mission compared to routine questions (see Figure 4C), $F(1, 37) = 5.12$, $p = .030$, $\eta_p^2 = .12$. The main effect of activity-response correspondence was not significant, $F(1, 37) = 2.67$, $p = .111$, $\eta_p^2 = .07$, whereas there was a significant interaction of both factors, $F(1, 37) = 16.54$, $p < .001$, $\eta_p^2 = .31$. Initiation times were faster in corresponding than in noncorresponding trials for routine questions, $t(37) = 2.89$, $p = .006$, $d = 0.47$, and an opposite pattern of results was evident for mission questions, $t(37) = -4.09$, $p < .001$, $d = -0.66$.

Movement time. Movements from the starting area to the correct response area took longer for mission than for routine questions (see Figure 4D), $F(1, 37) = 13.50$, $p = .001$, $\eta_p^2 = .27$, and in corresponding compared to noncorresponding trials, $F(1, 37) = 6.89$, $p = .013$, $\eta_p^2 = .16$. The main effects were qualified by a significant interaction, $F(1, 37) = 46.47$, $p < .001$, $\eta_p^2 = .56$. Noncorresponding trials increased movement times for routine questions compared to corresponding trials, $t(37) = 6.53$, $p < .001$, $d = 1.06$, but an opposite pattern of results emerged for mission questions, $t(37) = -6.10$, $p < .001$, $d = -0.99$.

Area under the curve. The main effects of question type and activity-response correspondence were not significant (see Figure 5), Fs < 1. The interaction of both factors was significant, however, $F(1, 37) = 41.16$, $p < .001$, $\eta_p^2 = .53$. Noncorresponding responses were bent more strongly toward the competing response area than corresponding movements for routine questions, $t(37) = 5.80$, $p < .001$, $d = 0.94$. For mission questions, however, the curve was bent more strongly toward the competing response area in corresponding than in noncorresponding trials, $t(37) = -5.13$, $p < .001$, $d = -0.83$.

Discussion

As in Experiment 1, participants received a false alibi before working on the inquiry during which they had to give a false impression about which activities they did or did not perform. In contrast to Experiment 1, the inquiry took place on an iPad that captured continuous responses to measure markers of response initiation and markers of response execution alike. Cues instructed honest and opposite yes and no responses to routine and mission questions. In accordance with their mission, participants answered mission questions with an honest response in the presence of opposite cues and with a dishonest response in the presence of honest cues while responding exactly as the cues instructed to routine questions.

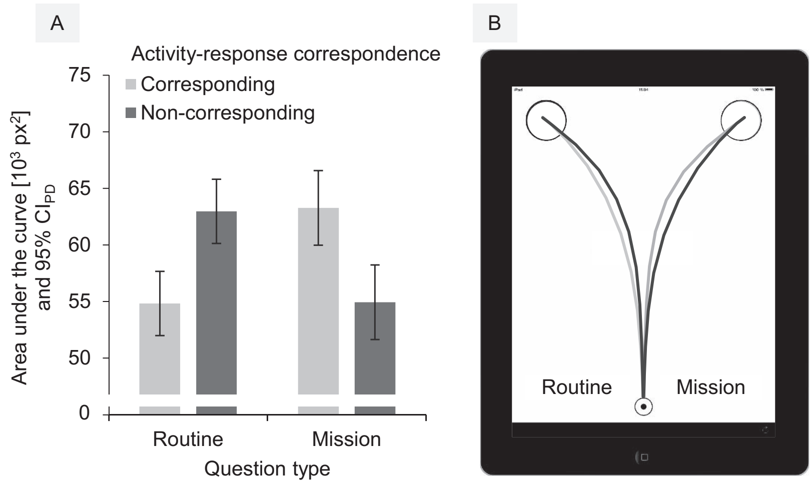

Figure 5. Mean areas under the curve (A) and mean time-normalized movement trajectories (B) of Experiment 2 plotted as function of activity-response correspondence and question type. For simplicity, movement trajectories of routine questions are plotted to the left and mission questions are plotted to the right. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions.

Response initiation and execution replicated the strong impact of false alibis on dishonest responding. Responses to mission questions in accordance with the false alibi (i.e., negation of performed actions and affirmation of not performed actions) were less error-prone, initiated and executed faster, and less attracted by the opposite response label than responses in accordance with participants' actual activities (i.e., negation of not performed actions and affirmation of performed actions). Responses to routine questions again showed the traditional correspondence effect as noncorresponding responses were less accurate, slower initiated and executed, and more strongly bent toward the competing response side compared to corresponding responses (Duran et al., 2010).

Together with the findings of Experiment 1, these results establish false alibis as a strong influence on dishonest processing, altering its behavioral signature substantially. Strikingly, a close look at the statistics reveals that for most dependent variables (error rate and RT of Experiment 1; initiation time and area under the curve of Experiment 2) the interaction was so strong that the reversed correspondence effect in mission questions was just as large as the traditional correspondence effect in routine questions, indicated by a nonsignificant main effect of activity-response correspondence. After establishing that there is a strong impact of false alibis on dishonest responding, the next step is to scrutinize how processing of dishonest responding changes under false alibis in Experiment 3. Finally, Experiment 4 provides a control condition to assess whether the specific design of the mission questions (relative to routine questions) promoted the observed effects.

Experiment 3

The complete reversal of the correspondence effect by explicit false alibis might be taken to suggest that the dishonest rather than honest response to mission questions became activated by default. That is: The observed reversal suggests that alibi-related questions might trigger a dishonest response which would have to be inhibited to respond honestly.

An appealing method to investigate automatic response activation in instructed intention paradigms was recently provided by Debey and colleagues (2014). As explained in the introduction, the authors presented truth or lie distractors (yes and no) with each question (for an illustration in the context of the current experiments, see Figure 6). That is, truth distractors would be "yes" for an affirmative response and "no" for a negation whereas lie distractors would be "no" for an affirmation and "yes" for a negation. They found not only honest but also dishonest responding to be facilitated by honest distractors. For example, if participants were to respond dishonestly with "yes," responses were slower with "yes" distractors than with "no" distractors. In that study, the distractors seemed to have activated a response in the time window of the first process in dishonest responding, namely during honest response activation. As such, honest distractors facilitated this first process while dishonest distractors hampered it because of conflicting response activation.

In Experiment 3, we combined the false alibi manipulation with a computerized inquiry that featured distractors that either did or did not correspond to the participants' actual experiences in the mission. Responses that match the false alibi should again be faster and more accurate compared to responses that match the actual activities. We hypothesized that if false alibis change the response that is activated by default, then noncorresponding instead of corresponding distractors should facilitate responding to mission questions. Responses to routine questions did not come with a false alibi and, thus, should be facilitated with corresponding distractors irrespective of whether participants responded honestly or dishonestly (Debey et al., 2014).

Method

Participants and overall procedure. A sample size of at least 39 participants was required to detect a distractor effect of $d_z = 0.46$ (computed from RT data of Experiment 1 of Debey

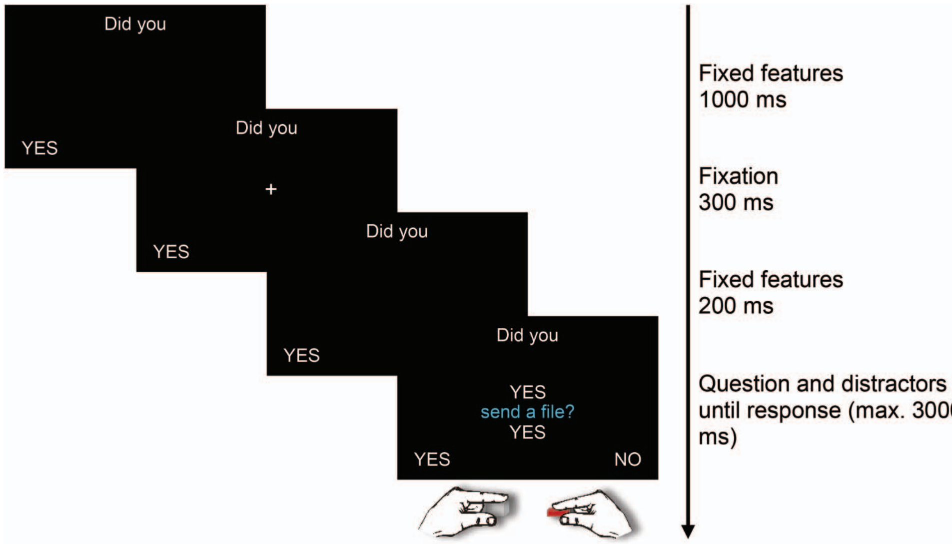

Figure 6. Trial procedure of the inquiry in Experiments 3 and 4. Participants responded to routine and mission questions with yes or no. The font color of the question indicated whether participants should give an honest or an opposite to honest response. Distractors were either yes or no and like the responses, the distractors either did or did not correspond to the participants' actual experiences in the mission. For legibility, question, distractors and response labels are not drawn to scale. See the online article for the color version of this figure.

et al., 2014, Table 1, p. 328) with a power of 80% in a two-tailed test ($\alpha = 5\%$). In the tradition of the former experiment, we opted for a sample of 44 participants (age: $M = 21.6$, $SD = 3.55$; 37 female; 39 right-handed) but decided to replace excluded participants as distractor effects are not as established as general effects of dishonesty, and to allow a fair comparison of Experiment 3 and Experiment 4 based on same sample sizes. As we had to exclude six participants for the same criteria as in the former experiments, a total of 50 participants took part in Experiment 3. All participants gave written informed consent and received course credit for participation. Like in the former experiments, participants went through a mission and an inquiry. All procedures were as in Experiment 1 with the following changes.

Inquiry. We adapted our trial procedure to the design of Debey et al. (2014; see Figure 6). Throughout all experimental trials, the first constant part of each question ("Hast du"—German for "did you") stayed centrally on the top of the screen as did the labels for "yes" and "no" (German: "ja" and "nein") in the bottom left and right of the screen as a reminder of the key-response assignment. The sentence fragment and response labels appeared in white font on black background. Each trial of the inquiry started with the presentation of these fixed features and after 1000 ms a white fixation cross appeared additionally in the center of the screen for 200 ms. After the offset of the fixation cross, the fixed features stayed on screen for 300 ms. In case of an early response before question onset, an error feedback was provided for 1,500 ms ("Zu früh!"—German for "too early!"). The fixed features were accompanied by a question and distractors. The distractors were either "yes" or "no" (German: "ja" and "nein") and occurred in a distance of 5% above and below the question, respectively (percentages refer to the vertical coordinate on the computer screen). The position of the question and its distractors was determined randomly in each trial. They appeared centrally on one of four positions on the vertical axis (33%, 43%, 57% and 67%). The font color of the distractors was white whereas yellow and blue font was again used for the central question fragment. The question and distractors stayed on screen until a response was given or a time limit of 3,000 ms was exceeded. In the latter case an error feedback was provided for 1,500 ms ("Zu langsam!"—German for "too slow!").

The combination of 2 question types (routine vs. mission; with 10 questions each) × 2 activity-response correspondence (corresponding vs. noncorresponding) × 2 activity-distractor correspondence (corresponding vs. noncorresponding) resulted in 80 trial types. Participants therefore went through nine blocks with 80 trials each, during which each combination was presented once. Participants could take self-paced breaks in between blocks.

Data treatment and analyses. The first block served as practice and was thus excluded from all statistical analyses, as was the first trial of each block. Trials that entailed question repetitions were excluded (3.8%). Trials in which participants gave an early response during fixation, did not respond, or responded with any other key than D or K (2.3%), were excluded prior to computing and analyzing error rates. All erroneous trials were excluded before analyzing RTs, as were outliers (2.0%).

Error rates and RTs were examined in two separate 2 × 2 × 2 ANOVAs with the within-subjects factors question type (routine vs. mission), activity-response correspondence (corresponding vs. noncorresponding) and activity-distractor correspondence (corresponding vs. noncorresponding). In case of significant three-way and two-way interactions, we conducted separate 2 × 2 ANOVAs and two-tailed paired t tests, respectively. Appendix C shows the mean error rates and RTs, computed separately for each combination of the three factors.

Results

Error rate. The main effects of question type and activity-response correspondence were not significant (see Figure 7), *F*s < 1, but the two-way interaction of both factors was significant, $F(1, 43) = 51.67$, $p < .001$, $\eta_p^2 = .55$. Noncorresponding responses were more error-prone than corresponding responses to routine questions, $t(43) = 6.16$, $p < .001$, $d = 0.93$, but a reversed correspondence effect emerged for mission questions, $t(43) = -6.05$, $p < .001$, $d = -0.91$. The main effect of activity-distractor correspondence was not significant, $F(1, 43) = 3.23$, $p = .080$, $\eta_p^2 = .07$, however, the two-way interaction of activity-distractor correspondence and question type was significant, $F(1, 43) = 12.42$, $p = .001$, $\eta_p^2 = .22$, as responding was more accurate with corresponding than with noncorresponding distractors for routine questions, $t(43) = 3.93$, $p < .001$, $d = 0.59$, whereas distractors had no effect on mission questions, $t(43) = -1.63$, $p = .111$, $d = -0.25$. None of the remaining interactions were significant, *F*s < 1.

Response time. Responses to mission questions were slower than responses to routine questions (see Figure 8), $F(1, 43) = 44.13$, $p < .001$, $\eta_p^2 = .51$. The main effect of activity-response correspondence was not significant, $F < 1$, but the two-way interaction between activity-response correspondence and question type was significant, $F(1, 43) = 186.39$, $p < .001$, $\eta_p^2 = .81$, as responses to routine questions showed a typical correspondence effect, $t(43) = 13.19$, $p < .001$, $d = 1.99$, and a reversed correspondence effect was evident for mission questions, $t(43) = -12.11$, $p < .001$, $d = -1.83$. The main effect of activity-distractor correspondence was significant, $F(1, 43) = 11.20$, $p = .002$, $\eta_p^2 = .21$, but was further qualified by question type as mirrored in a significant two-way interaction of both factors, $F(1, 43) = 8.26$, $p = .006$, $\eta_p^2 = .16$. Responding to routine questions was easier with corresponding than with noncorresponding distractors, $t(43) = 4.94$, $p < .001$, $d = 0.74$, but distractors did not affect responding to mission questions, $t(43) = 0.74$, $p = .465$, $d = 0.11$. None of the remaining interactions were significant, *F*s < 1.

Discussion

Experiment 3 implemented corresponding and noncorresponding distractors in an inquiry with responses that did or did not correspond with participants' actual experiences to examine the underlying processes of dishonest responding with (mission questions) and without (routine questions) a false alibi. Replicating the results of the preceding experiments, noncorresponding responses to mission questions were delivered more easily than corresponding responses with a false alibi. In line with the literature, a traditional intention effect was again found in absence of a false alibi (e.g., Debey et al., 2012; Foerster et al., 2016; Spence et al., 2001). Distractor effects in routine questions also corroborated previous observations as distractors that corresponded with actual experiences facilitated responding in comparison to noncorresponding distractors (Debey et al., 2014). However, distractors did not affect responding to mission questions where participants had a false alibi.

This pattern of results suggests that false alibis have a potent impact on the automatic retrieval of responses as the usually observed distractor effect vanishes. The absence of any distractor effects despite of large reversed activity-response correspondence effects indicates that false alibis do not simply substitute the noncorresponding response as an automatic default. A thorough discussion of this finding and potential theoretical explanations is postponed to the General Discussion, to establish with the following experiment whether the impact of false alibis on response and distractor correspondence effects do not stem from potential artifacts of the mission procedure.

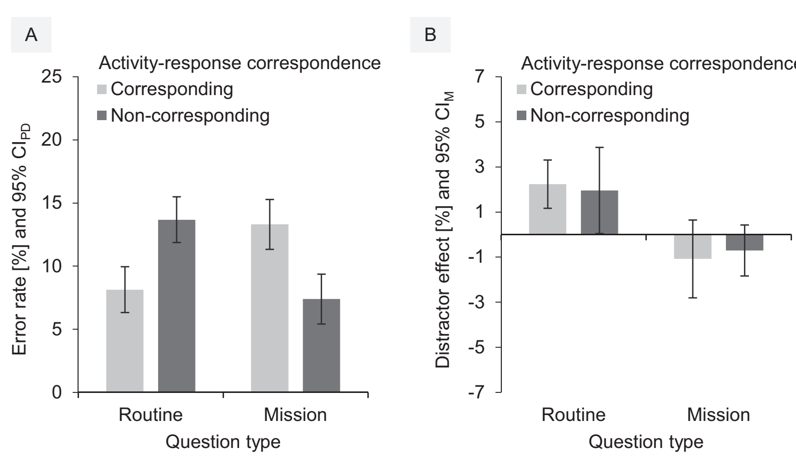

Figure 7. Results of the analysis of error rates in Experiment 3. Mean error rates (A) and mean activity-distractor correspondence effects on error rate (B) are plotted as function of activity-response correspondence and question type. Error rates of noncorresponding distractors were subtracted from error rates of corresponding distractors to compute distractor effects. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions (A) whereas error bars around distractor effects show the 95% confidence interval around each of the four means (CI_M; B).

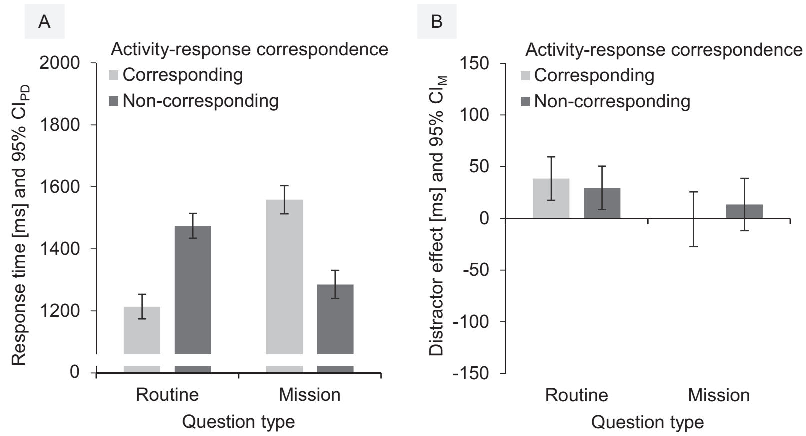

Figure 8. Response time results of Experiment 3. Mean response times (A) and mean activity-distractor correspondence effects on response time (B) are plotted as function of activity-response correspondence and question type. Response times of noncorresponding distractors were subtracted from response times of corresponding distractors to compute distractor effects. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions (A) whereas error bars around distractor effects show the 95% confidence interval around each of the four means (CI_M; B).

Experiment 4

In Experiment 4, we used the same mission and inquiry as in the former experiments. Participants also still learned about the actions they were not to perform but, crucially, they were asked to admit which actions they actually did and did not perform in the inquiry. Accordingly, Experiment 4 aimed at establishing (a) that the observed reversed activity-response correspondence effects of the mission questions of the previous experiments were not an artifact of specific experimental parameters, and (b) whether facilitating effects of corresponding distractors would emerge for mission questions without a false alibi. We hypothesized that without a false alibi, corresponding responses should be delivered faster and with fewer errors than noncorresponding responses in routine *and* mission questions. Likewise, corresponding compared to noncorresponding distractors should also facilitate responding for both question types.

Method

Participants and overall procedure. Forty-four participants (age: $M = 21.6$, $SD = 3.55$; 37 female; 39 right-handed) of a sample size of 48 could be considered for statistical analyses. The sample size was based on the same criteria as Experiments 3, and four participants were excluded for the same criteria as in the former experiments. All participants gave written informed consent and received course credit as compensation. The procedure of Experiment 4 was almost the same as in Experiment 3, except that it featured slightly different instructions in the mission (see Appendix A). Participants went through the same actions of the mission as the former participants. They were told, however, that most participants did five other actions and that these participants had to conceal which action they engaged in. In contrast, their task was to accurately report in the upcoming inquiry which action they had performed in the mission.

Data treatment and analyses. The first block served as practice and was thus excluded from all statistical analyses as was the first trial of each block. Trials that entailed question repetitions were excluded (3.9%). Trials in which participants gave an early response during fixation, did not respond, or responded with any other key than D or K (2.2%), were excluded prior to computing and analyzing error rates. All erroneous trials were excluded before analyzing RTs. Trials with RTs that deviated more than 2.5 *SD*s from the respective cell mean were eliminated as outliers (2.1%).

Error rates and RTs were examined in two separate $2 \times 2 \times 2$ ANOVAs with the within-subjects factors question type (routine vs. mission), activity-response correspondence (corresponding vs. noncorresponding) and activity-distractor correspondence (corresponding vs. noncorresponding). In case of significant three-way and two-way interactions, we conducted separate 2×2 ANOVAs and two-tailed paired t tests, respectively. Appendix C shows the mean error rates and RTs, computed separately for each combination of the three factors.

Results

Error rate. Errors were more frequent in routine questions than in mission questions (see Figure 9), $F(1, 43) = 10.86$, $p = .002$, $\eta_p^2 = .20$. Noncorresponding responses were more error-prone than corresponding responses, $F(1, 43) = 66.25$, $p < .001$, $\eta_p^2 = .61$. The two-way interaction between activity-response correspondence and question type was significant, $F(1, 43) = 9.33$, $p = .004$, $\eta_p^2 = .18$. Noncorresponding responses were less accurate than corresponding responses for both question types but the effect was larger for mission questions, $t(43) = 7.53$, $p < .001$, $d = 1.14$, than for routine questions, $t(43) = 5.68$, $p < .001$, $d = 0.86$. More errors were made with noncorresponding than with corresponding distractors, $F(1, 43) = 16.08$, $p < .001$, $\eta_p^2 = .27$. The two-way interaction of activity-response correspondence and activity-distractor correspondence was significant, $F(1, 43) = 4.14$, $p = .048$, $\eta_p^2 = .09$, as the activity-distractor correspondence effect was stronger when activity and response did not correspond, $t(43) = 3.70$, $p = .001$, $d = 0.56$, than when they corresponded,

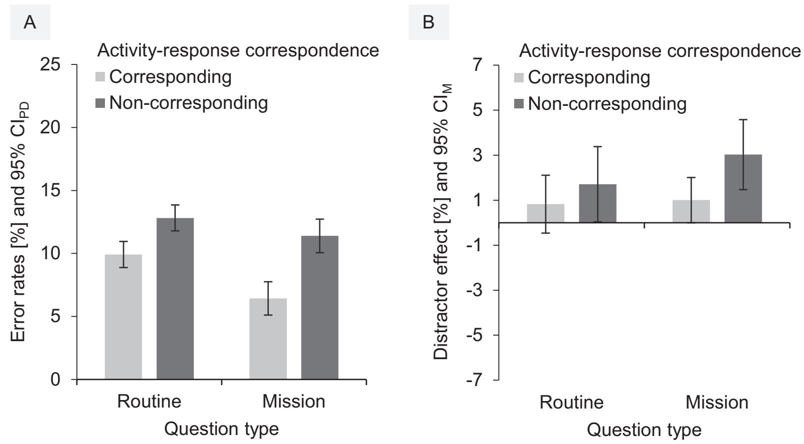

Figure 9. Results of the analysis of error rates in Experiment 4. Mean error rates (A) and mean activity-distractor correspondence effects on error rate (B) are plotted as function of activity-response correspondence and question type. Error rates of noncorresponding distractors were subtracted from error rates of corresponding distractors to compute distractor effects. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions (A) whereas error bars around distractor effects show the 95% confidence interval around each of the four means (CI_M; B).

$t(43) = 2.16$, $p = .036$, $d = 0.33$. None of the remaining interactions were significant, $Fs < 1.16$, $ps \geq .234$.

Response time. Responses to mission questions were slower than responses to routine questions (see Figure 10), $F(1, 43) = 27.15$, $p < .001$, $\eta^2_p = .39$. Noncorresponding responses were slower than corresponding responses, $F(1, 43) = 142.42$, $p < .001$, $\eta^2_p = .77$. This effect was larger in mission questions, $t(43) = 12.95$, $p < .001$, $d = 1.95$, than in routine questions, $t(43) = 9.71$, $p < .001$, $d = 1.46$, as indicated by a significant two-way interaction between activity-response correspondence and question type, $F(1, 43) = 5.51$, $p = .024$, $\eta^2_p = .11$. Responses were slower when activity and distractor did not correspond relative to when they corresponded, $F(1, 43) = 12.08$, $p = .001$, $\eta^2_p = .22$. None of the remaining interactions were significant, $Fs \leq 1.03$, $ps \geq .315$.

Discussion

In Experiment 4, participants performed actions in a mission and learned about other actions that were allegedly performed by most of the other participants. In an upcoming inquiry, they were to admit which actions they actually did and did not perform in the mission (mission questions) and on that day (routine questions) by responding honestly and oppositely. In line with our hypotheses and previous findings in the literature, responding in correspon-

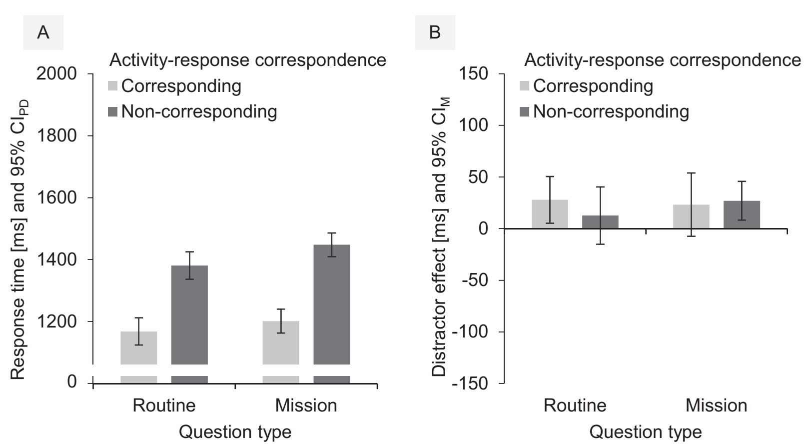

Figure 10. Response time results of Experiment 4. Mean response times (A) and mean activity-distractor correspondence effects on response time (B) are plotted as function of activity-response correspondence and question type. Response times of noncorresponding distractors were subtracted from response times of corresponding distractors to compute distractor effects. Error bars represent the 95% confidence interval of paired differences (CI_{PD}), computed separately for routine and mission questions (A) whereas error bars around distractor effects show the 95% confidence interval around each of the four means (CI_M; B).

dence with actual experiences was easier than responding noncorrespondingly in routine and mission questions (e.g., Debey et al., 2012; Foerster et al., 2016; Spence et al., 2001). Furthermore, corresponding distractors facilitated corresponding and noncorresponding responses (Debey et al., 2014). This suggests that mission questions activated their honest response which had to be overcome to respond dishonestly and that this process could be captured by competing response activation of distractors.

The former experiments featured opposite but in most cases equally sized activity-response correspondence effects in routine and mission questions. Potential conclusion from this pattern might have been confounded by the fact that mission questions were always accompanied by a false alibi whereas routine questions were not. Experiment 4 revealed that without a false alibi, the effect of more difficult responses in noncorresponding than in corresponding trials was even larger in mission than in routine questions. This is true for speed and accuracy alike. Accordingly, the reversing effect of false alibis appears all the more striking.

General Discussion

In the present experiments, participants were led to believe that they had to fulfill a special mission that entailed to lie in an upcoming inquiry. The inquiry featured an instructed intention paradigm on a personal computer (Experiments 1, 3, and 4) or on an iPad (Experiment 2). Cues signaled participants to respond honestly or oppositely with yes and no to routine questions and to questions regarding their mission. To fulfill their special mission, participants were given a false alibi that specified a series of alternative actions and they were to answer according to this alibi in the inquiry. That is, participants had to lie in the presence of honest cues and had to be honest in the presence of opposite cues when responding to mission questions in Experiments 1−3. In Experiment 4, participants were not provided with a false alibi but they simply were to tell the truth in the presence of honest cues and had to be dishonest in the presence of opposite cues when responding to mission questions to control for potential confounding effects from different questions. Distractors were used in Experiments 3 and 4 to scrutinize how false alibis change dishonest processing.

The Power of False Alibis

Responses to routine questions in all experiments and to mission questions in Experiment 4 replicated common findings on dishonesty in the literature, as response initiation and execution took longer and were more error-prone when actual experiences and responses did not correspond as compared to corresponding responses (e.g., Debey et al., 2012; Foerster et al., 2016; Spence et al., 2001). Experiment 2 also revealed an impact of the corresponding response on movement trajectories when giving a noncorresponding response. Movement trajectories were more strongly attracted toward the competing response side in noncorresponding trials than in corresponding trials for routine questions, which replicates and extends previous observations from studies on action dynamics during lying (Duran et al., 2010) and rule violations (Pfister et al., 2016; Wirth, Pfister et al., 2016). Furthermore, distractors that corresponded with actual experiences facilitated honest and dishonest responding to routine questions as compared to noncorresponding distractors, again replicating existing evidence in the literature (Debey et al., 2014). The results of the mission questions from Experiments 1−3, however, came unexpected: Responses in accordance with the false alibi were, in fact, faster and more accurate than responses according to the participants' actual experience during the mission (i.e., corresponding responses). In particular, it was easier for participants to negate than to affirm activities they had actually experienced in the mission and it was easier to affirm than to negate activities they had not performed. The correspondence effect was also reversed in movement trajectories with a stronger attraction toward the competing response in corresponding trials.

These counterintuitive observations of faster and more accurate noncorresponding responses relative to corresponding responses suggest that false alibis change the typical honest default response to a dishonest default response. As such, these dishonest responses would be retrieved directly and automatically. However, this assumption predicts faster and more accurate responding with noncorresponding distractors when participants received a false alibi, and this prediction was not confirmed in Experiment 3.

Three theoretical possibilities suggest themselves to account for this result. First, participants might have succeeded in building a vivid mental model of the alibi actions, resulting in a strong representation of those actions (for a primer on mental models, see, e.g., Johnson-Laird, 2004). However, retrieval of the appropriate mental model of such actions could be effortful and time-consuming compared to honest response retrieval. Accordingly, false alibis could actually substitute the honest default response with the dishonest default response, but the retrieval of that dishonest default would take sufficient time for any distractor effects to level off due to a mandatory built-up of the mental model. Second, false alibis could implement an automatic inhibition of the corresponding response rather than an activation of the noncorresponding response. The sequence of initial activation and automatic inhibition would impair performance for corresponding responses whereas it does not necessarily affect noncorresponding responses. Third, noncorresponding responses might be consistently activated in addition to the proposed automatic inhibition of the honest response on each trial. All three proposed mechanisms would produce reversed correspondence effects without necessarily showing effects of distractors. Absent distractor effects are in line with all three accounts by assuming that distractors were processed while the proposed automatic steps were still at work. Varying the temporal relation of question and distractors could give insight into whether the default response changed to the noncorresponding response (with a more effortful retrieval than for honest default responses) or corresponding responses were automatically inhibited and in the latter case, whether noncorresponding responses activation follows this inhibition process. Conceivably, dishonest processing could also operate less consistently with false alibis than without false alibis and entail a mixture of these mechanisms, thus, failing to show consistent distractor effects.

In any case, the present results raise the question of how basic the cognitive basis of dishonesty, as described in the introduction, really is. To recapitulate, contemporary models of the cognitive processes underlying dishonesty assume that the honest response to a question is retrieved automatically. When a question is always answered dishonestly, a stimulus-response association is built up that can be easily derived and makes lying as easy as being honest (Walczyk et al., 2009, 2012). An equal proportion of honest and dishonest responding to each question prevents such a creation of stimulus–response associations, like in the current experiments, where corresponding and noncorresponding responses differed in several behavioral measures. The present evidence suggests that false alibis change the automatically retrieved response and/or render the inhibition of honest default responses more efficient, thus, facilitating dishonest responding and interfering with honest responding.

In the current experiments, participants committed a considerable amount of errors which also led to the exclusion of several participants. Instructed lie paradigms without false alibi instructions already come with rather high error rates and participants frequently express that the task is difficult (cf., e.g., Debey et al., 2014; Foerster et al., 2016). The applied character of our task did not allow giving error feedback in case of wrong response commissions. Accordingly, participants with difficulty to understand the inquiry task could not be corrected during the inquiry. Many of the excluded participants failed to meet our predefined inclusion criteria by a considerable margin with error rates of at least 70% in one of the experimental cells of the mission questions. So, these participants responded consistently wrong and did not follow the alibi instruction but responded similarly to routine and mission questions.

One way to minimize data exclusion in future studies could be the use of a more accessible cover story, possibly combined with rewards to enhance motivation. This could give insight into whether false alibis are implemented more easily and successfully with higher motivation or whether false alibis always impose considerable difficulty. A challenge for researchers will be to establish a level of motivation in the laboratory that is comparable to the motivation of alleged criminals.

Implications for Lie Detection

The strong effects of false alibis cast doubt on whether cognitive tests may represent a feasible tool for lie detection, because of the clear-cut correspondence effects (standard or inverted) on the group level.[1] The present results further provide more insight for the conclusions of a similar, real-world investigation with a convicted woman (Spence, Kaylor-Hughes, Brook, Lankappa, & Wilkinson, 2008). The woman was already convicted for poisoning her child when she took part in the inquiry of the researchers. The authors created an instructed intention paradigm with questions relating to her case. According to a mapping rule, the woman responded to each question honestly, that is, analogously to her account of the events, or dishonestly, that is, analogously to the accusers' account of events. Enhanced RTs and hemodynamical activity in ventrolateral prefrontal and anterior cingulate regions emerged for responses in accordance with the accusers' account of the events. Based on the evidence in the literature that existed by then, the data supported the assumption that the woman was innocent of the crime. The present data shows, however, that a dishonest response can be well internalized and generated more promptly than an honest response. Hence, the instructed intention paradigm appears to be impractical for lie detection because innocent and guilty persons might produce the same pattern of results.

If instructed intention paradigms do not seem to have particular utility for lie detection—is there an alternative approach to lie detection using RTs or similar behavioral parameters? One solution that has recently been proposed capitalizes on congruency effects as measured via the autobiographical implicit association test (Sartori et al., 2008). In this procedure, participants usually indicate with button presses whether sentences describe innocence (e.g., "I bought the CD") or guilt (e.g., "I stole the CD") or whether sentences are true (e.g., "I am reading a scientific manuscript") or false (e.g., "I am swimming in the Red Sea"). Participants have to categorize sentences of all four categories in a random sequence in each block. Crucially, in one block innocence and truth share one response key and guilt and falsity share the other response key whereas in a second block guilt and truth, and innocence and falsity share response keys, respectively. Innocent participants who did not steal a CD would respond faster in the first than in the second block whereas the opposite would be true for guilty participants (e.g., Agosta, Ghirardi, Zogmaister, Castiello, & Sartori, 2011).

Though there are indeed several promising reports, the validity and robustness of this measure is still under discussion. Countermeasures were identified that diminish the detection accuracy of the autobiographical implicit association test as the instruction of speeded responses and training in those blocks where innocent persons respond faster than guilty persons (Hu, Rosenfeld, & Bodenhausen, 2012), and instructions to slow down responses in blocks were innocent persons respond slower than guilty persons (Verschuere, Prati, & Houwer, 2009). The results of the present study suggest false alibis as a potential countermeasure by representing performed actions strongly as being not performed and not-performed actions as being performed.

Another lie detection method, the concealed information test, relies on the fact that a crime stimulus, among more frequent but comparable neutral stimuli, is significant for the person who committed the crime but indistinguishable for innocent persons (e.g., Ben-Shakhar & Elaad, 2003). Accordingly, the crime stimulus produces detectable signatures because of significance only in guilty persons. False alibi stimuli could produce similar effects as crime stimuli in the concealed information test. As such, false alibis might be identified as true knowledge of the examined person. However, it seems less plausible that crime stimuli become inseparable of neutral stimuli by imagining to not have interacted with those crime stimuli.

Conclusion

The current study adapted the instructed intention paradigm to observe alibi effects on lying performance in a forensically applicable design. Participants had to implement a false alibi by pretending to have engaged in plausible alibi actions and denying the involvement in actually experienced activities. They succeeded to a level where the fake story appeared as being true in all relevant measures. The data suggest that mere instruction can cause either dishonesty instead of honesty to become the default response (but a weaker default than the honest one) or dishonest processing to

[1] Large effects on the group level (like the correspondence effects here) are a necessary precondition for successful lie detection, but classification accuracy on a participant- and item-specific level has to be assessed to evaluate how a method is actually suited for lie detection (Franz & von Luxburg, 2015).

become the default in the sense that a question still triggers an honest response which is automatically inhibited, thus, facilitating (automatic) dishonest response retrieval. These mechanisms could also operate simultaneously. In all cases, honest responding would be more effortful—contrary to the usually assumed cognitive processes operating during honest and dishonest responding.

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Appendix A

English Translation of the Mission Instructions

[Front page of all experiments]

Dear participant,

Please engage in all of the following activities in the given sequence. To ensure that the experiment can be completed successfully, it is important that you do not share with the experimenter which activities you engaged in.

Please engage in the following activities:

1. Sit down at the desk.
2. Take a sheet of paper and the pen from the box on the desk.
3. Draw a triangle and a circle on this sheet of paper.
4. Put the pen back in the box on the table.
5. Tear the sheet of paper in half.
6. Hide one piece of the sheet of paper **under** the stack of paper in the box on the desk and the other one in the box **below** the desk.

Please turn the page when you have engaged in all of the activities!

[Back page of Experiment 1–3]

Thank you for performing all activities. In a moment you are going to be questioned about these activities in a computerized inquiry. The majority of participants had to engage in different activities and had to be honest about them in the following inquiry. In contrast, you are on a special mission and have to hide your true activities you just engaged in. That helps us to learn more about lie detection.

Accordingly, you are going to deny that you used the sheet of paper, the pen and the boxes. Instead you are going to pretend that you engaged in other activities. Hence, you need an alibi. Your alibi activities are those that the majority of participants experienced. You are going to pretend that you

- switched on the computer,
- used the USB stick,
- opened a file called “table,”
- wrote an e-mail,
- and sent the file via e-mail.

(*Appendices continue*)

Please do not engage in any of these activities!

In the inquiry, you are going to be asked about activities you could or could not have engaged in today (**routine questions**) and about your secret activities in this room (**mission questions**). In addition, the color of the questions is going to indicate how you have to respond: either **honestly** or **oppositely to that honest response**.

The following is very important: You are going to follow these instructions exactly as told when you respond to **routine questions**. When you have engaged in the activity today, you respond with "yes" when you are to respond honestly and with "no" when you are to respond oppositely. When you have not engaged in the activity today, you respond with "no" when you are to respond honestly and with "yes" when you are to respond oppositely.

When you encounter a **mission question**, however, you always have to lie when the color indicates to be honest, similar as a criminal would respond to the police. Accordingly, you always need to pretend that you have engaged in the alibi activities and have not used the pen, paper and box. Example: When you are asked whether you wrote an e-mail, you respond with "yes" when you are to respond honestly and with "no" when you are to respond oppositely. When you are asked whether you hid a sheet of paper, you respond with "no" when you are to respond honestly and with "yes" when you are to respond oppositely.

Please take your time to memorize these instructions and the activities of your alibi as you will need this information shortly. When you are ready, insert this letter in the box under the table. Then go to room H9 for the inquiry. Do not talk about your activities to the experimenter.

[Back page of Experiment 4]

Thank you for performing all activities. In a moment you are going to be questioned about these activities in a computerized inquiry. Most other participants had to engage in different activities and were not allowed to reveal them in the following inquiry. In contrast, you are on a special mission and have to admit your true activities you just engaged in. That helps us to learn more about lie detection.

Accordingly, you are going to admit that you used the sheet of paper, the pen and the boxes. The majority of participants engaged in the following activities. They

- switched on the computer,
- used the USB stick,
- opened a file called "table,"
- wrote an e-mail,
- and sent the file via e-mail.

Please do not engage in any of these activities!

In the inquiry, you are going to be asked about activities you could or could not have engaged in today (**routine questions**) and about your secret activities in this room (**mission questions**). In addition, the color of the questions is going to indicate how you have to respond: either **honestly** or **oppositely to that honest response.**

When you respond to **routine questions** and you have engaged in the activity today, you respond with "yes" when you are to respond honestly and with "no" when you are to respond oppositely. When you have not engaged in the activity today, you respond with "no" when you are to respond honestly and with "yes" when you are to respond oppositely.

The same applies to **mission questions**. Example: When you are asked whether you wrote an e-mail, you respond with "no" when you are to respond honestly and with "yes" when you are to respond oppositely. When you are asked whether you hid a sheet of paper, you respond with "yes" when you are to respond honestly and with "no" when you are to respond oppositely.

Please take your time to memorize these instructions and the activities of your alibi as you will need this information shortly. When you are ready, insert this letter in the box under the table. Then go to room H9 for the inquiry. Do not talk about your activities to the experimenter.

(*Appendices continue*)

Appendix B

English Translations [and German Original] of Routine and Mission Questions

Activity status	Routine questions	Mission questions
Experienced	Did you cross a street?	Did you draw a triangle?
	[Hast du eine Straße überquert?]	[Hast du ein Dreieck gezeichnet?]
	Did you talk to somebody?	Did you rip a sheet?
	[Hast du mit jemandem gesprochen?]	[Hast du ein Blatt zerrissen?]
	Did you walk through a door?	Did you open a box?
	[Hast du eine Tür durchquert?]	[Hast du eine Box geöffnet?]
	Did you sign a document?	Did you hide a piece of paper?
	[Hast du ein Dokument unterzeichnet?]	[Hast du ein Papierstück versteckt?]
	Did you put your shoes on?	Did you draw a circle?
	[Hast du dir Schuhe angezogen?]	[Hast du einen Kreis gezeichnet?]
Not experienced	Did you pet a camel?	Did you write an email?
	[Hast du ein Kamel gestreichelt?]	[Hast du eine Email verfasst?]
	Did you win the lottery?	Did you send a file?
	[Hast du im Lottospiel gewonnen?]	[Hast du eine Datei gesendet?]
	Did you destroy a window?	Did you open a table?
	[Hast du ein Fenster zerstört?]	[Hast du eine Tabelle geöffnet?]
	Did you call the police?	Did you use a USB stick?
	[Hast du die Polizei angerufen?]	[Hast du den USB-Stick benutzt?]
	Did you pick mushrooms?	Did you turn on the PC?
	[Hast du einen Pilz gesammelt?]	[Hast du den PC eingeschaltet?]

Note. The first five routine questions concern activities that were likely experienced on that day and the other five activities of the routine questions were very unlikely to be experienced on a common day. The top five questions of the mission type asked about the activities participants were instructed to do in the mission. The last five questions concern the alibi that participants got but were asked not to engage in (Exp. 1–3) or were told about after their mission (Exp. 4).

Appendix C

Mean Error Rates, Response Times (RTs), and Their Respective Standard Deviations (*SD*s) for Each Combination of Question Type, Activity-Response Correspondence, and Activity-Distractor Correspondence of Experiments 3 and 4

Experiment	Question type	Activity-response correspondence	Activity-distractor correspondence	Error rate	RT
Exp.3	Routine	Corresponding	Corresponding	7.0 (7.63)	1195 (249)
			Noncorresponding	9.3 (8.39)	1233 (254)
		Noncorresponding	Corresponding	12.7 (11.23)	1460 (282)
			Noncorresponding	14.7 (11.48)	1489 (303)
	Mission	Corresponding	Corresponding	13.8 (12.74)	1559 (311)
			Noncorresponding	12.8 (12.34)	1558 (333)
		Noncorresponding	Corresponding	7.7 (9.20)	1279 (256)
			Noncorresponding	7.0 (8.34)	1292 (266)
Exp.4	Routine	Corresponding	Corresponding	9.5 (9.81)	1154 (231)
			Noncorresponding	10.3 (9.20)	1182 (235)
		Noncorresponding	Corresponding	12.0 (9.58)	1375 (284)
			Noncorresponding	13.7 (10.12)	1387 (289)
	Mission	Corresponding	Corresponding	5.9 (8.15)	1190 (243)
			Noncorresponding	6.9 (8.53)	1213 (262)
		Noncorresponding	Corresponding	9.9 (8.27)	1434 (292)
			Noncorresponding	12.9 (9.89)	1461 (286)

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