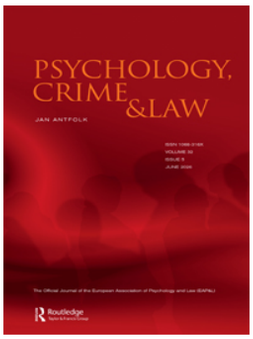

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Shedding light on deception: exploring an attribution approach

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ABSTRACT

Deception can produce cognitive dissonance (Festinger, 1962). We explored whether it was possible to leverage classic research on attributions of arousal to improve deception detection. In Study 1, targets voluntarily lied or told the truth about a transgression under one of three conditions: the room's lighting reportedly induced arousal (arousal attribution), induced relaxation (relaxation attribution), or was not mentioned (control). Study 2 featured similar attribution conditions; however, targets were explicitly instructed to lie or tell the truth about a witnessed event (i.e. behavioral choice was eliminated). As predicted, observers' deception detection improved in the arousal and relaxation attribution conditions (vs. control) only when targets' behaviors were volitional. This work provides preliminary evidence that misattributions of arousal might affect deception under high-choice conditions. In turn, attribution manipulation is a promising theory-based approach to improving deception detection that merits further investigation.

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Deception detection; arousal; attribution; misattribution; interviewing

Most people have difficulty detecting deceit (Aamodt & Custer, 2006; Bond & DePaulo, 2006). Recent research has explored cognitive approaches to improving deception detection, which involve creating an interviewing context that will differentially affect lie- and truth-tellers (Vrij et al., 2017; but see Levine et al., 2018). Several theories have postulated that lie-tellers must engage in more cognitively demanding tasks than truth-tellers, such as creating and remembering the fabrication, concealing cues to deception, and monitoring others for signs of disbelief (Sporer, 2004; Vrij et al., 2011; Zuckerman et al., 1981; but see Morrison et al., 2020). Accordingly, increasing the cognitive complexity of an investigative interview should disproportionately hinder lie-tellers. For instance, having lie-tellers recount events in reverse chronological order or maintain eye contact can facilitate deception detection (Vrij et al., 2008; Vrij et al., 2010; but see Levine et al., 2018). This research suggests that manipulating cognitive load can affect the ability to deceive. We speculated that exploiting other cognitive processes, beyond cognitive load, might yield similar effects. In two proof-of-concept studies, we applied well-established work from the attribution literature to the study of deception detection.

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Misattributions of arousal

Typically, individuals make cognitive appraisals about the sources of their arousal (e.g. Schachter & Singer, 1962). Deception should not be immune to this process. Presumably, lie-tellers should attribute any discomfort or physiological changes that they experience to their own behavior (i.e. the act of deception). However, what if there were an opportunity to make a different attribution?

Several classic studies have demonstrated that attributions can affect the expression of cognitive dissonance. According to cognitive dissonance theory, people experience discomfort and physiological arousal when discrepancies exist between their private thoughts (i.e. cognitions) and public actions (i.e. behaviors; Festinger, 1957).[1] This arousal can manifest through physiological and behavioral changes (Croyle & Cooper, 1983). Festinger argued that because arousal states were aversive, people were motivated to identify the source of the arousal so that it could be eliminated. Expanding upon this notion, Ross et al. (1969) proposed that arousal would be experienced less strongly when it could be misattributed elsewhere. In other words, they argued that when people could attribute their arousal externally rather than internally, they may experience dissonance reduction. For example, Zanna and Cooper (1974) asked participants to ingest a placebo pill said to induce tension, induce relaxation, or have no side effects. They then asked participants to write a counter-attitudinal essay, a task known to incite dissonance and arousal (e.g. Croyle & Cooper, 1983; Linder et al., 1967). When the arousal produced by writing the essay could be misattributed externally to the pill, participants experienced less dissonance. Conversely, those who expected to feel relaxed could not attribute their arousal to this external source and might have attributed it internally. Believing themselves to be more aroused, they exhibited considerably more dissonance.

Seminal misattribution studies have been replicated using various environmental stimuli. These have included room size (Fazio et al., 1977), temperature (Fried & Aronson, 1995), and lighting (Cooper, 1998; Fried & Aronson, 1995; Gonzales & Cooper, reported in Zanna & Cooper, 1976). Findings have also been extended beyond the typical cognitive dissonance paradigm. Storms and Nisbett (1970), for example, argued that if insomniacs could attribute the arousal caused by their condition to an external source (i.e. a placebo pill said to induce tension), rather than to themselves, then they would find it easier to fall asleep. They posited that those told that the pill would induce relaxation would become more agitated upon finding themselves still unable to fall asleep and thus experience heightened arousal. Indeed, this is precisely what was found.

In sum, upon experiencing arousal, people seek to identify its cause within their environment. In the absence of an external stimulus, this process is straightforward: arousal is attributed to an internal source (e.g. inconsistency between cognitions and behaviors), and an attempt is made to correct it. When arousal can be attributed externally, there is less of a need to address the discomfort. However, certain contexts can hinder external attributions. For instance, if a stimulus is reported to induce effects that are inconsistent with what one is experiencing (e.g. relaxation is expected but not experienced), then an external attribution for the arousal is discouraged. In such cases, one may believe oneself to be more physiologically aroused than initially thought, prompting an urgent need for introspection and the experience of even greater arousal.

The present studies

At first, it may seem that cognitive dissonance and deception are independent concepts. However, deception represents an inconsistency between what people believe (i.e. they should not lie) and what they do (i.e. lie anyhow). Even Festinger and Carlsmith's (1959) seminal cognitive dissonance work featured participants misleading others about a tedious task. The dissonance in this experiment was, in essence, produced by lying – an interpretation supported by Festinger (1962) himself. This work suggests that the disparity between cognitions and behaviors related to deception can lead to dissonance. However, attribution research has been largely overlooked in terms of its potential utility for deception detection.

We explored this area of research by extending the aforementioned classic attribution work to deception and its detection. Given that manipulating attributions can affect the expression of cognitive dissonance, we theorized that the same should be true for deception. Opportunities to generate misattributions should differentially affect lie- and truth-tellers, which could be manipulated to facilitate discrimination between the two. In other words, truth-tellers should experience less sympathetic arousal than lie-tellers (de Turck & Miller, 1985) and be unlikely to experience any dissonance, thus rendering them less susceptible to attribution manipulations. By contrast, lie-tellers should experience more dissonance and arousal than truth-tellers, which they can attempt to correct by generating misattributions of arousal. As a result, there should be detectable behavioral differences between truth- and lie-tellers due to differences in the level of effort and motivation underlying these corrections (or lack thereof for truth-tellers). In our first study, we examined attribution effects under typical cognitive dissonance conditions, wherein the decision to lie was made freely. In the second study, we explored a prediction derived from cognitive dissonance theory that these effects would not persist when participants were instructed to lie or tell the truth (i.e. behavior was constrained).

Study 1

We examined observers' abilities to detect deception among targets who were implicitly encouraged or discouraged from attributing the physiological arousal felt during deception to an external stimulus. Overhead lighting was chosen as the critical stimulus because it was innocuous, unobtrusive, and had previously been used to replicate attribution findings (see Cooper, 1998). Due to its immutability, the use of overhead lighting also allowed observed effects to be ascribed to changes in attributions rather than the stimulus itself. We manipulated attributions by telling targets that the lighting had been reported to make others feel aroused or relaxed (vs. control). In reality, the lighting was identical across conditions and had not been associated with effects of any kind.

We used a modified version of the Russano et al. (2005) cheating paradigm. Undergraduate students were tempted to commit a transgression (i.e. cheat on a difficult test) and lie about their actions. Those who chose to lie were led to believe that they might be accused of academic misconduct, making this paradigm arousing and naturalistic. Importantly, invoking psychological realism based on voluntary, motivated behavior ensured that sufficient dissonance and arousal were available for targets to attribute (see Festinger, 1962). Targets were tempted to lie or told the truth under one of three

conditions: (1) the room's overhead lighting reportedly induced arousal (arousal attribution condition), (2) the lighting reportedly induced relaxation (relaxation attribution condition), or (3) the lighting was not mentioned (no attribution control).

In keeping with the original misattribution literature (e.g. Zanna & Cooper, 1974), we theorized that the discomfort and arousal felt during deception could be manipulated. We focused on a proxy for dissonance arousal (i.e. detection of deception), as is typical of the dissonance literature. Similar to Storms and Nisbett (1970), we expected that when provided with an external stimulus to which arousal could be attributed, lie-tellers would dissociate their physiological arousal from the act of deception and ignore the need to resolve the dissonance generated by lying. Thus, two different outcomes were possible. We hypothesized that attributing arousal to an external source could reduce attempts to conceal deception, rendering observers better able to discriminate between lie- and truth-tellers in the arousal attribution condition than in the no attribution control condition (*H1a*). An alternative hypothesis equally consistent with the misattribution literature (e.g. Zanna & Cooper, 1974) was that, because those in the arousal attribution condition were expected to experience less dissonance-based arousal, observers would have more difficulty discriminating between lie- and truth-tellers (*H1b*). Those who expected to feel relaxed due to the lighting (and likely deterred from making an external arousal attribution) were anticipated to grow more uncomfortable upon experiencing deception-related arousal. Awareness of this discomfort was hypothesized to increase lie-tellers' arousal and attempts to conceal their lies (e.g. Frank & Ekman, 1997; Solomon & Corbit, 1974). We predicted that deception would be easier to detect in the relaxation attribution condition, as a result (*H2*). We also explored lighting effects on bias, behavioral cues, and confidence, but did not have any *a priori* hypotheses about the patterns of results.

Method

Participants and design

A 2 (Veracity: lie vs. truth) × 3 (Lighting: arousal attribution vs. relaxation attribution vs. control) within-participants design was used. Observers viewed videos of targets who had lied and told the truth in all three lighting conditions. A total of 117 university students (64 females, 53 males; $M_{age} = 19.81$, $SD = 3.01$) completed the study in exchange for extra credit. These observers self-identified as Aboriginal (0.85%), Arab/West Asian (8.55%), Black (5.98%), Chinese (9.40%), Filipino (29.06%), South Asian (36.75%), Southeast Asian (2.56%), and White (6.84%).

Measures

Target videos. We conducted a modified cheating paradigm with pairs of students (see Russano et al., 2005). One member of each pair was a female confederate, and the other was the target (i.e. an actual university-student participant completing the study for course credit). Once seated in a small room, the experimenter informed pairs that they would be completing a series of logic problems. The experimenter explicitly stated that they must work alone. That is, pairs were not allowed to share answers during the test.

Before leaving the room, the experimenter either made one of two comments about the lighting (adapted from Cooper, 1998) or said nothing, as determined by random assignment. In the arousal attribution condition, she said, 'I do need to apologize for

the lighting in this room. It's old fluorescent lighting, and we have had some reports that it makes some people a bit aroused and tense. It doesn't affect everyone, and the effects are temporary. But, UOIT Facilities and Building Management is looking into it'. Targets in the relaxation attribution condition heard the same apology, except they were told that the lighting might make them feel relaxed and calm. In the control condition, the lighting was not mentioned.

Targets were tempted to cheat on a test (or not) by a female confederate while the experimenter was out of the room. Specifically, the confederate feigned struggling with the second-to-last problem on the test. Once she was certain that the target had written down an answer, she said to those in the cheating condition, 'I can't get the answer – what did you get?' Her actions were determined through random assignment; however, the target's responses were completely volitional. After 20 min had elapsed, the experimenter returned briefly to collect the logic tests and ask targets to complete a demographics questionnaire and a survey purportedly from the university's Facilities and Building Management.[2] Embedded within this survey was the critical question, 'How comfortable were you with the room's lighting?' (1 = *uncomfortable*, 10 = *comfortable*). Then, the experimenter left the room. When she returned, she remarked that both members of the pair had the same wrong answer on one of the test questions. She asked the confederate to wait outside the room while she questioned the target. Once alone, the experimenter explained to the target that she had contacted the laboratory supervisor and that the pair's actions might be considered a case of cheating. Under the guise of establishing what had transpired, she asked the target a series of questions about what had occurred while she was out of the room (e.g. 'Did you ask the other person for help?'; 'Did she ask you for help?'; 'Did you share your answers?'; 'Did you cheat on the test?'). Then, participants were probed for suspicion and fully debriefed.

A hidden video camera documented the entire interview. Only targets' torsos, shoulders, arms, heads, and faces were visible given the camera's angle. We compiled footage from 45 targets, which included 30 truth-tellers (ten in each lighting condition) and 15 lie-tellers (6 in the arousal attribution condition, 4 in the relaxation attribution condition, and 5 in the control condition).[3] Lie-tellers were the targets who chose to cheat on the test and lied about it during the interview. Truth-tellers did not cheat and told the truth during the interview. Confessors – those who cheated and confessed – and targets who did not adhere to condition (i.e. did not cheat if asked or attempted to cheat without being asked) were excluded from analyses. The length of the interviews varied from 31 to 126 s ($M = 60.96$, $SD = 22.39$). There were no differences between conditions in terms of interview length.

Coding. Two independent raters, blind to hypotheses and conditions, coded the videos for 11 behavioral cues of truth and deception using the Psychologically Based Credibility Assessment Tool[4] (PBCAT; Evans et al., 2013). The second rater coded 25% of the videos. Inter-rater reliability was calculated using the two-way random effects model measuring absolute agreement, and ICCs ranged from .80 to 1.00.

Procedure

Observers were tested individually using MediaLab (Jarvis, 2006). First, they provided demographic information and completed the PPI-R. Next, all 45 videos were shown in

a single uninterrupted session (i.e. lighting condition was a within-participants factor). After each clip, observers indicated whether the target was lying or telling the truth and provided their confidence in their judgment (0% [*not at all confident*] to 100% [*extremely confident*]).[5] The entire procedure took approximately one hour.

Results

Preliminary analyses included observer age, gender, and race as factors; none affected the pattern of results. Thus, all reported analyses are collapsed across these variables.

Manipulation Check

Self-Report. We performed an ANOVA on targets' self-reported comfort, with lighting condition as the independent variable. There was a significant lighting effect, $F(2, 84) = 5.26$, $p < .01$, $\eta^2 = .11$, 95% CI [.01, .25]: targets in the arousal attribution condition ($M = 7.27$, $SD = 1.86$) were more uncomfortable than targets in the control condition ($M = 8.59$, $SD = 1.35$). There were no significant differences between the relaxation attribution ($M = 8.10$, $SD = 1.41$) and control, or the relaxation and arousal attribution conditions (all $ps > .05$).

Signal detection analyses

Signal detection theory (SDT; Green & Swets, 1966) can provide a framework for separating performance into two parameters: discrimination and response bias (Meissner & Kassin, 2002). Discrimination (d') refers to an observer's ability to differentiate between truths and lies by taking into consideration the number of targets and lures. A value of zero indicates no ability, and higher values represent increased sensitivity. Response bias (criterion c) refers to an observer's tendency to respond with 'truth' or 'lie'. A value of zero indicates no response bias; here, negative values suggest a tendency to believe the target (i.e. a truth bias), and positive values imply a tendency to disbelieve the target (i.e. a lie bias).

We assigned observers a score of 1 for each correct decision and 0 for each incorrect decision. An *a priori* decision was made to calculate discrimination using the most common sensitivity measure of detection theory, which is d' (i.e. $d' = z(Hit) - z(FalseAlarm)$). Response bias was calculated using the standard bias measure for detection theory, which is criterion c (i.e. $= -\frac{1}{2}[z(Hit) + z(FalseAlarm)]$; see Macmillan & Creelman, 2005). To calculate these parameters, we performed a standard correction recommended by Macmillan and Creelman (1991; 2005) in which values of 0 and 1 are corrected to $1/(2N)$ and $1–1/(2N)$, respectively, where N equals the number of trials on which the proportion is based (i.e. the number of lures).

Discrimination. To test our hypotheses that observers in both the arousal attribution and relaxation attribution conditions would differ from those in the control group (*H1a/b, H2*), we performed a one-way ANOVA on discrimination scores (i.e. d'). There was a significant effect of lighting, $F(2, 115) = 30.28$, $p < .001$, Wilks' $\Lambda = .655$, $\eta^2 = .34$, 95% CI [.20, .48] (Table 1). Observers were better at discriminating between lie- and truth-tellers in the arousal attribution condition, compared to control, $p < .001$, $d = 0.76$, 95% CI [0.38, 1.13] (supporting *H1a*). Discrimination was also higher among those who viewed targets in the

Table 1. Observers' mean discrimination and bias.

Measure	Arousal Attribution M (*SD*)	Relaxation Attribution M (*SD*)	Control M (*SD*)
Study 1			
Discrimination (*d'*)	.35[a] (.65)	.59[a] (.97)	−.17[b] (.72)
Bias (*c*)	.56[a] (.52)	.11[b] (.52)	.47[a] (.54)
Study 2			
Discrimination (*d'*)	.52[a] (.65)	.72[a] (.67)	1.10[b] (.70)
Bias (*c*)	−.20 (.39)	−.28 (.49)	−.21 (.38)

Note. Mean scores with a different superscript differ significantly ($p < .001$, Study 1; $p < .05$, Study 2) from one other. Pairwise comparisons were conducted using the Bonferroni correction.

relaxation attribution condition, compared to control, $p < .001$, $d = 0.78$, 95% CI [0.51, 1.27] (*supporting H2*). The arousal and relaxation attribution conditions did not differ significantly from one another ($p > .05$).

A one sample *t*-test comparing d' values to zero (i.e. no sensitivity) revealed that observers performed significantly better than chance when rating targets in the arousal attribution condition, $t(116) = 5.84$, $p < .001$, $d = 0.54$, 95% CI [0.35, 0.73] and relaxation attribution condition, $t(116) = 6.58$, $p < .001$, $d = 0.61$, 95% CI [0.41, 0.80]. However, performance was lower than chance in the control group, $t(116) = -2.53$, $p = .013$, $d = -0.23$, 95% CI [−0.05, −0.42].

Bias. We performed a one-way ANOVA to determine whether observers exhibited differences in response biases (*c*). There was a significant effect of lighting, $F(2, 232) = 51.80$, $p < .001$, $\eta^2 = .31$, 95% CI [.21, .40] (Table 1). Observers exhibited less of a truth bias when judging targets in the relaxation attribution condition compared to the arousal attribution condition, $p < .001$, $d = -0.87$, 95% CI [−1.25, −0.49] and control, $p < .001$, $d = -0.16$, 95% CI [−0.52, 0.20]. The truth bias between the arousal attribution and control groups did not differ ($p > .05$).

Comparing c to 0 (i.e. no bias) using a one sample t-test revealed that observers exhibited a truth bias when viewing targets in the arousal attribution condition, $t(116) = -11.62$, $p < .001$, $d = 1.07$, 95% CI [.85, 1.30], relaxation attribution condition, $t(116) = -2.21$, $p = .029$, $d = .20$, 95% CI [.02,.39], and control, $t(116) = -9.49$, $p < .001$, $d = .88$, 95% CI [.66, 1.09].

Discussion

The majority of our hypotheses were supported. As predicted, observers were better able to detect lie-tellers who expected to feel relaxed (vs. control), even in the absence of self-reported differences in targets' experience of arousal. Observers also exhibited less truth bias toward targets in the relaxation attribution condition. Deception detection improved when targets thought that the lighting might make them feel aroused, as well. In fact, these targets reported that the lighting made them feel less comfortable than those in the control group. Thus, it is likely that targets' expectations about the effects of the lighting were so strong that lie-tellers attributed deception-related arousal to the lighting and failed to adjust their behaviors accordingly during the interview. These findings suggest that, similar to other domains (e.g.

Storms & Nisbett, 1970; Zanna & Cooper, 1974), misattributions of arousal can affect deception detection.

Study 2

Deception detection research relies on theoretically specified boundary conditions to inform the ecological validity of experiments (see Levine, 2018). In Study 1, we used a modified cheating paradigm in which participants could choose whether they cheated and/or lied, reflecting high ecological validity. Yet, the field of deception detection has been criticized for featuring laboratory-based paradigms in which lie- and truth-telling are experimentally manipulated (see Hartwig & Bond, 2014). Hartwig and Bond's meta-analysis strongly suggests that deception detection is stable across contexts. In the cognitive dissonance literature, however, low choice scenarios have been found to incite less discomfort than high choice scenarios (Cooper, 1998). Thus, the greater experimental control afforded by randomly assigning targets to lie or tell the truth should come at a cost to target dissonance and, in turn, deception detection. We tested this idea.

To ensure that the mere mention of lighting was not responsible for the effects observed in Study 1, we replaced the original control condition with a modified 'unspecified' control condition in which the experimenter discussed the lighting without specifying any side effects. Consequently, in Study 2, the lighting was mentioned in all conditions. We also examined targets' self-reported emotional states, cognitive processing, and attempts to control behavior across conditions, guided by Zuckerman et al.'s (1981) multifactor theory. The multifactor theory identifies physiological arousal, emotional reactions, cognitive effort, and behavioral control as the central components of deceit. Thus, we surveyed targets on their emotional experience, cognitive effort, and behavioral control to explore the underpinnings of attribution effects.

We hypothesized that results from Study 1 would be significantly attenuated in Study 2 because targets had less freedom to choose to lie. That is, we expected that observers would be no better at discriminating between lie- and truth-tellers in the arousal attribution and relaxation attribution conditions compared to the unspecified attribution control (*H1*). We also predicted that observers would continue to demonstrate a tendency to believe targets (i.e. maintain a truth bias) in all conditions (*H2*), consistent with previous findings. Again, we explored targets' behavioral cues and observer confidence.

Method

Participants and design

A 2 (Veracity: lie vs. truth) × 3 (Lighting: arousal attribution vs. relaxation attribution vs. unspecified attribution control) mixed-factors design was used, with veracity as the repeated measure. Observers were randomly assigned to view videos of the lie- and truth-tellers within one of the lighting conditions. Using G*Power 3.1.9.2 (Faul et al., 2007), an *a priori* power analysis was conducted using the effect size of $\eta_p^2 = .144$ obtained in Experiment 1. The analysis revealed that 96 observers were required to achieve 95% power. The university's participant pool permits students to sign up for sessions at least one week in advance; thus, all registered students were permitted to participate, and our final sample size was slightly larger than what was specified. In total, 125

undergraduate students participated in the study in exchange for course credit. Seven observers were excluded from analyses for failing to follow instructions during the experiment. The final sample was 118 observers (66 females, 52 males, $M_{age} = 20.16$, $SD = 2.85$) who self-identified as Aboriginal (0.85%), Arab/West Asian (5.93%), Black (7.63%), Chinese (4.24%), Hispanic (0.85%), Japanese (0.85%), Latin American (0.85%), South Asian (16.10%), Southeast Asian (4.24%), White (48.30%), and Other (10.17%).

Measures

Target videos

Using the suspicious event paradigm (e.g. Elliott & Leach, 2016), we randomly assigned undergraduates to witness a suspicious event and lie or witness an innocuous event and tell the truth. Targets were brought into the laboratory individually and seated at a computer. MediaLab (Jarvis, 2006) randomly assigned targets to one of two video conditions while the female experimenter was out of the room. The innocuous video condition depicted a typical office setting (e.g. pens, books, and a calendar). In contrast, the suspicious video condition showed the same office but contained items suggestive of someone planning an act of terrorism. For example, there were wire-cutters and a bomb-making instructional manual on the desk instead of pens and books. An equal number of items appeared in each video. After the video was shown, on-screen instructions directed all targets to testify to the experimenter that they had viewed the innocuous office setting. Thus, half lied about what they saw, and half told the truth. To motivate targets, they were all instructed that if they successfully convinced the experimenter that they were telling the truth, they would be entered into a prize draw for $50. In fact, all targets were entered into the draw for the prize regardless of their performance.

After seven minutes, the experimenter, who was blind to veracity condition, re-entered the room and told the target that the interview portion of the study would be completed in the next room. While the experimenter was leading the target to the interview room, she made one of three comments about the lighting, as determined by random assignment. In the arousal attribution and relaxation attribution conditions, targets heard the same apologies as in Study 1. In the unspecified attribution control, the experimenter still apologized for the lighting but did not mention specific side effects, saying, 'I do need to apologize for the lighting in this room. It's old fluorescent lighting, and we have had some reports about it. It doesn't affect everyone, and the effects are temporary. But, UOIT Facilities and Building Management is looking into it'. As in Study 1, there were no actual differences in lighting across conditions.

Once the experimenter and target were seated, the experimenter turned on the video camera. Only the targets' heads, shoulders, torsos, and arms were visible in the videos. The experimenter asked open- and closed-ended questions regarding the video that the target had watched (e.g. 'What items did you see on the desk?', 'What was marked on the calendar?', 'Where was the gun?', 'Are you lying to me?'). After the interview was complete, the experimenter turned off the video camera. Each target completed a demographics questionnaire and the same Facilities and Building Management survey as in Study 1.

A more thorough manipulation check was added: targets were asked to rate how tense the room's lighting made them feel (1 = *not at all*; 10 = *extremely*) and describe any side

effects of the lighting they experienced. Using the same 10-point scale, targets also responded to 11 items related to their emotions (i.e. how nervous, aroused, guilty, anxious, and calm they felt when answering the interviewer's questions), cognitive load (i.e. the extent to which they had to think hard, control their behavior, monitor their behavior, monitor the interviewer's behavior, and plan their responses) and motivation to convince the interviewer that they were telling the truth. Finally, suspicion was probed and targets were debriefed.

We compiled footage from 60 interviews, with 10 truth-tellers and 10 lie-tellers from each of the three lighting conditions (arousal attribution, relaxation attribution, and unspecified attribution control). As in Study 1, confessors – targets who confessed to seeing the suspicious video – were excluded from analyses. The length of the interviews varied from 59 to 208 s ($M = 105.22$, $SD = 30.53$), and the conditions did not differ statistically in terms of interview length.

Coding

The same two independent raters coded the video clips using the PBCAT, with the second rater coding 25% of the videos, as in Study 1. ICCs ranged from .74 to 1.00.

Procedure

The procedure was the same as in Study 1, but observers viewed 10 truth- and 10 lie-tellers from one of the three lighting conditions, as determined by random assignment. After each clip, observers were asked to make deception detection and confidence judgments. The entire session lasted approximately 45 min.

Results

Preliminary analyses revealed no significant effects of observer age, gender, or race. Thus, all subsequent analyses are collapsed across these variables.

Manipulation Checks

Self-Report. We performed a MANOVA, with lighting condition as the independent variable, to examine targets' comfort, tenseness, and experience of side effects. Targets' free responses describing any side effects they perceived because of the lighting were recoded as '1' if side effects were perceived and '0' if they were not. Lighting condition did not have a significant effect on the combined dependent variables, $F(6, 108) = 0.352$, $p = .907$, Wilks' $\Lambda = .962$, $\eta^2 = .02$, 95% CI [.00, .03]. All data are provided in the Supplemental Materials section.

Motivation. We conducted a Lighting × Veracity ANOVA on targets' motivation to convince the experimenter of their truthfulness. There were no significant effect of lighting, $F(2, 54) = 1.11$, $p = .338$, $\eta^2 = .04$, 95% CI [.00, .17] or veracity, $F(1, 54) = 0.117$, $p = .734$, $\eta^2 = .00$, 95% CI [.00, .09], nor an interaction effect, $F(2, 54) = 0.061$, $p = .941$, $\eta^2 = .00$, 95% CI [.00, .03]. We conducted a one-sample *t*-test comparing the average motivation score ($M = 6.35$, $SD = 1.85$) to 1 (*not at all motivated*). Motivation was significantly higher than 1,

$t(59) = 22.42$, $p < .001$, $d = 2.89$, 95% CI [2.30, 3.47]. Thus, interviewees reported being sufficiently motivated to convince the interviewer that they were telling the truth.

Emotion. A Lighting × Veracity MANOVA was conducted on targets' responses to the five emotion items. The main effect of lighting on the combined dependent variables approached significance, $F(10, 100) = 1.90$, $p = .053$, Wilks' $\Lambda = .706$, $\eta^2 = .01$, 95% CI [.00, 1.00]. This pattern may have been driven by targets' feelings of anxiousness, $F(2, 54) = 3.21$, $p = .048$, $\eta^2 = .11$, 95% CI [.00, .27]: marginally more anxiousness was reported in the unspecified attribution control than in the arousal attribution condition (Table 2).

There was also a significant main effect of veracity, $F(5, 50) = 5.08$, $p = .001$, Wilks' $\Lambda = .663$, $\eta^2 = .34$, 95% CI [.08, .49]. Veracity had a significant effect on targets' feelings of guilt, $F(1, 54) = 23.47$, $p < .001$, $\eta^2 = .30$, 95% CI [.11, .50], with lie-tellers reporting that they felt guiltier than truth-tellers. There was no significant interaction, $F(10, 100) = 0.778$, $p = .650$, Wilks' $\Lambda = .861$, $\eta^2 = .07$, 90% CI [.00, .09].

Cognitive load. A Lighting × Veracity MANOVA was performed on the five cognitive load items. As shown in Table 2, there was no significant main effect of lighting, $F(10, 100) = 0.860$, $p = .573$, Wilks' $\Lambda = .848$, $\eta^2 = .08$, 95% CI [.00, .10] or veracity, $F(5, 50) = 1.49$, $p = .210$, Wilks' $\Lambda = .870$, $\eta^2 = .13$, 95% CI [.00, .25]. There was no significant Lighting × Veracity interaction, $F(10, 100) = 1.32$, $p = .230$, Wilks' $\Lambda = .780$, $\eta^2 = .12$, 95% CI [.00, .16].

Signal detection analyses

As in Study 1, deception judgments were recoded using 1 for each correct decision and 0 for each incorrect decision, and observers' scores were averaged. After standard corrections were made (see Study 1), signal detection analyses were conducted to examine the independent contributions of discrimination and bias.

Discrimination. To test our hypothesis that discrimination would not differ between lighting conditions (*H1*), we conducted a one-way ANOVA on *d'*. Contrary to our hypothesis, lighting had a significant effect on discrimination, $F(2, 115) = 7.86$, $p = .001$, $\eta^2 = .12$,

Table 2. Truth- and lie-tellers' emotional states and cognitive load (Study 2).

Measure	Arousal Attribution		Relaxation Attribution		Unspecified Attribution	
	Lie M (*SD*)	Truth M (*SD*)	Lie M (*SD*)	Truth M (*SD*)	Lie M (*SD*)	Truth M (*SD*)
Emotion						
Nervous	6.0 (2.98)	5.2 (2.57)	5.2 (2.44)	6.0 (2.67)	6.9 (2.18)	4.9 (2.23)
Aroused	4.0 (2.45)	4.0 (2.71)	2.6 (1.71)	3.6 (2.12)	5.2 (1.87)	3.9 (1.91)
Guilty	6.4^a (2.37)	4.2^b (3.46)	5.6^a (2.76)	2.7^b (2.67)	7.5^a (2.51)	2.6^b (2.01)
Anxious	4.8 (2.30)	4.4 (2.50)	6.0 (2.31)	4.8 (3.19)	7.7 (1.42)	5.2 (1.75)
Calm	5.4 (1.71)	4.3 (2.50)	6.0 (2.31)	4.8 (3.19)	6.6 (1.65)	4.5 (2.01)
Cognitive Load						
Thinking hard	4.3 (1.70)	5.3 (2.26)	5.6 (1.84)	4.7 (2.11)	6.3 (2.71)	5.6 (2.55)
Controlling self	5.1 (1.73)	5.4 (3.13)	6.4 (1.51)	3.9 (2.81)	6.0 (2.45)	4.9 (2.23)
Monitoring self	5.8 (1.93)	5.4 (2.32)	5.8 (1.81)	4.6 (3.06)	6.1 (2.51)	4.4 (2.01)
Monitoring interviewer	5.7 (2.75)	6.8 (2.30)	5.8 (2.35)	6.3 (2.87)	5.4 (3.06)	5.7 (2.71)
Planning	4.4 (2.46)	4.2 (2.30)	5.2 (1.62)	4.9 (3.41)	5.8 (2.10)	3.2 (1.75)

Note. We examined the univariate effects using a Bonferroni-adjusted alpha ($\alpha_{altered} = .01$). Mean scores with a different superscript differ significantly ($p < .01$) from one other.

95% CI [.02, .24] (Table 1). Observers were significantly poorer at discriminating between lie- and truth-tellers in the relaxation attribution condition, $p < .001$, $d = -0.96$, 95% CI [0.49, 1.42] and arousal attribution condition, $p = .042$, $d = -1.30$, 95% CI [0.83, 1.78] than in the unspecified attribution control. Discrimination in the arousal and relaxation attribution conditions did not differ.

A one sample t-test was conducted to compare d' to zero (i.e. no sensitivity). Observers performed significantly above chance when rating targets in the arousal attribution condition, $t(40) = 5.06$, $p < .001$, $d = 0.79$, 95% CI [0.44, 1.14], relaxation attribution condition, $t(36) = 6.49$, $p < .001$, $d = 1.07$, 95% CI [0.66, 1.47], and unspecified attribution control, $t(39) = 10.02$, $p < .001$, $d = 1.58$, 95% CI [1.11, 2.05].

Bias. We performed a one-way ANOVA to compare response bias (*c*) across lighting conditions. There were no significant differences, Welch's $F(2, 71.84) = 0.217$, $p = .806$, $\eta^2 = 0.01$, 95% CI [.00, .06] (Table 1).

We also compared *c* scores to 0 (i.e. no bias) using a one sample *t*-test to test whether observers tended to believe targets (*H2*). We found that observers exhibited a significant truth bias in each of the arousal attribution, $t(40) = -3.38$, $p = .002$, $d = -0.53$, 95% CI [−0.85, −0.21], relaxation attribution, $t(36) = -2.59$, $p = .014$, $d = -0.43$, 95% CI [−0.76, −0.11], and modified control conditions, $t(39) = -3.41$, $p = .002$, $d = -0.54$, 95% CI [−0.87, −0.21]. That is, our hypothesis was supported.

Discussion

We investigated the hypotheses that guided the first experiment in an experimentally manipulated context. Arousal was expected to be low due to a lack of freedom of choice. Indeed, targets' self-reported arousal ranged from as low as 2.6 to a maximum of just 5.2 out of 10. As predicted, discrimination did not significantly differ between observers who viewed targets in the arousal attribution and relaxation attribution conditions. A truth bias toward targets was also consistent across conditions.

Unexpectedly, however, observers who viewed targets in the unspecified attribution control outperformed those who viewed targets encouraged to make specific misattributions. We suspect that by warning the targets in the unspecified attribution control about the presence of unknown side effects, we inadvertently increased their arousal. Feelings of uncertainty and ambiguity are often unpleasant and cause confusion, leading to heightened discomfort and arousal, much in the same way as cognitive dissonance (Berlyne, 1960; Jonas et al., 1997; Leventhal, 1974). As such, targets in the unspecific attribution control might have perceived the ambiguous lighting remark negatively. Consistent with this interpretation, targets in the unspecified attribution control reported feeling slightly more anxious than targets in the arousal attribution condition. Although the effect of lighting condition on targets' self-reported emotionality failed to reach statistical significance, the large effect size ($\eta^2 = .11$) should not be discounted. Without a clear description of prospective side effects, targets in the unspecified attribution condition may have assumed more severe adverse effects than even those in the arousal attribution group and experienced greater arousal. This heightened arousal may have narrowed targets' focus to the point of excluding salient information (e.g. Easterbrook, 1959), which could have resulted in the targets failing to attend to the information in the

suspicious event paradigm videos. Consequently, they may have been less able to deceive the experimenter convincingly.

General discussion

As predicted by the classic cognitive dissonance literature (e.g. Zanna & Cooper, 1974), there was preliminary evidence that attributions of arousal might affect patterns of deception detection under conditions of free choice. In Study 1, observers were better able to discriminate between lie- and truth-tellers when an attribution was made in the relaxation attribution condition and in the arousal attribution condition (vs. control). Importantly, these results were obtained using an ecologically valid paradigm, designed to mirror studies within the misattribution literature. In Study 2, we used a design with more experimental control, akin to those common within the deception literature. Here, we predicted that deception detection performance would not differ between conditions because targets lacked freedom of choice – typically a necessary condition for dissonance to develop (e.g. Linder et al., 1967). We found that while there were no differences in observers' discrimination abilities between the relaxation attribution and arousal attribution conditions, targets in the unspecified attribution control were consistently classified more accurately than those in the specific attribution conditions.

We posit that the deception paradigms used in each study account for the variability in our findings. Notably, targets' deception was volitional in Study 1 but experimentally manipulated in Study 2. The modified cheating paradigm used in Study 1 both invoked psychological realism and was based on participants' freely chosen lie-telling behavior, thus ensuring that sufficient dissonance and arousal were available for targets to attribute (see Festinger, 1962). Because our manipulation checks only revealed a group difference in discomfort in Study 1, deception type (i.e. volitional vs. experimentally manipulated) might have moderated targets' experience of arousal. Our findings suggest that freedom of choice is possibly a necessary condition for effective misattribution manipulations in deception. Additional research explicitly testing this moderation effect is required, however, as we did not directly manipulate freedom of choice within a single experiment and deception paradigm, controlling for other factors. Identifying limits to the conditions under which a particular set of results can be obtained is a crucial aspect of theory development. As such, Study 2 not only advances the theoretical understanding of misattribution effects, but also provides practical insight as to when certain deception detection techniques may or may not be successful (see also Lane & Vieira, 2012).

Interestingly, attributions about the lighting also affect observers' biases. Truth biases may be problematic in real-world forensic contexts, where the base rate of lying can be higher than in daily social interactions. Encouragingly, in Study 1, observers were less biased in the relaxation attribution condition than in the control condition. In fact, the experimenter's comments were able to alter deception detection biases with unexpected ease. Within a single session, observers' biases varied with the stimuli. Recall that, on the surface, the video stimuli were the same across conditions, as all targets responded to the same questions. These findings support the notion that manipulating cognitive processes has the potential to reduce response biases. We suggest that if observers' response biases

can demonstrate variability with experimentally manipulated stimuli, response biases may not be stable. Rather, they may depend on subtle, underlying variations in targets, and be more flexible than originally theorized.

These proof-of-concept studies offer several directions for future study. Consistent with past research (e.g. Cooper, 1998), we asked targets to rate their discomfort with the room's lighting as a manipulation check. The existence of a group difference on this measure in Study 1 but not Study 2 suggests that there may not have been ample arousal to attribute in the latter study. Alternatively, the check itself may have been inadequate. In an effort to replicate previous research, we intentionally used the same manipulation check as in the classic misattribution studies upon which this work is based (e.g. Cooper, 1998; Fried & Aronson, 1995; Gonzales & Cooper, reported in Zanna & Cooper, 1976) to ensure that findings could be generalized from the attribution literature to the deception detection literature. Consistent with these studies, we did not directly ask about arousal or conduct a memory check for the comment about the lighting to avoid generating suspicion or priming targets by alerting them to the purpose of the study. Instead, questions about the lighting were asked through an ostensibly unrelated survey so that targets would not suspect the true nature of the study. A modified manipulation check may provide additional insight into the source of the observed group difference (or lack thereof).

We also did not explicitly measure arousal or dissonance. Instead, a proxy was used, as is typical within the literature (e.g. Cooper, 1998; Fazio et al., 1977; Storms & Nisbett, 1970). A direct measure of arousal (e.g. skin conductance, heart rate) would be challenging to obtain without inciting suspicion and demand characteristics among targets. Importantly, however, all observers were blind to condition within each study. The existence of significant differences on multiple outcome measures across lighting conditions does provide some evidence that our manipulations might have been effective, despite not being reflected in the checks that we used. To address these limitations, future research should be double-blinded and incorporate more sensitive manipulation checks, such as those that use psychophysical arousal measures, or attention checks to ensure that targets' abilities to focus on the presented stimuli is adequate even under high arousal conditions.

Our findings offer several implications for research and theory. First, our findings provide preliminary support for the notion that cognitive appraisals and attributions of arousal play a role in deception and its detection. That deception detection performance was altered by the misattribution manipulations is both novel and predicted by theory, thus contributing to our understanding of both the underpinnings of deception and basic attribution processes. This finding illustrates the value of contemporary integrations of classic theories in both research and practice. Second, few researchers have explicitly examined the stability of response biases and their effects on decision-making beyond deception detection (e.g. progression to interview). Our finding that bias can be so easily manipulated with a brief remark about the lighting is unprecedented. Finally, although this work is exploratory in nature, generating misattributions appears to be a fast, simple, and cost-effective way to facilitate deception detection, but only in arousing and volitional contexts. These avenues offer promising new directions that complement existing work on cognitive theories of deception detection.

Conclusion

We drew from well-established attributions of arousal literature to explore a novel approach to deception detection. As predicted, targets' opportunities to attribute arousal to an external source affected observers' deception detection and their response biases. The voluntariness of targets' deception may have been critical: experimentally manipulated lies did not appear to elicit the necessary degree of discomfort and arousal required to generate attributions. This work is the first to revitalize classic attribution theories to test an attribution approach to deception detection. Additional research is necessary to confirm the conditions under which effects can be replicated and strengthened; however, results suggest that manipulating attributions is a promising new theoretical approach to studying deception detection.

Notes

1. Eventually, dissonance produces attitude change; therefore, attitude change is often an indicator of dissonance. However, this aspect of the model is least applicable to deception detection. Deception detection researchers have not examined attitude change, nor is it necessarily useful for their purposes, because an *a posteriori* assessment of attitudes about lying will not make lies any more or less detectable. Thus, we do not include this aspect of cognitive dissonance in the present research.
2. Participants were also asked to complete the Psychopathic Personality Inventory-Revised (PPI-R; Lilienfeld & Widows, 2005) – a 154-item measure of psychopathic traits. This scale was part of a separate study relating psychopathic traits to global deception detection abilities (see Martin & Leach, 2013). The PPI-R was not relevant to the current research questions, and the previous study did not involve hypotheses or analyses of attribution effects.
3. An equal number of targets was intended for each lighting condition. Due to a coding error, an extra target in the arousal attribution condition was mislabeled as belonging to the relaxation attribution condition.
4. No differences in PBCAT measures were observed between lighting conditions across experiments. For the sake of brevity, these results are not reported here; however, they have been included as Supplemental Materials.
5. Analyses of these data appear in the Supplemental Materials section.

Disclosure statement

No potential conflict of interest was reported by the author(s).

Ethics approval

This research was approved by the Ontario Tech University Research Ethics Board (REB #08-053, #15-062). Written informed consent was obtained from all participants.

Author contributions

The second author conceptualized, designed, and conducted Study 1. Both authors were equally involved in the conception, design, data collection, and data analysis of Study 2. Both authors contributed to writing the manuscript.

Data availability

The data that support the findings of this study are available upon request from the corresponding author.

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