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To cite this article: Aldert Vrij, Christian A. Meissner & Saul M. Kassin (2015): Problems in expert deception detection and the risk of false confessions: no proof to the contrary in Levine et al. (2014), *Psychology, Crime & Law*, DOI: [10.1080/1068316X.2015.1054389](https://doi.org/10.1080/1068316X.2015.1054389)

To link to this article: <http://dx.doi.org/10.1080/1068316X.2015.1054389>



Accepted online: 26 May 2015. Published online: 14 Aug 2015.



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## Problems in expert deception detection and the risk of false confessions: no proof to the contrary in Levine et al. (2014)

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*(Received 21 January 2015; accepted 29 April 2015)*

Lie-detection research has shown that observers who rely on nonverbal cues or on verbal cues correctly classify on average 54% of truth tellers and liars. In addition, over the years, countless numbers of innocent people have made false confessions and, in analysing the problem, researchers have implicated both a suspect's vulnerability and the persuasive influence of certain police interrogation tactics. Levine et al. (2014) aim to contribute to these vast bodies of literature by reporting two studies purportedly showing that expert interviewers – when they are permitted to question interviewees – can achieve almost perfect accuracy without eliciting false confessions. We argue that theoretical and methodological aspects of these studies undermine the reliability and validity of the data reported, that as a result the studies do not contribute to the scientific literatures on lie detection and false confessions in any meaningful way, and that the results are dangerously misleading.

**Keywords:** false confession; confession; credibility; deception; deception detection

More than half a century of lie-detection research has shown that observers who rely on nonverbal cues or on verbal cues without using established verbal lie-detection procedures such as Criteria-based Content Analysis (Steller & Köhnken, 1989; Vrij, 2005), or Reality Monitoring (Masip, Sporer, Garrido, & Herrero, 2005; Sporer, 2004), correctly classify on average 54% of truth tellers and liars (Bond & DePaulo, 2006). This percentage is low given that 50% of truth tellers and liars would be correctly classified by just flipping a coin. Practitioners who see themselves as experts in lie detection and lay persons achieve similar accuracy rates (Aamodt & Custer, 2006; Bond & DePaulo, 2008; Vrij, 2008). Research suggests a possible explanation for the empirical failures of lie-detection training: by focusing on non-diagnostic behavioral cues, the Reid Technique (Inbau, Reid, Buckley, & Jayne, 2013) merely formalizes the folk wisdom that laypeople already use without much success (Masip, Barba, & Herrero, 2012; Masip, Herrero, Garrido, & Barba, 2011). In fact, the primary differences between experts and lay persons are that experts profess greater confidence in their ability to detect deceit and they tend to demonstrate a bias

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towards perceiving deception (Kassin, Meissner, & Norwick, 2005; Meissner & Kassin, 2002; Vrij, 2008).

Those who train practitioners for a living have argued that basic research is limited in external validity because laboratory experiments typically involve college students randomly assigned to lie or tell the truth in non-consequential, low stakes situations (Buckley, 2012; Frank & Svetieva, 2012; O'Sullivan, Frank, Hurley, & Tiwana, 2009). Yet in a recent meta-analysis of 144 samples containing 9380 speakers, providing a total of 26,866 messages, and spanning more than 40 years, Hartwig and Bond (2014) found that the detectability of deception did not differ as a function of whether the speaker was a college student or non-student; whether the motivation to evade detection was high or low; or whether the truths and lies were accompanied by high or low levels of emotion. Indeed, recent research has shown that the ability to discriminate between truth tellers and liars can be improved only when certain theoretically based interview protocols are used (Granhag & Hartwig, 2015; Hartwig, Granhag, & Luke, 2014; Vrij, 2015) and when training focuses on the most diagnostic cues (Hauch, Sporer, Michael, & Meissner, *in press*).

Over the years, countless numbers of innocent people have been wrongfully convicted after confessing to crimes they did not commit. This phenomenon has occurred throughout history, though the extent of the problem was unclear (e.g., Kassin & Wrightsman, 1985; *Miranda v. Arizona*, 1966; Munsterberg, 1908). Then in 1992 the Innocence Project was founded for the purpose of using new DNA technology to test biological evidence in disputed conviction cases involving rapes and murders. It is now clear that more than 25% of these DNA exonerations have involved as a contributing factor the false confession of an innocent person (Garrett, 2011; [www.innocenceproject.org/](http://www.innocenceproject.org/)). It is important to note that this sample of DNA exonerations represents only a fraction of all wrongful convictions – those cases in which the consequences were most severe (for a more comprehensive data base, see National Registry of Exonerations; [www.law.umich.edu/special/exoneration/Pages/about.aspx](http://www.law.umich.edu/special/exoneration/Pages/about.aspx)). Indeed, contrary to the commonsense belief that people do not confess to crimes they did not commit, large numbers of such cases have been exposed – not only in the USA, but all over the world. In analyzing the problem, researchers have implicated both individual difference characteristics that increase a suspect's vulnerability (such as youth, intellectual disability, and mental illness) and the persuasive influence of certain police interrogation tactics (such as accusatorial methods that include the use of guilt-presumptive questions, the presentation of false evidence, and minimization tactics that imply leniency; for reviews, see Gudjonsson, 2003; Kassin & Gudjonsson, 2004; Lassiter & Meissner, 2010; Meissner et al., 2014; Warden & Drizin, 2009; for an official White Paper, see Kassin et al., 2010).

Levine et al. (2014) aim to contribute to this vast body of lie-detection and false confessions literature by reporting two studies purportedly showing that expert interviewers – when they are permitted to question interviewees – can achieve almost perfect accuracy without ever eliciting a false confession. Specifically, using a variation of the Exline, Thibaut, Hickey, and Gumpert (1970) and Russano, Meissner, Narchet, and Kassin's (2005) cheating paradigms, Levine et al. permitted participants an opportunity to cheat at a trivia game in which they competed for a cash prize. Those who cheated were considered guilty, those who did not were considered innocent. Experienced interrogators then questioned the subjects in an effort to determine guilt or innocence through the elicitation of a confession or admission. In Study 1, the

interrogator was J. Pete Blair, an author of the published study who was aware of the study hypotheses. In Study 2, the interrogators were ‘five federal agents with substantial polygraph and interrogation experience’, In Study 1, the single expert obtained a 100% accuracy rate in distinguishing truth tellers (non-cheaters) from liars (those who cheated). In Study 2, five different experts obtained an average 97.7% accuracy rate. Furthermore, the experts in these studies did not elicit a single false confession. These are unquestionably the best interviewing data ever published in a peer-reviewed journal. An article that reports such high accuracy rates will attract much attention, not only from researchers, but also from the media and practitioners.

In this article, we argue that both theoretical and methodological aspects of these two studies seriously undermine the reliability and validity of the data reported, and that as a result the studies do not contribute to the scientific literatures on lie detection and false confessions in any meaningful way. In fact, the results are dangerously misleading.

### **Truth telling is easy and lying is difficult in the Levine et al. paradigm**

Levine et al. (2014) report that only ecologically valid studies reveal the true ability of experts to detect deception. While we have raised the same point (Vrij & Granhag, 2012), ecological validity should not be restricted to the task of the interrogator – it should also apply to the task of the truth tellers and liars in the deception study. Levine et al. neglect this second aspect of ecological validity and it is this aspect of their study that we find highly problematic. Participants in Levine et al. played a trivia game with a partner (a confederate) and received money (\$5 in Study 1; \$10 in Study 2) for each correct answer. The experimenter was called out of the room after the 3rd of 10 questions, at which point the confederate suggested cheating – and hence stealing money – during the experimenter’s absence. Very few participants in Study 1 (4 out of 33) but more participants in Study 2 (40 out of 89) actually cheated, and hence were ‘guilty’. All participants were then questioned by the expert interviewer. In Study 1, participants were asked which questions they got right and how they knew these correct answers. The participant was then asked whether s/he cheated and was informed that the partner would be interviewed next. The interviewer then left the room ‘to check some things’ and accused the participant of cheating upon his return. All four cheaters confessed. In Study 2, the interviews were unscripted and no information about the interview style was given. A total of 34 out of 40 cheaters confessed. No false confessions occurred in either study. The authors do not provide examples of the questions used in the trivia game except to say that they were difficult and that giving a correct answer was highly diagnostic (i.e. cheaters give correct answers, non-cheaters do not). The decision rule that more than one correct answer indicates cheating yielded an accuracy level of approximately 80%.

Let us go over this study again. Participants can win a cash prize when answering questions correctly. However, the questions are so difficult that most participants cannot give more than one correct answer. Those who gave a correct answer were asked how they came to know that answer. Those who really knew the correct answer could easily explain; those who cheated could not. Liars were thus set up in this experiment and had to lie about a topic for which they had no knowledge. Of course, such liars can be easily distinguished from truth tellers and for the investigator the lie-detection task is strikingly easy: judge anyone who gives no correct answer as a

truth teller (because why would a cheater who knew a correct answer not disclose it?) and ask those who did give one or more correct answers how they came to know these answers. If the participant can give a convincing answer, then they are a truth teller. If they cannot give a convincing answer, ask further questions. This makes the situation very awkward for liars because they will struggle to provide a convincing explanation. Clearly, the liars themselves realized the awkwardness of their situation; hence, the vast majority of them gave up and confessed. Their confessions made the investigator's lie-detection task even easier than it already was.

The experimental design used by Levine et al. does not begin to resemble truth and lie detection in real-life situations. The difficulty that investigators face in real life is that truth tellers can give poor and unconvincing performances, which can be easily exaggerated with an intimidating interview style resulting in false accusations of lying or false confessions (Kassin et al., 2010). This risk is not present in this scenario, as a single question easily discriminated between cheaters and non-cheaters. Most truth tellers did not know any correct answers or only one correct answer. Since no interrogator would think that participants who did not give a correct answer would be lying, false accusations will not occur in that group and there is no inclination to increase the pressure on such truth tellers. Truth tellers who gave a correct answer could explain where that knowledge came from. Thus, they could easily convince the interviewer that they were telling the truth, resulting in no false accusations and no inclination to intimidate them.

Another difficulty investigators face in real life is that liars can give good and convincing performances. They prepare themselves for their interviews (Granhag & Hartwig, 2008) and often embed their lies in mostly truthful accounts (Leins, Fisher, & Ross, 2013; Vrij, 2008). Liars are seldom unprepared for an interview, as they were in this study, nor would they choose to lie about something about which they have no knowledge. In fact, the liars in this study found themselves in such a futile situation that 38 out of 44 who cheated in the two studies simply gave up and confessed. In other words, the ecological validity from the perspective of truth tellers and liars in the two studies was poor and this resulted in inflated accuracy rates.

The task for observers who were shown the videotaped interviews was also easy. If a simple confession-based decision rule was used – that is, ‘all non-confessors are non-cheaters and all confessors are cheaters’ – this heuristic would have resulted in a correct classification of all truth tellers (non-cheaters) and liars (cheaters) in Study 1, the same 100% accuracy rate that the singular expert in Study 1 had obtained. In Study 2, all 49 truth tellers and 34 out of 40 liars would have been classified correctly, yielding a 93.2% accuracy rate, similar to the 97.7% accuracy rate obtained by the experts in Study 2. Using a confession-based decision rule means that Levine et al.'s article is not about deception detection at all, as they concede themselves, but rather about how individuals use confessions to determine the guilt or innocence of interviewees. This latter finding is quite in line with the findings of Narchet, Meissner, and Russano (2011) who demonstrated that interviewers' judgments of guilt-innocence in the cheating paradigm are largely based upon whether a confession has been elicited.

### **The studies say nothing about expertise in interviewing**

The interviews in Study 2 were not scripted. We understand that unscripted interviews are necessary to examine whether experts can accurately distinguish between truths

and lies in interviews. However, no analysis or description of the interview protocol or methods used by the experts was disclosed, which makes it impossible to determine what factors made the interviewers so apparently effective. The failure to include a control group of non-expert interviewers in these studies also means that no conclusion can be derived about the role of expertise. Given that truth telling was very easy and lying was very difficult, the combination of which creates highly favorable conditions for accurate truth and lie detection, we cannot rule out that non-expert interviewers would perform equally well in this paradigm – and therein that the interview style or expertise of the interviewer was irrelevant.

### **The false confessions problem has been misrepresented by Levine et al.**

Despite the numerous false confessions discovered in wrongful conviction cases, often involving rape and murder (Garrett, 2011; [www.innocenceproject.org/](http://www.innocenceproject.org/)), and in contrast to the false confessions routinely obtained in laboratory experiments that used the computer crash paradigm (Kassin & Kiechel, 1996) even when the confession was said to bear a financial or other consequence (Horselenberg, Merckelbach, & Josephs, 2003; Redlich & Goodman, 2003), the cheating paradigm (Russano et al. 2005) and adaptations of it (Perillo & Kassin, 2011), and other paradigms involving stolen money (e.g., Nash & Wade, 2009), Levine et al. report the ‘provocative finding’ (p. 457) that not a single false confession was elicited in their studies – ever.

Why might this be the case? The answer is simple: false confessors capitulate for a reason – because they are badgered over lengthy periods of time; want to escape a situation that is bad and getting worse; feel distressed, confused, and helpless to prove their innocence; or mistakenly believe that confession is in their self-interest. This is precisely what happened in 1955 when a false confession to murder was extracted from Darrel Parker by John Reid, architect of the Reid Technique and founder of John Reid & Associates (Starr, 2013). Yet clearly, the innocent participants in Levine et al. had no reason to confess. They did not cheat, *and they could prove it!* They could prove it because they could explain how they came to know the answers to the questions they answered correctly or by admitting that they did not know a single correct answer. In addition, in Study 1, the experimenter who administered the trivia game knew how many questions each participant had answered correctly, the partner/confederate knew whether the participant had cheated, and the interviewee had every reason to think that both individuals were available for the investigator to confirm innocence when he walked out of the interview room to ‘check on some things’.

Simply put, the innocent participants in this study – compared to those in other studies, and certainly compared to those who were wrongfully convicted in real life – were given no reason or motivation to confess to a transgression they did not commit. We would argue that the ‘How did you come to know the answer’ question was so diagnostic that investigators never had to apply the kinds of coercive questioning strategies that would yield a false confession. Hence, it is not surprising that other studies as well involving the cheating paradigm conducted by Levine et al. have similarly failed to ever obtain a false confession (Levine, Blair, & Clare, 2014; Levine, Kim, & Blair, 2010; Levine, Shulman, Carpenter, DeAndrea, & Blair, 2013).

Instead of pointing out that truth tellers could easily prove their innocence, Levine et al. (2014) describe several possible moderators that distinguish their studies and

findings from the original cheating paradigm study conducted by Russano et al. (2005). First, they suggest that there are differences in the seriousness of the offense, suggesting that the Russano et al. study frames cheating as an act of ‘helping’ another participant, while their study involved a motivation of ‘self-gain’. We argue that the framing of severity and related consequences across the two paradigms actually differed in the opposite direction. The Russano et al. study framed the cheating violation to all participants as a serious case of academic dishonesty. In contrast, Levine et al. state that their cheating is not described as serious, but rather is minimized by the interviewer: ‘It was explained that the important thing was the integrity of the data, and that was all the interviewer was interested in’ (p. 452). Thus, the perceived consequences of confessing (and therein the severity of their behavior) was likely quite low for Levine et al.’s participants.

A second moderator according to Levine et al. was the role of demand characteristics. They suggest that Russano et al. were motivated to demonstrate false confessions, and therein their study produced such data; in contrast, Levine et al. were motivated to demonstrate accuracy, and therein their study produced highly diagnostic outcomes. We agree with Levine et al. that their study suffers from the potential for demand characteristics. Study 1 is problematic precisely because the interviewer was a member of the research team and not blind to the hypotheses or desired outcomes. We know nothing about what the experts in Study 2 were informed of when they were ‘thoroughly briefed on the experimental setting and cheating game setup’ (p. 451), and we know nothing about the methods they used to elicit statements – so we can only speculate about the potential role of demand characteristics in these findings. In stark contrast, the Russano et al. study followed best practices in behavioral science for reducing the possibility of demand characteristics. All interviewers were blind to study manipulations and to the hypotheses of the study. Interviews were completely scripted and interview methods were manipulated across participants. Random assignment was used to determine guilt or innocence, as well as exposure to the interrogation scripts. In fact, the findings of Russano et al. are significantly less likely to have suffered from demand characteristics because these procedures were followed.

Finally, Levine et al. point to the potential role of interviewer intent. The authors suggest that their interviewers were focused on discovering the truth and that they were likely more discriminant as a function of this. We agree, in part, with Levine et al. on this point – and we do so based squarely upon data from a study employing the Russano et al. paradigm. Narchet et al. (2011) used the cheating paradigm to explore the role of interviewer intent on (a) the methods used to elicit a statement, (b) the diagnostic value of statements elicited, and (c) how an interviewer’s intent influenced their perception of the likely guilt/innocence of the subject.

We pause here to note once again that the Narchet et al. (2011) study followed prescribed practices for controlling demand characteristics – experimenters and interviewers were blind to the study hypothesis, and to the study’s manipulation of interviewer expectancy. Narchet et al. (2011) observed clear evidence for the role of interviewer intent in the behavioral confirmation process. When interviewers were led to believe that the subject was likely guilty, they used more guilt-presumptive methods, elicited more false confessions from the innocent, and were biased to believe that participants were more often guilty (not innocent), particularly once they had elicited a confession. When interviewers in the Narchet et al. (2011) study

were led to believe that the participant was likely innocent, interrogations still produced false confessions, though they were significantly less frequent when compared with investigators who presumed guilt. An analysis of these confessions from the innocent demonstrated that they were the product of methods known to produce false confessions (minimization, maximization, and false evidence ploys).

With respect to the Levine et al. studies, we do not know what tactics were employed in Study 2 and whether all participants in Study 1 experienced the same interview protocol. An analysis of the methods used by experts in Study 2 could inform us regarding the role of certain interview methods in eliciting true confessions, and whether the same or different methods were applied when the suspect was determined to be innocent (perhaps as a result of the 'correct question' tactic) vs. guilty. Nevertheless, interviewer intent (in particular, their belief in the guilt or innocence of the suspect) can alter the manner in which an interrogation is conducted and is likely to influence the incidence of a false confession.

### Conclusion

Levine et al. (2014) claim that experts can accurately distinguish truths from lies when they are allowed to actively question a potential liar. We agree that using specific interview protocols facilitates lie detection, though we base that conclusion on the extensive research in this area rather than on the Levine et al.'s study. The problem with the Levine et al.'s paradigm is that it is easy for truth tellers to demonstrate their innocence but virtually impossible for liars to tell a convincing lie. When it is obvious to investigators who is telling the truth and lie, high accuracy rates will appear and oppressive interview styles that elicit false confessions are unlikely to occur. In our view, therefore, the studies reported in this article are not only out of step with prior research – they offer no proof of the authors' claims and are dangerously misleading.

### Disclosure statement

No potential conflict of interest was reported by the authors.

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