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SPECIAL ISSUE ARTICLE

WILEY

Discriminating deceptive from truthful statements using the verifiability approach: A meta-analysis

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Abstract

The Verifiability Approach predicts that truth tellers will include details that can be verified by the interviewer, whereas liars will refrain from providing such details. A meta-analysis revealed that truth tellers indeed provided more verifiable details ($k = 28$, $d = 0.49$, 95% CI [0.25; 0.74], $BF_{10} = 93.28$), and a higher proportion of verifiable details ($k = 26$, $d = 0.49$ 95% CI: 0.25, 0.74, $p < .001$, $BF_{10} = 81.49$) than liars. We found no evidence that liars would include more unverifiable details than truth tellers ($k = 20$, $d = -0.31$, 95% CI [-0.02; 0.64], $BF_{10} = 1.12$) Moderator analysis revealed the verifiable detail effect was substantially larger when the statement is the suspect's alibi, but smaller when an incentive to appear credible was used. Our findings support the main prediction behind the Verifiability Approach, but also stress the need for larger sample sizes and independent replications.

KEYWORDS

deception detection, honesty, meta-analysis, reality monitoring, verifiability approach

1 | INTRODUCTION

Lie detection has been shown notoriously difficult not just for lay people, but also for professionals including police officers and judges (C. F. Bond & DePaulo, 2008, 2006). Because decades of research yielded very few, if any, robust non-verbal cues to deception, researchers turned their attention to the content of statements. Data do suggest that truth tellers, compared to liars, typically provide statements that are richer in detail (DePaulo et al., 2003; Hauch et al., 2017; Luke, 2019; Oberlander et al., 2016), and this finding is incorporated in several lie detection tools including CBCA, RM, and ACID (Colwell et al., 2007; Sporer, 1997, 2004; Steller & Köhnken, 1989). The recently developed Verifiability Approach to lie detection (VA; G. Nahari & Vrij, 2014; G. Nahari, Vrij, & Fisher, 2014a, 2014b) builds upon this research on detailedness, but suggests that verbal credibility assessment can be improved by considering whether or not the provided details can be verified. The current manuscript reports a meta-analysis to evaluate the validity of this approach.

The VA is built on research showing that liars and truth tellers differ in the strategies they use to disclose information (G. Nahari, Leal, et al., 2014). Truth tellers adopt forthcomingness and tell it as it happened, including all the relevant details in their statement (Strömwall et al., 2006; A. Vrij, Mann, et al., 2010). Liars realize that their account is more likely to be believed when it is perceived to be rich in detail (G. Nahari, Vrij, Fisher, 2012), posing them with a dilemma; providing too little information will make them look deceptive, whereas providing too much detail would allow the investigative authorities to detect their deception by further investigation (G. Nahari, 2018). The VA predicts that liars solve this dilemma by (1) selectively leaving out details that investigators can verify (i.e., verifiable details; G. Nahari, Vrij, & Fisher, 2014a, 2014b); for example, "I sent an email to my boss around 3:00 this afternoon," and (2) including more details that are difficult to verify (i.e., unverifiable details; G. Nahari, Leal, et al., 2014). For example, a liar might include information such as "This afternoon, I was walking in the park, alone." While the first example is easy to verify because it is possible to check if and when the email was sent,

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the second example is not because it mentions no identifiable people who can corroborate the statement.

Three dependent variables are used in VA research: The number of verifiable details, the number of unverifiable details, and a ratio score. The ratio score combines the first two dependent variables. In original work the ratio score was calculated as (verifiable details/unverifiable details; G. Nahari, Vrij, & Fisher, 2014a). In later studies, the ratio score was typically calculated as (verifiable details/total details), with total details being the sum of verifiable + unverifiable details (A. Vrij & Nahari, 2019). The ratio score can range from 0 to 1 with scores above .50 meaning that the statement contained more verifiable than unverifiable details. As the truth is expected to be associated with more verifiable than lies, and lies with more unverifiable than the truth, the ratio score can be expected to be most diagnostic of veracity (A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017).

The decision whether a detail is verifiable is made based on the following checklist (taken from A. Vrij & Nahari, 2019). A verifiable detail includes perceptual or contextual information which can be potentially checked, defined as (1) activities that were carried out with identifiable or named persons who the interview can consult ("I went to a restaurant with my sister"), (2) activities that have been witnessed by identifiable or named persons who the interviewer can consult ("When I left for work, my neighbour was taking out the mail"), (3) activities that the interviewee believes may have been captured on CCTV (I withdrew cash from the ATM around 8 a.m., there was a camera) and (4) activities that may have been documented in any other way ("I called my friend in the afternoon"). All other details are categorized as unverifiable details.

Qualitative reviews of the VA literature have noted that the results for the three dependent variables vary greatly across studies (A. Vrij & Nahari, 2019). A vote count in Bogaard et al. (2020) showed that, for verifiable details, out of 20 effect size estimates, seven showed a null effect, four a small effect, two a moderate effect, and seven a large effect. For unverifiable details, out of nine effect size estimates, three showed a null effect, two a moderate effect, and four a large effect. Note that for unverifiable details no estimate was reported in approximately half of the cases (11/20). For the ratio score, out of 16 effect size estimates, four showed a null effect, four a small effect, two a moderate effect, and six a large effect. At the time of writing, there were no published quantitative reviews of the VA literature available. We therefore decided to conduct a meta-analysis on the three key dependent variables used in VA-research.

Apart from estimating the average effect size, we try to capture the observed variance between studies. Several moderators have been proposed to explain variance between studies, and have been argued to be augmenting or even necessary factors to obtain lie-truth differences with VA. First, VA-studies differ in the level of information participants are provided with before giving a statement. In some studies, participants are informed that the verifiability of the provided details will be checked, stressing the importance of including verifiable details in their account. This type of instruction is referred to as the information protocol (IP; G. Nahari, Vrij, & Fisher, 2014b). It is

expected that such an instruction increases the distinction in reporting verifiable details between truth tellers and liars (A. Vrij & Nahari, 2019), as was indeed found in G. Nahari, Vrij, and Fisher (2014a). Hence, we coded whether or not participants were informed that the verifiability of the details may be checked (i.e., used the information protocol or not).

As a second moderator, we included "scenario." The first study (G. Nahari, Vrij, & Fisher, 2014a) used alibi statements and found large effects for verifiable details and the verifiable non-verifiable ratio. In subsequent studies, the alibi scenario also yielded positive results (A. C. Harvey, Vrij, Sarikas, et al., 2018; G. Nahari & Vrij, 2014; G. Nahari, Vrij, & Fisher, 2014b; Vernham et al., 2020). Nahari (G. Nahari, 2018; G. Nahari, Leal, et al., 2014) argued that the alibi scenario is ideally suited for VA because an interviewee's statement is limited to the specific time and location the crime was committed. Due to these limitations, there are less opportunities for liars to embed their lie within truthful details, a strategy that is commonly used by liars (Leins et al., 2013). Nonetheless, researchers have expanded VA to a wide range of other scenarios including insurance claims (A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017; G. Nahari, Leal, et al., 2014; A. Vrij, Nahari, et al., 2016), autobiographical memories (Bogaard et al., 2020), feigning symptoms of an illness (Akca et al., 2020; I. Boskovic, Bogaard, et al., 2017; I. Boskovic, Dibbets, et al., 2019; I. Boskovic, Gallardo, et al., 2019), statements about one's occupation (L. Jupe, Vrij, et al., 2016), and future travel plans (L. M. Jupe, Leal, et al., 2017). Not all of these scenarios may be equally suitable for VA because the effectiveness of the VA "depends on the possibility for truth tellers to provide verifiable details and for liars to fool investigators" (A. Vrij & Nahari, 2019, p. 119). For example, the original authors especially doubt the usefulness of the VA approach in a malingering setting, arguing that even for truth tellers it might be difficult to report verifiable details. We therefore coded whether studies used the alibi scenario or other scenarios.

We explored three other moderators: incentives, experimental design, and publication status. Incentives have been argued to be critical for lie-truth differences in verbal behavior (Porter & ten Brinke, 2010) and is also theorized to be of importance for VA (A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017; A. Vrij & Nahari, 2019). We coded whether or not incentives were offered for providing a credible statement. The quality of the experimental design impacts the confidence that the (VA) outcome can be attributed to the (veracity) manipulation, hence the precision of the effect size estimates (Borenstein et al., 2009). Hence, we coded whether studies used an experimental or a quasi-experimental design. Finally, to assess publication bias, we coded whether studies were published (including studies that were accepted for publication) or unpublished.

To estimate to what extent the Verifiability Approach helps to establish the veracity of a statement, we conducted a meta-analysis on the three dependent measures, examining (1) the number of verifiable details, (2) the number of unverifiable details, and (3) the ratio score. Specifically, we tested three predictions. First, whether truth tellers provide more verifiable details than liars, which is the main

prediction resulting from the VA. Second, whether liars include more unverifiable details than truth tellers (A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017; G. Nahari & Vrij, 2014). In their most recent reviews, the original authors argue that predictions about unverifiable details may not be that straightforward and may depend on level of motivation, with only motivated liars expected to provide unverifiable details in their account (A. Vrij & Nahari, 2019). Despite these recent insights, many researchers have derived the prediction from VA that liars will include more unverifiable details than truth tellers (Bogaard et al., 2020; A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017; I. Boskovic, Bogaard, et al., 2017; Körner and Urban, 2018; G. Nahari, Vrij, & Fisher, 2014a; Vernham et al., 2020; Verschuere et al., 2020). Third, a ratio score is expected to capture both effects, and truth tellers are predicted to provide statements that hold a larger number of verifiable details (relative to total number of details) compared to liars.

2 | METHOD

2.1 | Literature search

A recent review (Bogaard et al., 2020, Table 1) summarized published studies that examined lie-truth differences for verifiable details, unverifiable details, and the ratio score. This review summarized the results of 13 papers containing 20 effect sizes. We expanded this database by (a) inspecting the reference list of other recent reviews of the Verifiability Approach (G. Nahari, 2018; A. Vrij & Nahari, 2019) [0 additional papers identified], (b) scanning available conference programs of two legal psychology conferences (EAPL and APLS) in the years 2014–2020 with the search terms “verifiability approach” OR

“verifiable detail*” [0 additional papers identified], (c) launching a call for studies on social media (Twitter, ResearchGate) [1 additional paper identified], (d) inspecting the 36 abstracts obtained through a Web of Science search with the search terms “verifiability approach” OR “verifiable detail*” [2 additional papers identified], (e) scanning our own (unpublished) work [2 additional paper identified], and (f) emailing all first authors of identified papers whether they had additional studies not yet in our database [2 additional paper identified]. In total we identified seven additional papers, resulting in a total of 20 papers containing 30 effect size estimates, of which two were excluded (L. Jupe, Vrij, et al., 2016; L. M. Jupe, Leal, et al., 2017, see below). Thus, the final database for the meta-analysis consisted of 18 papers containing 28 effect size estimates.

2.2 | Study inclusion

Studies were included if they:

1. Were published in English or one of the authors could assist us in determining study inclusion, coding of relevant (e.g., moderators) variables, and provide necessary data.
2. Used a (quasi-) experimental design reporting on original data.
3. Enabled a between-subject comparison of deceptive versus truthful.
4. Involved participants who provided a statement on a past event that they claim to have experienced.
5. Statements were coded blind to veracity condition on the number of (perceptual and contextual) details in the statements and coded whether those details were verifiable (i.e., documented, recorded, executed or witnessed by an identifiable other).

Two studies used a within-subjects design (Bogaard et al., 2020; Boskovic et al., 2020), but allowed for a between-subject comparison by only including the first statement. When papers reported on several between subject conditions, they were included as separate studies when the condition was hypothesized to affect the variables of interest (e.g., with or without IP; open or closed questioning procedure; with or without model statement), otherwise they were merged (e.g., across different types of deception; I. Boskovic, Dobbins, et al., 2019; I. Boskovic, Gallardo, et al., 2019). For the study that interviewed pairs of participants collectively versus individually (Vernham et al., 2020), we only included the individually delivered statement. For Verigin et al. (2020) we included only the condition in which participants reported their actions over the 2-h window. Two studies were excluded either because participants made a statement on a future event (L. M. Jupe, Leal, et al., 2017) or the study did not allow for between-study comparison (L. Jupe, Vrij, et al., 2016).

We extracted the necessary information from the study reports. If the report did not mention all necessary details, we approached the corresponding author of the paper. All authors responded to our request, though not all could provide us with the requested data, for example because the variable of interest was not coded, or the data

TABLE 1 Moderator analysis for verifiable details

	<i>k</i>	<i>d</i> (with 95% CI)	<i>Z</i>	<i>p</i>
Information Protocol (IP)				
With IP	10	0.76 (0.42, 1.10)	4.37	<.001
Without IP	18	0.34 (0.03, 0.65)	2.16	.03
Scenario				
Alibi	11	0.94 (0.64, 1.25)	6.04	<.001
All other	17	0.19 (−0.06, 0.43)	1.51	.13
Incentives				
Incentivized	14	0.36 (0.05, 0.68)	2.26	.02
Non-incentivized	14	0.62 (0.27, 0.96)	3.50	<.001
Design				
Experimental	14	0.72 (0.39, 1.05)	4.30	<.001
Quasi-experimental	14	0.25 (−0.04, 0.54)	1.71	.09
Publication status				
Published	23	0.59 (0.34, 0.85)	4.55	<.001
Unpublished	5	0.00 (−0.36, 0.37)	0.03	.98

Note: *k* denotes the number of conditions. *Z* and corresponding *p*-value refer to the difference from 0.

was no longer available. For verifiable details, we obtained the necessary data for all 28 included conditions. For unverifiable details, data were missing for eight of the included conditions. For the ratio score, data were missing for two of the included conditions.

2.3 | Meta-analytic procedure

There were three dependent variables: the number of verifiable details, the number of unverifiable details, and the ratio score (number of verifiable details/total number of details). Of note, the ratio score is not independent from, but rather a function of the other two dependent variables. Because the maximum possible number of details varies greatly between studies, we standardized the dependent variables to Cohen's *d* scores (using formulas 4.18–2.22 in Borenstein et al., 2009). Cohen's *d* expresses how many standard deviation units

liars and truth tellers differ in the three dependent variables. A useful source for the interpretation of Cohen's *d* is <https://rpsychologist.com/d3/cohend/>. For instance, with a Cohen's *d* of 0.5, 69% of the truth tellers will be above the mean of the liars, 81% of the two groups will overlap, and there is a 64% chance that a randomly chosen truth teller will have a higher score than a randomly chosen liar.

Given we expected real differences between study effect sizes, we chose for the random-effect size model rather than the fixed-effect size model. All analyses were conducted with the JASP 0.12.2.0 Meta-Analysis module (JASP Team, 2020). Because JASP is user-friendly and freely available (<https://jasp-stats.org/>), our analyses can be easily reproduced. For moderation analysis, we considered Information Protocol (Yes, No), Scenario (Alibi, Other), Incentives (Yes, No), Experimental design (Experimental; Quasi-Experimental), and Publication status (published, unpublished).

FIGURE 1 Forest plot of the 28 estimated effects of the difference in number of verifiable details between truthful and deceptive statements, sorted in descending order of effect size. The error bars represent 95% confidence intervals, and the size of each square represents the magnitude of the standard error for the effect (larger squares indicate less variability in the estimate). The diamond at the bottom represents the outcome of the random effect meta-analysis, with its 95% confidence interval

3 | RESULTS

The data to reproduce the analyses can be found on <https://osf.io/zpck4/>

3.1 | Do truthful statements hold more verifiable details than deceptive statements?

Across $k = 28$ conditions, encompassing 813 truthful statements and 991 deceptive statements, the average observed difference in verifiable details was significant, $Z = 4.10$, $p < .001$, see Figure 1. The effect was moderate in size, $d = 0.49$, with the 95% Confidence Interval ranging from 0.25 to 0.72.

As could be expected from the qualitative reviews, the observed effect varied considerably from study to study. The Q statistic indicated that the observed variance was larger than expected by sampling error alone, $Q(27) = 155.95$, $p < .001$. I^2 was 81.89, 95% CI [70.53; 90.32], indicating a sizable portion of the observed variance in effect sizes can be attributed to real differences between the studies (Higgins & Thompson, 2002). To try to explain the variance in effect sizes, we computed a meta-regression model with five moderators (Information protocol, Scenario, Incentives, Experimental Design, Publication Status). Particularly *scenario* significantly predicted the difference between truthful and deceptive statements, $Z = -3.48$, $p < .01$, see Table 1. The effect for the 11 studies using an Alibi scenario was large, and it was non-significant for the 17 studies using another scenario. *Incentives* also showed a significant effect, $Z = 2.48$, $p = .01$, with the 14 incentivized studies showing a smaller and non-significant difference in verifiable details than the 14 non-incentivized studies. No significant effect was found for the other moderators (Experimental design, $Z = -0.48$, $p = .63$; Information Protocol, $Z = 1.10$, $p = .27$; Publication Status, $Z = -0.30$, $p = .77$). After inclusion of scenario and incentives as moderators the remaining unexplained variance was reduced, but still substantial ($I^2 = 56.83$, 95% CI [29.28; 78.67]).

Egger's test pointed towards funnel plot asymmetry, $Z = 2.05$, $p = .04$. A possible reason for funnel plot asymmetry is publication bias. The results of this test should be interpreted with caution, not only because p was close to .05, but mainly because the large heterogeneity may erroneously point towards publication bias (Ioannidis & Trikalinos, 2007). Visual inspection of the funnel plot did not point towards an overrepresentation of imprecise studies with large effect sizes (i.e., in the bottom right of Figure 2).

3.2 | Do deceptive statements hold more unverifiable details than truthful statements?

Across $k = 20$ conditions, encompassing 601 truthful statement and 745 deceptive statements, the average observed difference in unverifiable details was, while numerically in the predicted direction, not significant, $Z = 1.84$, $p = .07$, $d = -0.31$, 95% CI [-0.02, 0.64], see Figure 3.

FIGURE 2 Funnel plot for the meta-analysis on verifiable details

The observed effect varied considerably from study to study, $Q(19) = 112.04$, $p < .001$. I^2 was 88.12, 95% CI [79.60; 95.05], indicating a sizable portion of the observed variance in effect sizes can be attributed to real differences between the studies. To try to explain the variance in effect sizes, we computed a meta-regression model, see Table 2. None of the moderators showed a significant effect (Information Protocol, $Z = -0.48$, $p = .63$; Incentives, $Z = -0.03$; $p = .98$; Experimental design, $Z = 0.11$, $p = .91$; Publication Status, $Z = 0.60$; $p = .54$; Scenario, $Z = -1.70$; $p = .09$), and there remained unexplained excess variance.

Egger's test showed no evidence for funnel plot asymmetry, $Z = -1.31$, $p = .19$, see Figure 4. This test should be interpreted with caution because $I^2 > 50\%$ (Ioannidis & Trikalinos, 2007).

3.3 | Ratio score: Do truthful statements hold relatively more verifiable than unverifiable details compared to deceptive statements?

Across $k = 26$ conditions, encompassing 726 truthful statements and 867 deceptive statements, the average observed difference in the ratio score was significant, $d = 0.49$ (95% CI: 0.25, 0.74), $Z = 3.94$, $p < .001$, see Figure 5.

The observed effect varies considerably from study to study, $Q(25) = 129.91$, $p < .001$. I^2 is 81.59 [95% CI: 70.49, 91.40], indicating that a sizable portion of the observed variance in effect sizes can be attributed to real differences between the studies, see Table 3. None of the moderators were significant (Information Protocol, $Z = 0.91$, $p = .36$; Incentives, $Z = 1.38$, $p = .17$; Experimental design, $Z = -0.12$, $p = .90$; Publication Status, $Z = -0.33$, $p = .74$; Scenario, $Z = -0.79$, $p = .43$), and there remained unexplained excess variance.

FIGURE 3 Forest plot of the 20 estimated effects of the difference in number of unverifiable details between truthful and deceptive statements, sorted in ascending order of effect size. The error bars represent 95% confidence intervals, and the size of each square represents the magnitude of the standard error for the effect (larger squares indicate less variability in the estimate). The diamond at the bottom represents the outcome of the random effect meta-analysis with its 95% confidence interval

Egger's test pointed towards funnel plot asymmetry, $Z = 3.50$, $p < .001$, but this test should be interpreted with caution because $I^2 > 50\%$ (Ioannidis & Trikalinos, 2007). A possible reason for funnel plot asymmetry is publication bias. Visual inspection of the funnel plot did not point towards an overrepresentation of imprecise studies with large effect sizes (i.e., in the bottom right of Figure 6).

4 | MODERATOR DEPENDENCY ANALYSES

We assessed dependencies between our categorical moderators (Lipsey, 2003). Cramer's V expresses the strength of the association, varying from 0 to 1, with 0.10, 0.30, and 0.50 having been suggested as benchmarks for small, moderate, and large effects (Cohen, 1988). Most of the dependencies were small to moderate (i.e., 0.03–0.33).

There were two notable exceptions. There was a significant association between incentives and publication status, $V = 0.495$, $p = .01$, indicating published studies (8/22 or 36%) used incentives less often than unpublished studies (5/5 or 100%). There was also a significant association between scenario and experimental design, $V = 0.739$, $p < .001$, with alibi scenario studies (10/10 or 100%) more often using an experimental design than studies using another scenario (4/17 or 23%).

5 | ADDENDUM. BAYESIAN META ANALYSES

For verifiable details and the ratio score, the average meta-analytic effect was significant, p 's $< .001$. For unverifiable details, the p value was .07, that is, there was no statistically significant effect. To try and discriminate

TABLE 2 Moderator analysis for unverifiable details

	<i>k</i>	<i>d</i> (with 95% CI)	<i>Z</i>	<i>p</i>
Information Protocol (IP)				
With IP	6	−0.34 (−1.25, 0.57)	−0.74	.46
Without IP	14	−0.30 (−0.61, 0.00)	−1.93	.05
Scenario				
Alibi	5	0.40 (−0.14, 0.94)	1.46	.14
All other	15	−0.54 (−0.87, −0.21)	−3.22	<.01
Incentives				
Incentivized	11	−0.03 (−0.43, 0.36)	−0.17	.87
Non-incentivized	9	−0.65 (−1.15, −0.15)	−2.55	.01
Design				
Experimental	9	−0.01 (−0.48, 0.45)	−0.05	.96
Quasi-experimental	11	−0.55 (−0.99, −0.12)	−2.49	.01
Publication status				
Published	15	−0.37 (−0.79, 0.05)	−1.74	.08
Unpublished	5	−0.12 (−0.59, 0.34)	−0.52	.60

Note: *k* denotes the number of conditions. *Z* and corresponding *p*-value refer to the difference from 0.

FIGURE 4 Funnel plot for the meta-analysis on non-verifiable details

absence of evidence from evidence of absence (Gronau et al., 2020), we conducted a Bayesian model-averaged meta-analysis for the three dependent variables. We followed the guidelines by Gronau et al. (2020) and used JASP 0.12.2 default settings (i.e., an Inverse-Gamma (1, 0.15) heterogeneity prior, and a Cauchy (0, .707) effect size prior). For verifiable details, the data were 93.28 times more likely under the hypothesis of a lie-truth difference than under the null hypothesis of no difference. For the ratio score the data were 81.49 times more likely under the

hypothesis of a lie-truth difference than under the null hypothesis of no difference. For unverifiable details, the data were not diagnostic ($BF_{01} = 1.12$) and about equally likely under the hypothesis of a difference than under the null hypothesis of no difference.

6 | GENERAL DISCUSSION

Research has yielded theoretical and empirical evidence to suggest that truthful statements are richer in detail than deceptive statements. The Verifiability Approach extends this research, suggesting it matters whether or not those details can potentially be verified. The current study tested three predictions that have been derived from the Verifiability Approach. In line with this approach, we found that truth tellers provided more verifiable details than liars. We failed to find support for the second prediction: Liars did not include more unverifiable details than truth tellers. Our findings regarding the ratio score of verifiable details were in line with the VA predictions: Truthful statements included a higher proportion of verifiable details compared to deceptive statements.

The original authors of the Verifiability Approach argued that the effectiveness of the approach depends on several factors (A. Vrij & Nahari, 2019). First, they argued that the criminal setting – where the statement is the suspect's alibi – is most suitable for VA. This is because an interviewee's statement has a restricted time and location framing (G. Nahari, 2018; G. Nahari, Leal, et al., 2014), and these restrictions limit liars' use of embedded lies, making it more difficult for liars to provide a detail rich account (G. Nahari, 2018). Second, they argued that prompting the interviewees to provide details that can be checked enhances the effect of the approach (A. Vrij & Nahari, 2019). Our moderator analyses suggest that the effect of verifiable detail on truth-lie differences is indeed larger for the alibi scenario than for the other scenarios, but we did not find evidence for the prediction that the Information Protocol would benefit the effectiveness of the Verifiability Approach (G. Nahari, 2018; A. Vrij & Nahari, 2019). Our moderator analysis did show that using incentives decreased the effect of verifiable details on lie-truth differences. This finding is in line with C. F. Bond and DePaulo (2008) who showed that an incentive to appear credible makes lies more difficult to detect. Whether the study design was experimental or quasi-experimental, and whether or not the study was published did not moderate the effect. Importantly, studies varied greatly in the reported effects for unverifiable details. Yet, none of our moderators – scenario, information protocol, design, incentives, publication status – was able to explain all this variance. The Bayesian analysis indicated that the non-significant effect for unverifiable details should be interpreted as absence of evidence rather than evidence of absence. This calls for more and better studies assessing whether and when lie-truth differences in unverifiable details emerge.

Overall, we found a Cohen's *d* of 0.49 for verifiable details. Given the lack of a significant lie-truth effect for unverifiable details, it is not surprising the ratio score did not outperform the verifiable detail effect, showing a similar, moderate effect of *d* = 0.49. Such a

FIGURE 5 Forest plot of the 26 estimated effects of the difference in the ratio score between truthful and deceptive statements, in descending order of effect size. The error bars represent 95% confidence intervals, and the size of each square represents the magnitude of the standard error for the effect (larger squares indicate less variability in the estimate). The diamond at the bottom represents the outcome of the random effect meta-analysis with its 95% confidence interval

moderate effect size is comparable to that of the criterion “quantity of details” (Steller & Köhnken, 1989). Meta-analytic estimates for the effect size of this criterion range from $d = 0.55$ (Amado et al., 2016) to $d = 0.30$ (C. F. Bond & DePaulo, 2008; Luke, 2019). These estimates are well within the lower bound estimate of the 95% CI (0.25) found for verifiable details in this meta-analysis. This raises the question of whether there is added value to distinguishing total details in verifiable versus non-verifiable details. The original authors argued that there are unique advantages to the Verifiability Approach beyond accuracy, including that it is not affected by the interviewee's knowledge of its rationale and is therefore less susceptible to countermeasures. In fact, informing interviewees about the approach would enhance its effect. Our meta-analysis found no support for this latter prediction. Moreover, previous research has shown deception cues are sensitive to the decline effect. That is, with more published studies the estimate of the overall effect becomes smaller (Bond et al., 2015). The

effect for (total) details may be more robust than the effect of verifiable details, as the current evidence base is larger and more diverse.

Is the Verifiability Approach ready to be used for lie detection in practice? The effect size for verifiable details is moderate overall, and large for the alibi scenario. At minimum for the alibi scenario, this seems promising. Three considerations are of relevance. First, it remains to be seen to what extent the results of well controlled laboratory studies generalize to the field. In its seminal report on the validity of the polygraph-based lie detection tests, the National Research Council (2003) noted that laboratory studies tend to overestimate validity. The significant moderation of incentives in this meta-analysis, for example, lends reason to believe that accuracy is lower in field settings, where incentives are obviously more pronounced. Secondly, the vast majority of conditions included in our meta-analyses were co-authored by one or both of the original authors (Nahari and/or Vrij). Specifically, for verifiable details 10 out of the 11 alibi scenario

TABLE 3 Moderator analysis for the ratio score

	<i>k</i>	<i>d</i> (with 95% CI)	<i>Z</i>	<i>p</i>
Information Protocol (IP)				
With IP	10	0.24 (0.30, 1.25)	3.21	<.01
Without IP	16	0.33 (0.07, 0.60)	2.47	.01
Scenario				
Alibi	9	0.65 (0.25, 1.05)	3.19	<.01
All other	17	0.41 (0.10, 0.72)	2.61	<.01
Incentives				
Incentivized	13	0.28 (−0.03, 0.58)	1.79	.07
Non-incentivized	13	0.71 (0.35, 1.08)	3.80	<.01
Design				
Experimental	13	0.52 (0.25, 0.80)	3.74	<.01
Quasi-experimental	13	0.46 (0.05, 0.87)	2.19	.03
Publication status				
Published	21	0.59 (0.30, 0.88)	4.04	<.001
Unpublished	5	0.34 (−0.13, 0.38)	0.95	.34

Note: *k* denotes the number of conditions. *Z* and corresponding *p*-value refer to the difference from 0.

FIGURE 6 Funnel plot for the ratio score

conditions included Nahari and/or Vrij as a co-author. The only independent evaluation found an effect size of $d = 0.50$, which is more in line with the average effect obtained in our meta-analysis. Such a lack of independent replication is problematic as it may inflate false positives errors. Research on violent risk assessment tools, for example, showed that studies authored by tool designers reported predictive validity findings around two times higher than those of investigations reported by independent authors (Singh et al., 2013). Notably, the

G. Nahari and Vrij (2014) reported an effect size ($d = 2.03$) that is much larger than the average meta-analytic effect. A third critical consideration is that with an average of 32.2 participants per condition ($SD = 18.6$), the sample sizes of most primary studies was modest at best. To detect an effect of $d = 0.49$ in a between-subjects design, a minimum of $N = 146$ is needed to reach 90% power. Only three conditions met this minimum of 72 participants per condition. As shown by Kleinberg et al. (2019), and Luke (2019), underpowered studies are also associated with an increased risk for false positive outcomes (see also Simmons et al., 2011).

Some limitations of our meta-analysis deserve attention. First, this meta-analysis was not pre-registered. This means that many decisions were made based on the available data leading to a large researchers' degrees of freedom. To be completely transparent, we reported all decisions made, including inclusion and exclusion criteria, and moderator analyses. Second, the results of our moderation analysis should be interpreted with caution. The number of available studies was limited, meaning our power to detect a given moderating variable relationship was also low (Hunter & Schmidt, 1996), as was the power to detect publication bias or the lack thereof. Moreover, many moderators were unevenly distributed, resulting in potentially confounded results. For instance, alibi scenario studies more often used an experimental (rather than a quasi-experimental) design than other scenario studies. It is possible that the larger average effect for the alibi scenario studies is not (only) due to the use of the alibi scenario but can be partially explained by the use of a method of higher quality. Third, the quality of a meta-analysis is largely determined by the quality of the studies included in the meta-analysis. The vast majority of the included VA-studies have not been pre-registered, and other open science practices such as sharing of data, materials, and code also remain rare. And while all studies use VA coding, there exists no certified coding.

Studies typically report high inter-rater reliability for VA coding. But we caution that such within-laboratory reliability does not guarantee that different laboratories code similarly (Nahari et al., 2019). Future research would benefit from a standardized coding scheme, and training tools (e.g., webinar, code book, fully coded example statements) to master VA coding. This would not only improve the between-laboratory reliability of the coding but would allow for scaling-up VA research to larger samples and multi-lab studies. For meta-analytic purposes, it would be helpful if future studies would systematically report on all effects, including a benchmark effect such as total details and/or make these data publicly available. Finally, the field would benefit from (more studies) experimentally manipulating conditions that were included as moderators in this meta-analysis. To the best of our knowledge, no study has directly tested the impact of scenario or incentives. Moreover, because our meta-analysis found no support for the presumed benefits of the information protocol (A. C. Harvey, Vrij, Leal, et al., 2017; A. C. Harvey, Vrij, Nahari, & Ludwig, 2017; G. Nahari, Leal, et al., 2014), a renewed inquiry of the information protocol is needed.

In sum, we found that truthful statements contain significantly more verifiable details and included a higher proportion of verifiable

details than deceptive statements. Deceptive statements did not include more unverifiable details than truthful ones. The effect sizes for verifiable details and for proportion of verifiable details was moderate. Although the moderator analysis indicated that the effect for verifiable details was substantially larger for the alibi scenario, the lack of independent replication and low sample sizes qualify these findings.

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CONFLICT OF INTERESTS

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

The data to reproduce the analyses can be found on <https://osf.io/zpck4/>.

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