A Meta-Analysis of Test Scores in Proctored and Unproctored Ability Assessments

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Abstract

Unproctored, web-based assessments are frequently compromised by a lack of control over the participants' test taking behavior. It is likely that participants cheat if personal consequences are high. This meta-analysis summarizes findings on context effects in unproctored and proctored ability assessments and examines mean score differences and correlations between both assessment contexts. As potential moderators, we consider (a) the perceived consequences of the assessment, (b) countermeasures against cheating, (c) the susceptibility to cheating of the measure itself, and (d) the use of different test media. For standardized mean differences, a three-level random-effects meta-analysis based on 108 effect sizes from 49 studies (total N = 100,434) identified a pooled effect of $\Delta = 0.20$, 95% CI [0.10, 0.31], indicating higher scores in unproctored assessments. Moderator analyses revealed significantly smaller effects for measures that are difficult to research on the Internet. Regarding rank order stability, a small subsample of studies (n = 5) providing 15 effect sizes (total N = 1,280) indicated considerable rank order changes ($\rho = .58$, 95% CI [.38, .78]). These results demonstrate that unproctored ability assessments are markedly biased by cheating. Unproctored assessments may be most suitable for tasks that are difficult to search on the Internet.

A Meta-Analysis of Test Scores in Proctored and Unproctored Ability Assessment

Recent technological developments changed the way researchers collect psychological data in general (Miller, 2012) and conduct psychological assessments in particular (Harari et al., 2016). Gathering data outside the laboratory in an unproctored setting, for example, using mobile devices or web-based tests serves as an ecologically valid (Fahrenberg, Myrtek, Pawlik, & Perrez, 2007) and economic method (Buhrmester, Kwang, & Gosling, 2011) to collect psychological data on large, heterogeneous samples (Gosling, Sandy, John, & Potter, 2010). Therefore, unproctored, web-based testing has become the dominant assessment mode in market and public opinion research (Evans & Mathur, 2005) and is similar popular in the academic realm (Allen & Seaman, 2014) or in personnel selection (Lievens & Harris, 2003; Tippins, 2011). The advantages of unproctored testing, however, come at a cost: the lack of supervision results in less standardized test taking conditions and less control over test-takers' behavior (Wilhelm & McKnight, 2002). Therefore, the question arises if the opportunity for dishonest behaviors in unproctored assessments leads to biased scores and threatens the usefulness of these tests in applied settings (Rovai, 2000; Tippins et al., 2006). To this end, a meta-analysis is presented that compares scores from proctored and unproctored ability tests across assessment contexts and examines potential moderating influences thereon.

Mode Effects in Ability Assessments

While scores of self-report instruments can be considered equivalent for proctored and unproctored testing (Gnambs & Kaspar, 2017), respective results for tests of maximal performance are rather inconclusive (Do, 2009): Some studies found no systematical differences between self-selected web samples and traditional lab samples (e.g., Ihme et al., 2009), whereas others reported significantly higher scores for unproctored tests (e.g., Carstairs & Myors, 2009) or, occasionally, for proctored tests (e.g., Coyne, Warszta, Beadle, & Sheehan, 2005). Inconsistent results were also reported for the prevalence of cheating:

Some studies found low cheating rates varying from below 2.5% (Lievens & Burke, 2011; Nye, Do, Drasgow, & Fine, 2008) up to 7.0% (Tendeiro, Meijer, Schakel, & Maij-de Meij, 2013). Conversely, in an online survey, every fourth participants reported cheating on knowledge task without being offered performance-dependent incentives (Jensen & Thomsen, 2014).

One reason for the heterogeneous results are the varying settings that unproctored assessments were administered in (Reynolds, Wasko, Sinar, Raymark, & Jones, 2009), such as personnel selection (Bartram, 2006; Tippins, 2009), educational contexts (Allen & Seaman, 2014), and research contexts, in which the feasibility, equivalence, and validity of web-based assessments are examined (e.g., Jensen & Thomsen, 2014; Wilhelm & McKnight, 2002). These settings differ in the perceived consequences of assessment, the countermeasures that are taken to prevent cheating, and the measured cognitive domain. In industrial and organizational (I/O) psychology, ability testing often takes place in high-stakes settings with hiring decisions linked to the individual test results. Thus, the test-takers have a strong motivation to achieve high scores to increase their chances of employment. To maximize the benefits for both applicants and employers (Gibby, Ispas, Mccloy, & Biga, 2009), countermeasures against cheating are implemented to discourage participants from faking their test scores in recruitment procedures. In educational assessments, online placement tests or exams are most commonly knowledge tests that are tailored to students' specific knowledge acquired in university courses. In a research context, however, test-takers' performance in unproctored assessments usually have no severe consequences, thus, participants are expected to cheat less (Do, 2009). In contrast to the applied contexts, a wide range of different measures are examined, such as reasoning tests (e.g., Preckel & Thiemann, 2003), perception tasks (e.g., Williamson, Williamson, & Hinze, 2016), and knowledge tests (e.g., Jensen & Thomsen, 2014). Accordingly, the current meta-analysis investigates whether there are systematic score differences in proctored and unproctored ability assessments depending on the aforementioned differences in the test environment.

Research Questions

The aim of this meta-analysis was to investigate to what extent a lack of supervision undermines psychological assessment of cognitive abilities. Given that unproctored assessment procedures are on the rise (Gosling & Mason, 2015), it is crucial to know whether the mode of test administration influences test scores. Our outcome variables are standardized mean differences and correlations between proctored and unproctored ability assessments, respectively. We take into account all test situations without a human supervisor present (see also Tippins, 2009). Accordingly, a setting is proctored if a human supervisor is present or remotely proctored if the testing is supervised via web-cam. Additionally, this meta-analysis considers various moderators to explain the heterogeneous findings reported in the literature.

First, test-takers' cheating motivation can be influenced by the perceived consequences of a test result. If participants anticipate severe consequences such as hiring or university admission, they are most likely more motivated to cheat. Therefore, proctored assessments are still viewed as the gold standard in high-stakes testing (Rovai, 2000). Do (2009) hypothesized that cheating is not as prevalent in low-stakes contexts, even though previous results point in a different direction (Jensen & Thomsen, 2014). We expect that in case important consequences are directly linked to the participant's performance, test-takers might be more likely to cheat. Conversely, test-takers are presumably less motivated to cheat if no consequences are linked to the test results. Thus, we expect higher score differences in high-stakes settings (*Hypothesis 1*).

Second, test administrators can implement countermeasures that overcome participants' motivation to cheat. Especially in high-stakes test contexts, administrators are advised to use honesty contracts or follow-up verification tests (International Test

Commission, 2006). Honesty contracts include explicit policies and negative consequences of fraudulent test taking practices. Usually, such honesty contracts are presented to the test-taker prior to the testing and must be signed to indicate commitment. Verification tests are proctored follow-up tests that help to identify participants with aberrant test scores (Guo & Drasgow, 2010; Tendeiro et al., 2013). To work as a countermeasure designed to lower the test-takers' motivation to cheat, it is important to inform test-takers about the follow-up tests in advance. These procedures are often used in personnel selection (Lievens & Burke, 2011; Nye et al., 2008). In academic settings, institutions often implement honor codes not only to raise students' awareness of cheating, but also to call attention to the consequences linked to unethical behavior (McCabe & Treviño, 2002; O'Neill & Pfeiffer, 2012). Furthermore, other researchers suggested the use of specific instructions to reduce cheating that can contain the note that test results, or feedback, are only valid if the test-taker does not cheat (e.g., Wilhelm & McKnight, 2002). These precautions are intended to lower participants' cheating motivation, thus should result in reduced score differences (*Hypothesis 2*).

Third, the measurement instrument itself can affect participants' opportunity to cheat. Diedenhofen and Musch (2017) investigated cheating in an unproctored assessment, comparing a knowledge quiz and a reasoning task. They found that participants switched between browser tabs more often when answering knowledge questions that can be looked up on the Internet. Moreover, a positive relationship between page switches and test performance was found for the knowledge task, whereas no significant relationship was found for the reasoning test. These findings are in line with other studies reporting that cheating was most effective for subtests that assess abilities such as vocabulary and numeracy, in which performance can be enhanced through the use of a web search, dictionaries, or calculators (Bloemers, Oud, & Dam, 2016). In contrast, tasks that assess fluid abilities such as reasoning are less susceptible to cheating. Therefore, score difference should be higher for tests with a high *searchability* (*Hypothesis 3*).

Lastly, a factor that can lead to test score differences is the use of cross-mode comparisons. Unproctored assessments are usually administered over the Internet and, therefore, computer-based. Most studies compared these web-based assessments to proctored, computer-based assessments (e.g., Germine et al., 2012). However, not all studies adopted identical test modes in both contexts: Some studies compared unproctored, computerized tests to proctored, paper-and-pencil assessments (e.g., Coyne et al., 2005). Although computer-based and paper-and-pencil ability assessments are considered equivalent for non-speeded measures (Mead & Drasgow, 1993), Schroeders and Wilhelm (2010) suggested differences in perceptual and motor skills as potential influencing factors. These differences, however, might lead to biased scores when proctored and unproctored assessments are compared across test media. If substantial mode differences exist, cross-mode comparisons are expected to result in larger mean differences between proctored and unproctored settings (*Hypothesis 4*).

However, the equivalence of test scores across proctored and unproctored ability assessments should not be solely based on the comparison of mean scores. From a psychometric perspective it is important to ensure that test scores are only dependent on the trait in question and independent of testing conditions. The comparability of test scores gathered in different settings should be carried out using latent variable modeling (Schroeders & Wilhelm, 2010, 2011). However, such strict psychometric procedures require raw data, which is usually not available for meta-analysis. One of the simplest statistic indexing the similarity of the test-takers' ranking across conditions are correlation coefficients (Mead & Drasgow, 1993). A low correlation indicates differences across conditions in the assessment of test-takers' ability. If examinee ranking is invariant across modes (i.e., high cross-mode correlations are obtained), mean scores can be converted using linear transformations (Green, 1991; Hofer & Green, 1985). Therefore, we additionally examine correlations between ability test scores in proctored and unproctored settings.

Method

Literature Search and Study Selection

An overview of the literature search is depicted in Figure 1 of the Electronic Supplemental Material. In total we identified 101 potentially relevant studies, searching in major scientific databases, screening reference lists, and contacting authors. Subsequently, these studies were examined regarding the following criteria to be included in the meta-analysis: (a) The study reported a comparison of test scores obtained in a (remotely) proctored setting versus an unproctored setting, (b) administered cognitive ability measures, (c) was published during the last 25 years (1992–2017), (d) was written in English, and (e) reported appropriate statistical information that allowed the calculation of an effect size. Studies only reporting latent mean scores were excluded from the analyses. Furthermore, studies were excluded from the analyses, if (a) participants were actively instructed to cheat (e.g., Bloemers et al., 2016), (b) participants underwent different training phases prior to the assessments (e.g., online vs. traditional classes), or (c) different tools and aids were allowed across testing conditions (e.g., open vs. closed book exams; Brallier & Palm, 2015; Flesch & Ostler, 2010). After applying these criteria, 50 studies were considered eligible for the meta-analysis (see Table 1 of the ESM1 for an overview of the study pool).

Coding Process

To facilitate transparency and replicability (Wilson, 2009), we developed a standardized coding protocol assessing descriptive information, effect sizes, and the moderator variables. For each study, we coded the type of publication (i.e., peer-reviewed journal, contribution to an edited book, master or doctoral thesis, conference presentations, or unpublished manuscripts), year of publication, mean age, percentage of female participants, sample type (i.e., children or adolescents up to 11th grade, college or university students, or mixed/ adult samples), the assessment context (i.e., academic research, educational, or I/O context), and research design (i.e., within- or between-subject).We extracted the sample sizes, means, and standard deviations of the ability scores in the unproctored and proctored setting as well as the correlation coefficients between test scores, and any other information that could be used to calculate an effect size (e.g., *t*-values). Moreover, we recorded whether test-takers expected consequences of the test results (such as a hiring decision or grading). If test performance yielded important consequences for the test-taker, the assessment was coded as high-stakes. To examine the usefulness of countermeasures against cheating, we coded different procedures (i.e., honesty contracts, honor codes, announcement of verification tests, instructions, or a combination of them). We also rated the proneness of the measure for cheating, that is, whether the searchability was high (e.g., for knowledge tests) or low (e.g., for figural matrices tests). Finally, we noted whether identical presentation modes (i.e., computerized or paper-and-pencil) were used in both assessment conditions.

All studies were coded twice by three independent raters. To evaluate the coding process, Cohen's (1960) κ was calculated. Intercoder agreement is considered strong for values exceeding .70 and excellent for values greater than .90 (LeBreton & Senter, 2008). The pairwise intercoder reliability ranged from .70 to .92. All discrepancies were discussed until consensus was reached.

Statistical Analyses

Calculation of effect sizes. As mean differences between scores assessed in proctored and unproctored settings were the primary topic of interest, the standardized mean difference Hedge's (1981) *g* was calculated with positive effect sizes indicating higher scores in the unproctored condition. For studies not reporting information necessary to calculate *g*, we applied transformation formulas to derive *g* from *t* values (Morris & DeShon, 2002). Studies that only reported regression weights were excluded from the analysis (Aloe, 2015). For a subsample of studies reporting within-group comparisons, we additionally pooled Pearson correlations between the two test contexts to investigate the effects of mode differences on the rank ordering of test-takers. Extreme effect sizes were identified using internally studentized residuals (Viechtbauer & Cheung, 2010). Two extreme effect sizes with standardized residuals larger than 3 (Tukey, 1977) were removed from the analyses.

Meta-analytic model. Effect sizes were pooled using a random-effects model with a restricted maximum likelihood estimator (Viechtbauer, 2005). To account for dependent effect sizes (e.g., if a study reported more than one effect size for a given sample), we conducted a three-level meta-analysis (Cheung, 2014), in which individual effect sizes are nested within samples. To account for sampling error, we used different weighting procedures for the analysis of standardized mean differences and the correlational analysis. For the analysis of standardized mean differences, each effect size was weighted by the inverse of its variance, which is superior to other weighting procedures and results in more precise estimates of the mean effect (Brannick, Yang, & Cafri, 2011; Marín-Martínez & Sánchez-Meca, 2010). Correlations were weighted using sample size weights, which is the most accurate procedure (Brannick et al., 2011). Heterogeneity in the observed effect sizes was quantified by the I^2 statistics (Higgins & Thompson, 2002), which describes the proportion of total variation in study estimates that is due to heterogeneity. Following the often applied rules of thumb, I^2 of .25, .50, and .75 indicate low, medium, and high heterogeneity, respectively (Higgins, Thompson, Deeks, & Altman, 2003). We examined moderating effects on the pooled effect size using mixed-effect regression analyses (Viechtbauer, 2010). All models were estimated using the R package *metafor* version 1.9.9 (Viechtbauer, 2010) in R version 3.3.2 (R Core Team, 2016). To make the present analyses transparent and reproducible (Nosek et al., 2015), we provide all material (i.e., coding protocol, data sheets, and syntax) online within the *Open Science Framework* (Center for Open Science, 2017): https://osf.io/xf8dq/?view_only=23571304c6844bfcbf9a15b099a5f406

Results

The meta-analysis of mean differences was based on 49 studies[1] that were published between 2001 and 2017, mainly in peer-reviewed journals (67%). Unpublished work comprised master and doctoral theses (11%), conference proceedings (19%), and unpublished reports (3%). The meta-analytic database included 65 independent samples providing 109 effect sizes, with each sample reporting between 1 and 7 effect sizes. Overall, the meta-analysis covered scores from 100,434 participants (range of samples' *n*s: 19 to 24,750). Most studies were conducted in an educational (43%) or research context (41%); fewer studies reported on I/O contexts (16%). Low-stakes settings were reported more often than high-stakes settings (62% vs. 38%). In 29% of the samples countermeasures against cheating were implemented. Approximately half of the reported effect sizes (48%) were based on highly searchable tasks. In all cases that reported cross-mode comparisons (29%) the proctored assessment was paper-and-pencil, whereas the unproctored assessment was computerized.

The subsample reporting rank order stabilities comprised 5 studies published in peer-reviewed journals between 2005 and 2009. The studies included 7 independent samples providing 15 correlations. The total sample size was 1,280 (range of the samples' *n*s: 29 to 856). The subsample covered articles from all settings described above, with three studies being conducted in a research context and one each in educational and I/O context.

Mean Score Differences between Proctored and Unproctored Assessments

The pooled mean difference between proctored and unproctored settings was $\Delta = 0.20$ ($SE = 0.05$), 95% CI [0.10; 0.31]; thus, on average, test-takers achieved slightly higher scores

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[1] One study only reported correlation coefficients and was therefore only included in the meta-analysis of rank order stability

in unproctored settings (Table 1). The between-cluster heterogeneity was $I^2 = .80$ and the within-cluster heterogeneity was $I^2 = .17$, indicating pronounced variability between samples, but negligible differences within samples. To quantify the influence of a potential publication bias, we compared effect sizes from published sources (i.e., journal articles) to effect sizes from unpublished sources (i.e., theses, conference proceedings, and unpublished manuscripts). The respective mixed-effects regression analysis identified no significant difference between effect sizes extracted from both sources, $\gamma = 0.09$, $SE = 0.11$, $p = .43$. Furthermore, funnel plot analyses (Figure 1 in ESM 2) and a rank correlation test ($\tau = .12$, $p = .07$; Begg & Mazumdar, 1994), which tests the distribution of effect sizes for asymmetry, revealed no evidence of a potential publication bias. Although the funnel plot illustrated pronounced heterogeneity of the effect sizes, this most likely reflects the effects of moderators on score differences in proctored and unproctored settings.

To quantify the influence of moderators on the pooled effect, a mixed-effects regression analysis was conducted to examine the effects of test setting, countermeasures, searchability, and test media. The correlations among the moderators varied between $r_\phi = -.18$ and $r_\phi = .44$ (Table 2), indicating negligible multicollinearity. Together, the four moderators explained about 18% of the random variance (Table 2). Searchability was the only significant moderator ($\gamma = 0.26$, $SE = 0.09$, $p < .01$); mean score differences between proctored and unproctored settings were significantly larger for tasks that could be easily solved using the Internet ($\Delta = 0.38$, $SE = 0.08$, $p < .001$) as compared to measures for which correct solutions were difficult to identify using ordinary web searches ($\Delta = 0.02$, $SE = 0.05$, $p = .66$). Moderator analyses yielded the same results when each moderator was examined individually (Table 1). No significant effects were found for the other moderator variables, suggesting that the score differences between proctored and unproctored assessments are not affected by anticipated consequences of test results, the implementation of countermeasures against cheating, or a change of test media.

Rank Order Stability between Proctored and Unproctored Assessment

We identified a pooled correlation of $\rho = .58$ ($SE = .10$), 95% CI [.38, .78] (Figure 1). This result suggested a moderate relationship between test scores obtained in proctored and unproctored assessment, indicating substantial rank order changes for the different testing conditions. The between-cluster heterogeneity was $I^2 = .80$, and the within-cluster heterogeneity was $I^2 = .12$, indicating a large variability of the pooled effect sizes between samples. As the meta-analysis of correlation coefficients was based on a small number of effects, we did not pursue further moderator analyses.

Discussion

Unproctored, web-based assessments are typically faced with highly unstandardized settings that allow limited control over the participants' test taking behavior. A pressing issue in this regard pertains to the question whether test scores from unproctored assessments can be readily compared to test scores from proctored lab sessions. Although a growing number of studies addressed score differences between proctored and unproctored settings, they reported rather inconclusive results (see also Do, 2009). Therefore, the current meta-analysis provided a comprehensive overview of the existing findings and studied various moderators of potential cross-mode differences. Overall, the meta-analysis revealed significantly higher scores on cognitive tests in unproctored settings as compared to proctored test contexts. However, with a standardized mean difference of $\Delta = 0.20$ the respective effect was rather small. Because the comparison of mean scores does not warrant conclusions about the equivalence of two measurements (AERA, APA, & NCME, 2014; Schroeders, 2009), we also analyzed correlations between scores of proctored and unproctored ability assessments for a subset of studies. This analysis showed a relationship of $\rho = .58$, indicating considerable changes in the rank order of participants. These results highlight that participants' relative standing within a group does not solely depend on their ability, but also on other factors such as their motivation or their ability to cheat. Intrinsic factors such as fear of failure (Atkinson, 1957), need for self-enhancement (Sedikides & Gregg, 2008) or impression-management (Leary & Kowalski, 1990) presumably influence the extent to which participants cheat. Even if no direct consequences are linked to the results, participants might try to establish a positive self-image (Greenwald & Breckler, 1985) and strive to boost their test scores. Thus, people with a higher tendency to control their public self might be more likely to cheat. Similar, personality facets such as the honesty-humility factor proposed by the HEXACO model might additionally explain dishonest behavior (Ashton & Lee, 2008; Lee, Ashton, & de Vries, 2005). Therefore, test scores from unproctored assessments not only reflect individual differences on the measured ability, but also distinct personality profiles associated with risk-taking and cheating. Therefore, results from studies that include proctored and unproctored ability assessments should be interpreted with caution.

In general, the effect sizes exhibited a large heterogeneity between samples. Therefore, we examined the influence of moderators on the observed score differences between proctored and unproctored ability assessments. Using a meta-regression approach, we found significant effects for the searchability of a task. If correct solutions were not easily identifiable over the Internet, mean score differences were approximately zero. This finding corroborates previous research suggesting that some tasks are more prone to cheating than others (Diedenhofen & Musch, 2017; Karim, Kaminsky, & Behrend, 2014). For instance, Bloemers and colleagues (2016) investigated cheating strategies for various subtests of a web-based cognitive ability test battery. They demonstrated that cheating was most effective for subtests that could be tampered through Internet searches, while cheating did not affect tasks that required complex reasoning. Interestingly, moderator analyses found no significant effect for score differences between proctored and unproctored settings for high and low-stakes testing. This finding does not support the prevailing assumption that cheating only corrupts high-stakes settings (Arthur, Glaze, Villado, & Taylor, 2010; Do, 2009) whereas it can be ignored in low-stakes testing. Furthermore, moderator analysis showed no significant effect for the implementation of countermeasures against cheating. Despite the vast body of research that advocates the implementation of countermeasures to improve data quality in unproctored assessments (Bartram, 2009; Bryan, Adams, & Monin, 2013; Dwight & Donovan, 2003; O'Neill & Pfeiffer, 2012), we found no empirical evidence for their effectiveness. Conversely, on a descriptive level, mean score differences appeared to be higher when countermeasures were implemented. Finally, differences in the test modes did not have a significant effect on the mean score differences. This finding is in line with previous results on the equivalence of paper-pencil and computerized ability tests (e.g., Mead & Drasgow, 1993; Schroeders & Wilhelm, 2010, 2011).

Recommendations for Unproctored and Proctored Assessment

Unproctored, web-based or mobile assessments promise a low-cost opportunity to reach large, heterogeneous, and geographically scattered samples (Fahrenberg et al., 2007; Gosling et al., 2010) and, thus, increasingly complement or even replace traditional data collection techniques. However, our results demonstrate considerable differences in the mean and variance-covariance structure between proctored and unproctored assessments. Based on our findings some words of caution are warranted if results obtained in one specific setting are to be generalized to the other. We also recommend against relying on countermeasures to overcome effects of cheating. What makes matters worse, the present data does not support the assumption that cheating is limited to high-stakes testing and can be ignored in low-stakes settings, including research contexts. Taking a pessimistic view, one might conclude that some participants will always cheat if they have the opportunity, regardless of countermeasures or anticipated consequences. On a more positive stance, participants will not cheat if they are not given the opportunity. Accordingly, a straightforward recommendation for ability assessments in unproctored settings is the development of test batteries that are limited to measures with a low searchability. In any case, administrators of unproctored assessments are encouraged to adopt *post hoc* strategies to identify potential cheaters, for example, using incidental data (Couper, 2005) such as reaction times or non-reactive behavioral data (Diedenhofen & Musch, 2017), seriousness checks (Aust, Diedenhofen, Ullrich, & Musch, 2012), or data-driven anomaly detection (Karabatsos, 2003). However, these analytical methods are no panacea, since identifying and excluding cheaters results in selective and most likely biased samples.

Limitations and Implications for Future Research

Some limitations to the present meta-analysis must be noted. First, most research on the comparability of ability scores in proctored and unproctored assessments focused on mean score differences, which do not allow drawing inferences about the equivalence of a measure. Measurement invariance is best studied with a latent variable approach (Raju, Laffitte, & Byrne, 2002; Schroeders & Wilhelm, 2011). We analyzed correlation coefficients as a proxy indicator for the equivalence of proctored and unproctored settings (Mead & Drasgow, 1993). Despite an extensive literature search, we only identified five studies that reported correlation across conditions. Therefore, we stress that the analysis is tentative and results must be interpreted with caution. Also, the correlations analyzed in the present meta-analyses were highly heterogeneous, ranging from r = .27 to r = .92, leaving open the question of potential moderator variables. Future research should also focus on the covariance structure by meta-analyzing raw data (Kaufmann, Reips, & Merki, 2016).

Second, the present research makes no inference about the extent of cheating in unproctored settings. Against the background of the data available for the study, we were able to ascertain that ability scores, on average, are higher in unproctored settings. Although dishonest behavior is one of the major concerns in unproctored settings (Tippins, 2009), the increased test scores might also be the result of reduced test anxiety, since participants might feel more comfortable if they are able to freely choose their testing environment (Stowell & Bennett, 2010). Further research might also address cheating directly by investigating appropriate means for the detection of dishonest behavior in ability tests. These measures include traditional approaches, such as scales measuring personality traits or integrity (McFarland & Ryan, 2000), or over-claiming (Bing, Kluemper, Kristl Davison, Taylor, & Novicevic, 2011), as well as data-driven approaches (Couper, 2005; Diedenhofen & Musch, 2017).

Finally, our data does not allow conclusions about groups of people that are more likely to cheat than others. We assume that individual differences in personality, moral beliefs, and social norms are predictive of cheating behavior. For example, some studies suggested culture-dependent differences in cheating behavior (Chapman & Lupton, 2004; McCabe, Feghali, & Abdallah, 2008). Future research might focus on test-takers who show large differences between an unproctored and a proctored assessment. For applied contexts, this might exert valuable diagnostic information (e.g., faking ability, Geiger, Sauter, Olderbak, & Wilhelm, 2016).

Conclusion

The presented meta-analysis identified higher mean scores for unproctored ability assessments, independent of the test setting (high- vs. low-stakes) and whether countermeasures were taken. However, mean score differences highly depended on the administered measure itself and its proneness to cheating. Mean differences were more pronounced for tasks that are easy to look up on the Internet, while no mean differences were found for other tasks. These findings, however, do not imply that unproctored ability assessments are not feasible *per se*. Based on the present meta-analysis, we recommend to carefully evaluate task characteristics when developing or choosing test instruments for an unproctored test battery. For example, the measurement of declarative knowledge seems better conducted in a proctored setting, whereas figural reasoning tasks might be comparably administered in unproctored contexts. We also caution researchers to generalize statements across test conditions and encourage test users to further examine the equivalence of proctored and unproctored ability tests with appropriate statistical methods.

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Table 1

Meta-Analysis of Mean Differences and Separate Moderator Analyses

	k_1	k_2	N	g	SD_g	Δ	SE_Δ	z	Q_M	$\sigma^2_{(2)}$	$\sigma^2_{(3)}$	$I^2_{(2)}$	$I^2_{(3)}$
Overall	109	65	100,434	0.19	0.46	0.20	0.05	3.85*		.14	.03	.80	.17
Stakes								1.62	2.64				
High	67	42	79,203	0.31	0.47	0.27	0.07	4.02*		.15	.03	.80	.18
Low	42	23	21,231	-0.01	0.35	0.09	0.08	1.06		.13	.03	.75	.18
Countermeasures								1.64	2.68				
Yes	32	19	6,518	0.33	0.49	0.35	0.11	3.13*		.18	.04	.80	.18
No	77	46	93,916	0.13	0.43	0.15	0.06	2.53*		.13	.03	.79	.17
Searchability								3.73	13.95*				
High	51	34	21,407	0.40	0.50	0.38	0.08	4.76*		.14	.06	.65	.28
Low	58	34	79,863	0.00	0.32	0.02	0.05	0.44		.05	.03	.62	.33
Modality								1.73	3.01				
Cross-mode	31	18	16,409	0.27	0.55	0.39	0.14	2.89*		.30	.02	.90	.07
Same mode	78	50	84,428	0.16	0.41	0.15	0.05	2.87*		.08	.04	.64	.33

Note. k_1 = Number of effect sizes; k_2 = Number of samples; N = Total sample size; g = Observed mean difference; Δ = Weighted standardized mean difference; SE_Δ = Standard error of Δ; $z = \Delta/SE_\Delta$; Q_M = test statistic for the omnibus test of coefficients (df = number of moderator categories − 1); $\sigma^2_{(2)}$ = between-cluster variance; $\sigma^2_{(3)}$ = within-cluster variance; $I^2_{(2)}$ = proportion of between-cluster heterogeneity; $I^2_{(3)}$ = proportion of within-cluster heterogeneity. * $p < .05$

Table 2

Moderator Analysis Including all Four Moderator Variables Simultaneously

	Moderator Analysis			Correlations		
	γ	SE_γ	z	(1)	(2)	(3)
Intercept	-0.04	0.09	-0.49			
(1) Stakes (1 = high; 0 = low)	0.08	0.11	0.69			
(2) Countermeasures (1 = yes, 0 = no)	0.12	0.11	1.05	.43		
(3) Searchability (1 = high, 0 = low)	0.26*	0.09	2.87	.44	.24	
(4) Modality (1 = cross mode, 0 = same mode)	0.14	0.10	1.50	-.17	-.18	.10
Q_M	17.62*					
$\sigma^2_{(2)}$ / $\sigma^2_{(3)}$	0.11 / 0.03					
k_1 / k_2	109 / 65					

Note. Phi coefficient of correlation for dichotomous variables is displayed. All correlations are based on 109 effect sizes. γ = Fixed effects regression weight; SE_γ = Standard error of γ; Q_M = test statistic for the omnibus test of coefficients (*df* = number of categories of the moderator − 1); $\sigma^2_{(2)}$ = between-cluster variance; $\sigma^2_{(3)}$ = within-cluster variance; k_1 = Number of effect sizes; k_2 = Number of samples. * $p < .05$

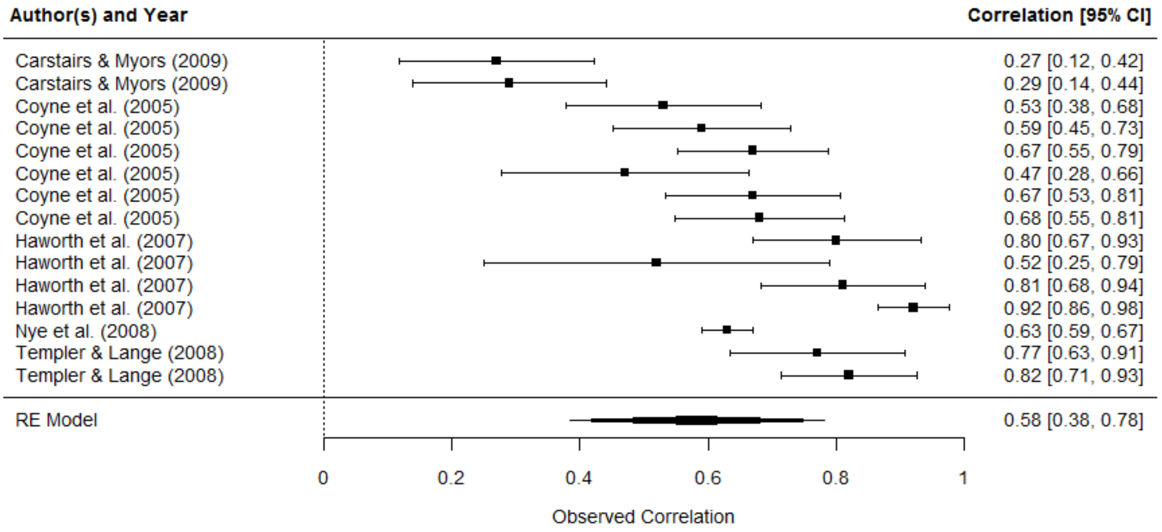

Figure 1. Forest plot of the results of the random-effects model for the analysis of correlation coefficients.