How Reliable and Valid are Frequency-Based Originality Scores? Evidence from a Two-Step Approach

Boris Forthmann¹, Dorota M. Jankowska² and Maciej Karwowski³

1: University of Münster, Germany

2: The Maria Grzegorzewska University, Poland

3: University of Wrocław, Poland

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Boris Forthmann, Institute of Psychology and Education, University of Münster, Germany. Dorota Maria Jankowska, Institute of Education, The Maria Grzegorzewska University, Poland, Maciej Karwowski, Institute of Psychology, University of Wrocław, Poland.

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Please address correspondence concerning this article to Boris Forthmann, University of Münster, Fliegerstrasse 21, 48149 Münster, Germany, +49-251-83-34162, boris.forthmann@wwu.de

Abstract

Creativity—as any other object of scientific endeavor—requires a sound measurement that adheres to quality criteria. For decades, creativity science has been criticized as falling short in developing valid and reliable measures of creative potential, activity, and achievement. Recent years have witnessed growth of theoretical and empirical works that focused on improving creativity assessment. Here, we apply one of such recently developed approaches based on item response theory to examine ideas' and person score reliability in a divergent thinking task. A large sample ($N = 621$) of children and adolescents solved the Circles task from Torrance Tests of Creative Thinking-Figural and two other figural tests measuring creative thinking (Test of Creative Thinking-Drawing Production) and creative imagination (Test of Creative Imagery Abilities). By employing response propensity models, we observed that separate ideas' reliability tended to fall below recommended thresholds (even liberal ones, e.g., .60) unless the sample size as well as the number of generated ideas (fluency) were large. Importantly, reliability at the idea-level affected reliability at the person-level much less than could be assumed based on recent findings. We propose a systematic perspective on divergent thinking assessment that considers responses as nested in tasks and tasks as nested in tests. Finally, we recommend that adding more tasks to divergent thinking tests might increase reliability at the task-level.

Keywords: originality; frequency; reliability; validity; divergent thinking

How Reliable and Valid are Frequency-Based Originality Scores? Evidence from a Two-Step Approach

Originality has long been an important construct under investigation within social and humanist sciences, although it is defined differently depending on the discipline (Guetzkow et al., 2004). Creativity scholars mostly agree that any perceivable output that is creative (e.g., a response given to solve a creative thinking task) needs to possess a certain degree of originality (Kharkhurin, 2014; Plucker et al., 2004; Runco & Jaeger, 2012; Simonton, 2018). Nevertheless, measuring originality still presents a formidable challenge (Said-Metwaly et al., 2017). As opposed to creative output produced by eminent scientists or artists, originality as operationally defined in creative thinking tests requires redefinition in relative terms (Wilson et al., 1953). Indeed, for the measurement of originality in many assessment contexts, researchers need to shift away from originality in absolute terms (i.e., the output is original for mankind; see Shibayama & Wang, 2020) to originality in relative terms (i.e., the output is original as compared to output produced by other participants). In this vein, originality, operationally defined as the frequency of occurrence in a sample, has strong face validity as a measure of relative originality (Silvia et al., 2008): something that is original should be observable on very rare occasions in a sample (Hargreaves, 1927; Wilson et al., 1953; Mouchiroud & Lubart, 2001).

Frequency-Based Originality

All commonly used definitions of creativity include originality as one defining facet (Kharkhurin, 2014; Pichot et al., 2020; Runco & Jaeger, 2012; Simonton, 2018). That includes the standard two-facet definition comprising of originality and usefulness/appropriateness (Runco & Jaeger, 2012), extensions/variations of the

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standard definition (Kharkhurin, 2014; Simonton, 2018), and the more parsimonious conceptualization of creativity as intended originality (Pichot et al., 2020).

Consequently, researchers have called for scoring creative thinking tests for originality of responses instead of mere productivity indicators (Zeng et al., 2011). This is particularly true for tests of divergent thinking (DT), which are the most widely used measures of the potential for creative thought (Kaufman et al., 2008; Runco, 1999).

DT refers to the ability to produce multiple solutions as a response to a specific task or problem (Guilford, 1967). Hence, it has been considered to be crucial for the idea generation phase in the creative process (Lubart, 2001; Mumford & McIntosh, 2017). DT tests are estimates of creative potential, and studies have demonstrated their predictive validity in this regard (Carson et al., 2005; Jauk et al., 2014; Runco et al., 2010). Performance on DT tests is assessed in terms of multiple components, with fluency and originality being most commonly used (Dumas & Dunbar, 2014; Silvia et al., 2008). As for the fluency score, it is usually quantified by the number of ideas that individuals generate in a given period of time (see e.g., *Thinking Creatively with Pictures*; Torrance, 2008), but assessing the aspect of originality is clearly less straightforward. And that's primarily because there is no agreement on how to score DT tests for originality (Reiter-Palmon et al., 2019). However, besides remoteness and cleverness (Wilson et al., 1953), uncommonness of responses is most often used as an indicator of originality.

Creativity researchers use different (subjective and objective) methods to assess originality in DT tests (Reiter-Palmon et al., 2019), among others: (a) assessment on age-based and grade-based norms (Torrance, 2008); (b) sample-based originality derived from Cropley's procedure (Cropley, 1967), namely, 4 points for each idea given by less than 1% of the sample, 3 points for ideas with a frequency seen in less than 3%,

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2 points for ideas with a frequency below 5%, 1 point for ideas with a frequency below 15%, and 0 points for ideas that appeared in exactly or more than 15% of the sample; and (c) holistic, snapshot scoring of originality (e.g., Forthmann, Holling, Zandi, et al., 2017; Mouchiroud & Lubart, 2001; Silvia et al., 2009). Scores based on the frequency of occurrence of ideas are probably most commonly used. Some of the most recent ways for the scoring of DT tests employ semantic scoring algorithms (e.g., Beaty & Johnson, 2020; Beketayev & Runco, 2016; Dumas et al., in press).

The First Step of Scoring Happens at the Response-Level

Frequency-based originality scoring starts with a cross-tabulation of all responses in a sample (Cropley, 1967; Reiter-Palmon et al., 2019). This cross-tabulation procedure requires decisions about which responses are treated as the same and which are not. For example, should *drinking milk* and *drinking juice* be treated as the same responses for uses of a *cup* (Forthmann, Paek et al., 2020; Reiter-Palmon et al., 2019)? In the case of graphical DT tests, an additional assessment decision should be made; for example, responses irrelevant to the stimulus, duplicate responses, meaningless titles combined with a drawing, should be excluded (Torrance et al., 1992). Then, based on the cross-tabulation frequency of occurrence, estimates for each response can be derived and used for subsequent originality scoring, which can take one of many different forms. Uniqueness awards a point for responses when they were provided by only one participant in the sample (Silvia et al., 2008; Wallach & Kogan, 1965). Threshold scoring awards responses that occur less frequently as compared to predefined thresholds (Cropley, 1967; Runco, 2008; Torrance, 1990). The complement of frequency estimates (i.e., 1-relative frequency; see Forthmann, Holling, Çelik, et al., 2017; Mouchiroud & Lubart, 2001) or the inverse of frequency estimates (i.e., [relative frequency]⁻¹; see Beketayev & Runco, 2016; Dziedziewicz et al., 2014) can also result

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directly in an originality score. What all of these scoring approaches have in common is that frequency estimates are the basis for originality.

Frequency-based originality scoring has also been criticized for potentially awarding random, fuzzy, or non-sensical responses (Silvia et al., 2008). Such problems, however, can be mitigated by employing appropriate criteria for response adequacy (Reiter-Palmon et al., 2019). As a more critical issue, the sample size dependence of frequency estimates (Silvia et al., 2008; Simpson, 1922) and their measurement precision (Forthmann, Paek, et al., 2020; Reiter-Palmon et al., 2019) need to be taken into account. In particular, the measurement precision of frequency estimates has been studied using an item response theory approach that allows for quantifying the reliability of estimates in general and specifically the reliability for given levels of response propensity (Forthmann, Paek, et al., 2020). It was found that frequency estimates might yield the required reliability levels when 300 to 400 participants are examined. However, often research relies on way smaller samples (De Dreu et al., 2014; Wallach & Kogan, 1965), which is particularly problematic for uniqueness scoring based on frequency-estimates with the lowest conditional reliability (Forthmann, Paek, et al., 2020).

How accurate are the recently recommended sample sizes of 300 to 400 participants for reliably using frequency-based originality scoring? And—along the same lines—do the findings from Forthmann and colleagues who used the Alternate Uses Task (Guilford, 1967; Wallach & Kogan, 1965) generalize to other DT tasks? This work aims at extending recent research on the measurement precision of response-frequency estimates to find answers to these important research questions related to the assessment of originality.

The Second Step: Aggregating Response-Level Originality at the Person-Level

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In addition, we posit that the scoring of DT tasks benefits from being understood as a two-step procedure. Initially, responses are scored for originality. Next, response originality scores are aggregated to yield an originality score for each test-taker. Hence, it is of great interest how reliability at the response-level translates into reliability at the person-level. To examine this research question, we also examined the reliability of originality estimates at the person-level.

A related issue was to study reliability of the person originality scores as a function of fluency. Originality scores are based on more information for persons with larger fluency scores (Cronbach, 1941) and, hence, they should display larger reliability (i.e., reliability of originality conditional on fluency). Consequently, here we examine reliability of the person originality scores for a variety of scorings.

Finally, validity of the originality scores is essential. We assess validity of several frequency-based originality scores for a figural DT task based on several criteria. We compare correlations between the originality scores obtained by different methods (i.e., complementary scoring, inverse scoring, and variants of threshold scoring including uniqueness as a special case) and two established figural creativity tests, yet not DT tests.

Method

The data and analysis scripts to reproduce the findings reported in this work are openly available in a repository of the Open Science Framework (https://osf.io/hkf7w/?view_only=99f9c98063fa43c0b60d1b8bd1dc5f8c).

Participants

Altogether, $N = 621$ children and adolescents were examined in this study. The participants were at an average age of 9.41 years ($SD = 4.53$, range: 4 to 19). Concerning gender, $n = 278$ of the participants were male, while $n = 342$ were female.

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Measures

Circles Task from the Torrance Tests of Creative Thinking. The primary measure of our study was a Circles task (Activity 3) from the figural form of TTCT (Thinking Creatively with Pictures-Version B; Torrance, 2008). The response booklet includes 3 tasks (activities), the last of which is a task with 20 identical circles. The task of the participant is to add elements to the circles to end up with interesting drawings. The timing of the task is limited to 10 minutes (Torrance, 2008). The number of solutions an individual expressed through interpretable responses that use the stimulus in a meaningful manner serves as the fluency score. Given the number of circles, fluency has a limited maximum value of 20 points. The following ideas were excluded from the analyses: (a) irrelevant to the stimulus, (b) duplicate responses (only first response was scored), (c) responses with meaningless titles. That is, a response is irrelevant if the stimulus is not directly used; if the idea is independent of the stimulus; if it is a meaningless abstraction; or if it is a duplication in usage and title (Torrance, 2008). In our study, also all drawings with connections between circles were excluded.

If any drawing was unscorable for fluency, it was also eliminated from originality scoring. Next, prior to originality scoring, a cross-tabulation of responses was created from which frequency estimates for each response could be derived. This cross-tabulation required decisions about which responses are to be treated as synonymous and which are not. For example, adjectives used in a description of a drawing were ignored when the adjectives did not essentially change the meaning of a response (i.e., *human face*, *smiling face*, and *pretty face* were all treated as synonymous responses).

Scoring of Person Originality in the Circles Test. Person originality was based on six different scoring approaches: (a) complementary scoring (Forthmann,

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Holling, Çelik, et al., 2017; Mouchiroud & Lubart, 2001); (b) inverse scoring (Beketayev & Runco, 2016); (c) multiple-threshold scoring (Cropley, 1967); (d) uniqueness scoring (Silvia et al., 2008; Wallach & Kogan, 1965); (e) 5% threshold scoring; and (f) 10% threshold scoring.

Each of these scoring approaches requires evaluation of each response according to a certain weighting based on a response's frequency of occurrence: (a) complementary scoring weighs every response by 1-relative frequency (Forthmann, Holling, Çelik, et al., 2017; Mouchiroud & Lubart, 2001; Runco et al., 1987); (b) inverse scoring weighs each response by $(\text{relative frequency})^{-1}$ (Beketayev & Runco, 2016; Dziedziewicz et al., 2014); (c) Cropley's multiple threshold approach (Cropley, 1967) weighs responses according to the following scheme: relative frequency of < 1% is awarded 4 points; relative frequency of < 3% is awarded 3 points; relative frequency of < 5% is awarded 2 points; relative frequency of < 15% is awarded 1 point, and all relative frequencies $\geq 15\%$ are awarded 0 points; (d) uniqueness scoring weighs every response that was provided by only one participant in the sample with a value of 1 and 0 otherwise (Silvia et al., 2008; Wallach & Kogan, 1965); (e) 5% threshold scoring weighs relative frequencies of < 5% with a value of 1 and relative frequencies $\geq 5\%$ with zero (Runco, 2008; Runco et al., 1987); (f) 10% threshold scoring weighs relative frequencies of < 10% with a value of 1 and relative frequencies $\geq 10\%$ with zero (Runco, 2008; Runco et al., 1987). These originality evaluations applied at the response level were then used to aggregate scores at the person-level (see details below).

Validity Measures

The Test of Creative Imagery Abilities (TCIA). We used TCIA to measure creative visual imagination (Jankowska & Karwowski, 2015) on part of our sample ($N = 494$). The TCIA quantifies three characteristics of creative imagery abilities as

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established elsewhere (Dziedziewicz & Karwowski, 2015): vividness, imagery originality, and transformations of mental images. The TCIA test booklet consists of seven tasks. In an individual interview, the researcher writes down the participant's answers on a prepared answer sheet. The first stage of solving each task is exploratory. The participant generates as many ideas as possible based on a simple graphic sign, called the initial figure. Next, the participant selects the most original of the ideas and creates a drawing accompanied by a brief description. The instruction emphasizes the necessity to elaborate and change the initial idea. The final drawings and descriptions are scored on three scales: the imagery vividness scale, the imagery originality scale, and the imagery transformation scale. The imagery vividness scale measures the capability to evoke clear, lucid, and vivid visual images. Another crucial capability to produce creative mental images characterized by uniqueness (newness) is measured by the originality scale. Finally, the imagery transformation scale measures the capability to actively mentally manipulate and transform visual mental images (Jankowska & Karwowski, 2020). Each drawing is scored 0, 1, or 2 points on each item. The TCIA reliability in this study was estimated by Cronbach's α based on a matrix of polychoric correlations (overall $\alpha = .88$; imagery vividness $\alpha = .75$; imagery originality $\alpha = .75$; imagery transformation $\alpha = .87$).

Test for Creative Thinking-Drawing Production (TCT-DP). For $N = 606$ of the participants, we used TCT-DP (Urban & Jellen, 1996) to measure creative thinking on figural material. The participant's purpose is to complete the drawing, starting from six elements placed on the test sheet. The scoring of the TCT-DP test includes 14 detailed, mostly formal, criteria. The total score is the sum of points obtained in all fourteen criteria, which gives one overall score (Urban & Jellen, 1996). In this study, reliability of the overall score was $\alpha = .77$.

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Procedure

We aggregated datasets from separate studies on different samples, mainly consisting of children and adolescents. When children aged 4-9 ($n = 378$) participated, tests were solved individually by a child accompanied by a researcher. The researcher recorded each child's responses that were later transcribed and coded. In the case of older children and adolescents ($n = 243$), the tests were solved in group settings, and participants wrote down their answers. Apart from the individual-versus-group settings, all other testing conditions were the same. Informed consent was obtained from parents, schools' and kindergartens' authorities, and from the participants themselves.

Analytic Strategy

Following recent suggestions (Forthmann, Paek, et al., 2020), the one-parameter logistic (1PL) model (De Boeck et al., 2011) was applied in this study to estimate response propensity estimates β_i for the response i . In addition to response propensity, probability for a response to be generated is modeled as a function of ideation parameter θ_v of person v :

$$P(X = 1|\beta_i, \theta_v) = \frac{\exp(\beta_i + \theta_v)}{1 + \exp(\beta_i + \theta_v)}. \quad (1)$$

The β_i at the logit-level follow an $N(0, \sigma_\beta^2)$ distribution (i.e., the β_i are modeled as a random effect), whereas the ideation parameters θ_v are treated as fixed effects.

Importantly, based on this model, empirical reliability (Brown & Croudace, 2015; Green et al., 1984) can be estimated as

$$Rel(\beta) = 1 - \overline{SE}_\beta^2 / s_\beta^2, \quad (2)$$

with the average standard error across all is $\overline{SE}_\beta^2$, and the estimated variance of the propensity parameter distribution s_β^2 . Moreover, conditional reliability (Green et al.,

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1984) can be obtained when replacing the average standard error in Equation 2 by the standard error $SE_{\beta_i}^2$ for a given propensity estimate β_i .

The analysis was performed by means of the package ‘mirt’ (Chalmers, 2012) for the statistical software R (R Core Team, 2019). The propensity parameters and their standard errors were estimated by means of the maximum a posteriori method implemented in ‘mirt.’ To assess relative model fit the 1PL was contrasted with a less restrictive 2PL model following previous work on propensity based on Equation 1 (Forthmann, Paek, et al., 2020). Finally, absolute model fit was examined by means of covariate-adjusted frequency plots (Holling et al., 2015, 2016) based on absolute response frequencies, which are known to follow a generalized binomial distribution (González et al., 2016; Lord, 1980). The R package *GenBinomApps* (Lewitschnig & Lenzi, 2014) provided the functions needed for the generalized binomial distribution. Finally, to assess the sample size dependence of response propensity reliability estimates, we used the same down-sampling approach as Forthmann, Paek, et al. (2020). Here, the original data were resampled 100 times either with $N = 100$ or $N = 300$, respectively.

Next, person originality was estimated by means of intercept only models with random intercepts across persons. For complementary, inverse, and multiple-threshold scoring, a model based on the Gaussian distribution was used, whereas for uniqueness, a 5% threshold and a 10% threshold logistic models were used. Empirical reliability and conditional reliability were calculated for the person’s originality scores analogous to the reliability of response propensity. The intercept only models were estimated by the R package lme4 (Bates et al., 2015). Person originality scores were also based on the maximum a posteriori estimator (and their standard errors as implemented in lme4). Reliability of person originality was also assessed for sample size dependency by means

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of the down-sampling approach described above. The estimated person originality scores were then correlated with the criterion measures to assess validity of the scores. To allow comparison of validity coefficients that is not affected by varying levels of reliability of the person originality scores, we applied the correction of attenuation formula as suggested by Wang and colleagues (Wang, 1999; Wang et al., 2004)

Results

Descriptive Statistics

Descriptive statistics revealed that responses occurred either once (i.e., unique responses; relative frequency = 0.2%) or up to 249 times (relative frequency = 40.1%) in the sample. Altogether, 921 different responses were provided by the children (560 were unique responses and 361 were non-unique responses). Further, the children generated a total of 4,363 responses. The children had an average of 7.03 responses on the circles task ($SD = 5.53$; range: 1 to 20).

Step 1: Reliability of Response Propensity Estimates

Model fit indicated that the data fit better to the 2PL model than the 1PL model (see Table 1). As indicated by CAFPs, the fit of both models was highly comparable (see Figure 1). However, differences between empirical reliability estimates were clearly more substantial as compared to previous related work (Forthmann, Paek, et al., 2020). Importantly, the empirical reliability estimate of .60 was lower as compared to all AUT items studied in Forthmann, Paek, et al. (2019). This is further highlighted in Figure 2. In addition, 27.47% of the responses had a conditional reliability of $> .70$, which highlights that only approximately one-third of the responses were associated with acceptable reliability estimates. The smallest observed response frequency with an acceptable reliability estimate was 0.32% (i.e., a response that was generated by only two participants).

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Variation of conditional reliability estimates is clearly stronger when the estimates were based on the 2PL compared to the 1PL. In this vein, conditional reliability estimates of unique responses were also affected by using the 2PL instead of the 1PL. This is illustrated in Figure 3, which displays a histogram of the conditional reliability estimates of unique responses (range of estimates was from .29 to .65). Clearly, reliability of unique responses depended on the persons who generated the unique response, because increasing person discrimination increases measurement precision in the 2PL (Doebler & Doebler, 2020). Hence, it was decided to run the down-sampling procedure based on the 2PL. The average of empirical reliability estimates across resampled datasets was even negative (-0.48 and, hence, it should be understood as zero reliability) for $N = 100$, whereas for $N = 300$ it was .30. This finding was paralleled by an examination of conditional reliability (see Figure 4). Hence, for the full sample size of $N = 621$, a clear gain in terms of reliability of response propensity estimates was observed for empirical reliability (.60; see Table 1) and conditional reliability (range: .41 to .99).

Step 2: Reliability and Validity of Person Originality Estimates

With respect to the reliability of person originality estimates, the findings revealed that empirical reliability was highest (.46) for inverse scoring and lowest (.16) for complementary scoring (see Table 2). Hence, reliability was clearly below any suggested standard (e.g., .70) for all applied scoring approaches. In addition, contrary to what was found for the reliability of propensity estimates, it was revealed that the reliability of person originality scores was only negligibly affected by sample size (see Table 2). Hence, suboptimal reliability of person originality is unlikely to be mitigated by increasing the sample size alone. Figure 4 further illustrates that acceptable levels of reliability of person originality can be achieved when fluency is taken into account. For

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example, when at least 15 responses were provided, reliability of person originality was above .70 for both inverse scoring and uniqueness scoring. It is also illustrated in a clear manner that reliability of person originality is an increasing function of fluency (see Figure 5). The bottom row of plots shows reliability of person originality estimates conditional on fluency that are based on a logistic model. This explains that reliability of person originality also varies within the fluency values (as compared to the top row of plots that are based on the Gaussian distribution).

Validity coefficients are displayed in Table 3. It should be noted that the correction for attenuation was applied for both the unreliability of the person originality scores and the respective validity criteria, to allow comparisons between the originality scoring approaches. It was found that the complementary scoring approach displayed the best validity results for both criteria with moderate to large effect sizes. Inverse scoring and uniqueness scoring displayed highly comparable validity findings with small to moderate correlations. In addition, multiple-threshold scoring, 5% threshold scoring, and 10% threshold scoring also yielded highly comparable validity findings with small to large correlations. Finally, we generally found slightly larger correlations between person originality and the TCIA vs. the TCT-DP.

Discussion

Reliability is a basic psychometric property that has been recently studied at the level of response estimates as the basis of originality (Forthmann, Paek, et al., 2020). The current study demonstrates that also for a figural DT task reliability of response propensity estimates is smaller than compared to recommended standards (Aiken, 1994; Murphy & Davidshofer, 1994). However, the current work extends recent findings by considering DT scoring as a two-step approach. Importantly, the reliability of person originality estimates was only negligibly affected by the applied down-sampling

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approach. This suggests that recent claims for rather large sample sizes (Forthmann, Paek, et al., 2020) were perhaps exaggerated when person originality—and not the reliability of response propensity estimates—is of concern.

Responses in Tasks and Tasks in Tests: Divergent Thinking Assessment has a Nested Structure

Findings of the current study emphasize the complexity that arises from the different steps required for originality scoring: (a) evaluation of frequency-estimates of single responses, and (b) combination of evaluations of single responses into a person originality score. Notably, person scoring implies a kind of an interaction of propensity and ideational parameters (see Equation 1 or its 2PL extension). In fact, by using one of the applied scoring approaches (e.g., uniqueness and so forth) a researcher implicitly assumes this interaction a priori. This observation was not yet understood and acknowledged in previous research (Forthmann, Paek, et al., 2020), while it actually seems to be important. In addition, there is still another level to consider, namely the test-level, when multiple tasks are available. Responses are nested in DT tasks, and DT tasks are nested in DT tests. Critically, reliability can be taken into account at each of the three levels and the different levels interact with each other. For example, it seems that reliability at the response level affects reliability at the task level less than could be assumed when frequency-based scoring is used. This is the main finding of the current research. Below, we discuss each level that was examined in this work and how they might affect reliability and validity.

Response-Level

Forthmann, Paek, et al. (2020) have suggested that reliability of response propensity estimates is unlikely to reach acceptable levels with sample sizes below $N = 300$. The findings of the current work are clearly in accordance with these recent

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recommendations. In the current study, reliability of .00 or .30 for sample sizes of $N = 100$ and $N = 300$ was found, respectively. Only for the full sample size of $N = 621$ reliability of response propensity estimates was near acceptable levels (.60), but it was still suboptimal. Hence, just from the response-level perspective one would conclude that to reach higher levels of reliability, sample sizes must be even higher than $N = 621$. This conclusion might sound disheartening for creativity researchers who understand how time-, effort- and funds-consuming conducting and scoring such studies are.

Clearly, the down-sampling approach revealed that reliability of response propensity estimates is sample size-dependent. The varying levels of conditional reliability of unique responses in this study resulted from relying on the 2PL model for response propensity. Having more discriminating participants in the sample will affect measurement precision of response propensity estimates. Hence, conditional reliability of (unique) responses depends on which persons generate them. This observation introduces yet another sample dependence of frequency-based scorings. As a result, the observed frequencies (Silvia et al., 2008) and summary indices of reliability of response propensity estimates (Forthmann, Paek, et al., 2020) depend on sample size, whereas conditional reliability of response propensity depends on varying person discrimination in the sense of the 2PL model used in this work (i.e., on the composition of the sample). That is, persons with discrimination around zero generate responses with a broad range of frequencies, whereas persons with decreasingly negative discrimination tend to generate infrequent responses, and persons with increasingly positive discrimination tend to generate the most frequent responses. This pattern is illustrated in Figure 6 and observable particularly when minimum or average response frequencies are aggregated at the person-level (i.e., when aggregating the maximum the pattern was less clear).

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One clear difference that can be observed between this study and the work of Forthmann, Paek, et al. (2020) relates to the DT tasks' solution space characteristics. Forthmann, Paek, et al. (2020) observed for the verbal Alternate Uses Tasks ratios of the number of unique responses to the number of non-unique responses, ranging from 0.60 to 0.83 (i.e., for each task, there were less unique responses as compared to non-unique responses). For the circles task used in the current work, however, a ratio of 1.55 was observed. This highlights that the number of unique responses was clearly larger as compared to non-unique responses. Overall, response propensity reliability must be affected by such a difference in the relative amount of unique responses, because unique responses will necessarily display comparably lower reliability estimates, decreasing such a reliability summary statistic as empirical reliability. Hence, again, this work arrives at the somewhat paradoxical conclusion that we measure originality in a reliable manner when responses are not too original.

Task-Level

The current study extends recent research on frequency-based originality scoring that focused on the response-level by additionally taking the task-level into account. Several approaches to derive originality scores at the response-level were aggregated at the person-level for all responses generated on the circles task. This level of aggregation further allowed us to study validity of the various approaches. Concerning reliability of person scores at the task-level, we observed that aggregated scores for a single-task were, on average, not sufficiently reliable. Given that the approach of aggregation was conceptually similar to average scores (i.e., ratio scores), it is not surprising that reliability is low because this is psychometrically expected (Cronbach, 1941) and has been shown empirically (Plucker et al., 2014; Runco et al., 1987). Importantly, this aggregation approach prevented the confounding effect of fluency on person originality

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(Forthmann, Szardenings, et al., 2020; Hocevar, 1979; Runco et al., 1987). We see two ways to mitigate reliability issues at the task-level: (a) redesign the circles task in a way that allows for more responses per person, and (b) assessing more than one figural DT task.

First, person-level reliability would likely benefit from either a complete lack of time limits (a solution that is unlikely and problematic from standardization and comparison purposes) or at least more extended time limits, allowing participants to fully explore the solution space of each task. This, however, might be easier to obtain if participants will be presented with a larger pool of objects to be explored. We hypothesize that a richer pool of objects to be creatively expanded—not necessarily the same; let them be triangles, circles, and so on—would increase reliability even more. For example, a 15-minutes-long task with 30 to 40 varying objects could be developed. In this work, it was clearly demonstrated that sufficiently reliable person-scores (at least for a subset of the studied approaches) depend on a person's fluency. Participants had twenty circles to be filled, yet reliable scores were obtained among those who used at least fifteen of them. However, participants may dry out of ideas while working on the task. Hence, this approach could become ineffective, but as long as participants have not yet reached a point of idea saturation, reliability could be improved by increasing objects and time on task. Barbot's Multi Trial Creative Ideation framework (Barbot, 2018) is a promising assessment framework in this regard. Yet, we emphasize that when time is money, the observable tendency among scholars is quite the opposite—researchers tend to shorten their test (consider, for example, a shortened version of TCIA; Jankowska & Karwowski, 2015, 2020), not to lengthen them. There is no easy solution to this alternative; having a DT test that is both short and reliable seems not very realistic given our estimates.

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Second, a natural alternative and recommendation that is informed by our analyses is to rely on multiple tasks when assessing originality of persons. In this vein, reliability could simply be increased by testing more than one DT task from the same content domain (here figural). This, however, comes at a cost too. More tests means more time, more coding, and scoring, not to mention participants' fatigue and lack of motivation (Haager et al., 2018; Wilken et al., 2019). Separate sessions might help, but they come along with other inconveniences. Despite these potential drawbacks, it also has the advantage that the assessment is more diverse (i.e., the assessment shifts away from a mono-methodological approach). The current study is clearly limited in this regard, because we used only a single task. Therefore, future research should carefully examine both ways to increase reliability and validity of DT measures. What seems further promising in this regard, is developing batteries of well-calibrated, computerized, adaptive creativity tests to make the process shorter without losing its reliability. This is, however, a solution to come.

In terms of validity, we observed that the most reliable methods did not necessarily result in the highest validity. Of course, reliability limits validity, and without correction for attenuation it would not be possible to arrive at this conclusion. This correction for attenuation, however, allowed for a comparison between the various scoring approaches, which takes varying levels of reliability into account. Complementary scoring yielded the highest and moderate to large validity coefficients. The lowest validity was found for inverse scoring, whereas all other approaches were found to be moderately valid. In fact, the confidence intervals for multiple-threshold scoring, uniqueness scoring, 5%, and 10% threshold scoring largely overlapped. In general, the findings speak in favor of the validity of frequency-based scoring when aggregated at the person-level (i.e., validity can also be studied at the response-level and

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our findings do not generalize in that regard) and they further highlight the importance to take unreliability into account.

Response-Level and Task-Level Interplay

It was unknown how the different levels of scoring DT tasks affect each other when it comes to psychometric properties. However, this study clearly revealed that sample size heavily influences reliability of propensity estimates at the response-level, but reliability of person originality aggregates is only negligibly affected. Whether this observation generalizes to other tasks remains a somewhat open question. For example, it could be the case that other tasks with other solution space characteristics (e.g., in terms of the ratio of unique to non-unique responses) are affected more strongly at the task-level. In relation to this, it should be noted that response propensities, as studied in models such as the one in Equation 1, do not represent the same signal as compared to the approaches to score response originality. The originality scoring approaches suggest that individual differences in originality become observable by weighting a person's ideational output. However, response propensities in Equation 1 and person originality as implied by aggregating the various weighting approaches can only be studied as a two-step approach. Future work might consider approaches that tackle both levels at the same time in a way that allows to empirically determine a weighting that maximizes individual differences in originality.

Conclusion

It goes without saying that to better understand and—ultimately—develop creativity, we must measure it. Even more importantly, we must measure it well. For decades, such a measurement was mainly conducted with the use of DT tests. And for good reasons. Although criticized (Hocevar, 1981), DT tests are based on a logically-consistent theory (Guilford, 1967), their results are not reducible to intelligence (Gerwig

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et al., in press), and their scores predict real-life creative accomplishments (Plucker, 1999). So indeed, Plucker and Runco (1998) seemed to be right, when two decades ago they paraphrased Mark Twain by saying that the death of creativity measurement has been greatly exaggerated.

Yet, modern times require modern solutions. Over the years, DT tasks have been reconceptualized in terms of cognitive processes engaged (Beaty & Silvia, 2012; Nusbaum et al., 2014), new measurement approaches were proposed (Barbot, 2018), and—last but not least—more advanced statistical techniques were used to assess their psychometric properties. Recently, Forthmann and colleagues (2020) provided a set of results that might be overwhelming for scholars who apply DT tasks in their research. This is the case, as Forthmann et al. demonstrated, of very limited reliability of originality scores, especially when the study's sample size is small.

The present study replicated and extended this recent finding. On the most general level, our results are concordant with those reported by Forthmann and colleagues (2020). The conclusion that originality scores' reliability is too low to make divergent thinking tasks useful seems tempting. However, we believe that it would be premature and this issue is far more complex. We demonstrated that different ideas' reliability is low unless the sample size is considerable (probably higher than our six hundred participants). The recommendation that stems from this finding seems obvious: robust estimates of reliability at the idea-level require large samples. Forthmann et al. recommended *N*s of 300-400 for reliable scoring of verbal Alternate Uses Tasks. For figural DT tasks, the situation might be even more challenging—to precisely estimate the originality of rare ideas, we might need really large samples.

The second finding of our study concerns person-level reliability. As we demonstrated, here increasing sample size brings little benefits. What seems critical is a

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rich pool of responses obtained from each participant. When a child generates only one idea in response to a task—even a great idea—a precise estimation of that child’s originality is hardly possible. Person-level originality seems to be conditional on fluency. Two natural recommendations that come to mind are re-thinking time limits while testing and considering using longer tests. We have discussed potential drawbacks and advantages of these assessment strategies and see this as a promising avenue for future research. As suggested in this work, the nested structure of DT assessment—responses in tasks, tasks in tests—provides a useful framework to facilitate the interpretation of existing psychometric findings in the DT literature and to better study the complex interplay between the levels (steps) of DT assessment.

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Table 1

Model information criteria, model comparison statistics, and empirical reliability results.

	1PL	2PL ^a
AIC	39,197.42	38.535.03
BIC	42,198.85	44,528.26
LR-test (<i>df</i>)		1902.38 (620)***
$\overline{SE}_{\beta_i}^2$	0.39	0.32
s_{β}^2	0.82	0.80
Marginal reliability	.53	.60

Notes. AIC = Akaike's Information Criterion; BIC = Bayesian Information Criterion; lower values on AIC and BIC imply that a model is superior in terms of fit and model parsimony as compared to a model with higher values (only the models that fit for the same task are comparable here); LR-test = Likelihood-Ratio test for comparing the 1PL with the 2PL; $\overline{SE}_{\beta_i}^2$ = average of the squared standard errors for the β_i estimates; s_{β}^2 = variance of the β_i estimates. Marginal (empirical) reliability = $1 - \overline{SE}_{\beta_i}^2 / s_{\beta}^2$ (see Brown & Croudace, 2015). ^aThe 2PL model had to be estimated with nlminb as optimizer for the M-step in the expectation maximization (EM) algorithm, because the default L-BFGS-B optimizer did not yield convergent results at the default level of tolerance (for details see Chalmers, 2012).

*** $p < .001$.

Table 2

Down-sampling results for person originality.

	<i>N</i> = 100 ^a	<i>N</i> = 300	Full sample (<i>N</i> = 621)
Complementary scoring	.18	.16	.16
Inverse scoring	.44	.46	.46
Multiple-threshold scoring	.36	.38	.38
Uniqueness scoring	.31	.32	.33
5% threshold scoring	.25	.26	.27
10% threshold scoring	.17	.18	.19

Notes. Depicted are the empirical reliability estimates for person originality estimates. Complementary scoring, inverse scoring, and multiple threshold scoring were based on a Gaussian intercept-only model. Uniqueness scoring, 5% threshold scoring, and 10% threshold scoring were based on a logistic intercept-only model. ^aFor 19 of the resampled datasets a model estimation error was flagged (i.e., results are actually based on 81 resampled datasets).

Table 3

Validity results for person originality.

	TCIA (<i>n</i> = 494)	TCT-DP (<i>n</i> = 605)
Complementary scoring	.55 [.33, .78]	.51 [.29, .73]
Inverse scoring	.31 [.17, .44]	.21 [.08, .34]
Multiple-threshold scoring	.39 [.25, .54]	.33 [.18, .47]
Uniqueness scoring	.36 [.20, .51]	.27 [.11, .42]
5% threshold scoring	.47 [.29, .64]	.38 [.21, .55]
10% threshold scoring	.45 [.23, .66]	.43 [.22, .64]

Notes. Correlations are corrected for attenuation (Wang, 1999; Wang et al., 2004).

Figure 1. Left: Covariate-adjusted frequency plot for the 1PL and 2PL. Model-implied counts are depicted by gray lines and they should be as close as possible to the observed frequency counts to indicate model fit. Right: model-implied frequency counts of absolute response frequencies derived from the 2PL (y-axis) are plotted against model-implied frequency counts of absolute response frequencies derived from the 1PL (x-axis). Fit of the 1PL and 2PL is comparable when model-implied frequency counts of absolute response frequencies from both models are on the reference line (intercept = 0 and slope = 1).

Figure 2. Conditional reliability estimates for the 2PL (y axis) are plotted against conditional reliability estimates for the 1PL (x axis).

Figure 3. Histogram of the conditional reliability estimates for unique responses.

Figure 4. Conditional reliability estimates for responses are depicted as a function of predicted response probability (relative frequency) and sample size of the resampled data. For each sample size, 100 resampled data sets were drawn from the original data. Reliabilities and predicted response probability values were averaged across resampled data sets. Left: x-axis ranges from 0 to .30 to cover the full range of predicted response probabilities. Right: x-axis ranges from 0 to .05 for better visual comparison of the respective sample size conditions at lower predicted response probabilities.

Figure 5. Conditional reliability estimates for person originality are depicted as a function of fluency for each of the respective scoring approaches. Top-left: Complementary scoring. Top-middle: Inverse scoring. Top-right: Multiple-threshold scoring. Bottom-left: Uniqueness scoring. Bottom-middle: 5% threshold scoring. Bottom-right: 10% threshold scoring.

Figure 6. Observed response frequency (y axis) as a function of person discrimination parameters (x axis). Depicted are further average response frequencies (black circles), minimum response frequencies (red plus signs), and maximum response frequencies (blue Xs) for each person.