**Psychometric Models of Individual Differences in Reading Comprehension:
A Reanalysis of Freed, Hamilton, and Long (2017)**

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Abstract

Individual differences in reading comprehension have often been explored using latent variable modeling (LVM), to assess the relative contribution of domain-general and domain-specific cognitive abilities. However, LVM is based on the assumption that the observed covariance among indicators of a construct is due to a common cause (i.e., a latent variable; Pearl, 2000). This is a questionable assumption when the indicator variables are measures of performance on complex cognitive tasks. According to Process Overlap Theory (POT; Kovacs & Conway, 2016), multiple processes are involved in cognitive task performance and the covariance among tasks is due to the overlap of processes across tasks. Instead of a single latent common cause, there are thought to be multiple dynamic manifest causes, consistent with an emerging view in psychometrics called network theory (Barabási, 2012; Borsboom & Cramer, 2013). In the current study, we reanalyzed data from Freed et al. (2017) and compared two modeling approaches: LVM (Study 1) and psychometric network modeling (Study 2). In Study 1, two exploratory LVMs demonstrated problems with the original measurement model proposed by Freed et al. Specifically, the model failed to achieve discriminant and convergent validity with respect to reading comprehension, language experience, and reasoning. In Study 2, two network models confirmed the problems found in Study 1, and also served as an example of how network modeling techniques can be used to study individual differences. In conclusion, more research, and a more informed approach to psychometric modeling, is needed to better understand individual differences in reading comprehension.

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Reading comprehension is a higher-order ability that involves the recruitment of multiple lower-order cognitive processes. Previous research has identified certain domain-general mechanisms involved in reading comprehension: reasoning (Siddiqui et al., 1998; Schatschneider et al., 2007; Tiu et al., 2003), working memory capacity (Just & Carpenter, 1992; McVay & Kane, 2012; Unsworth & McMillan, 2013), perceptual/processing speed (Caplan et al., 2011; Joshi & Aaron, 2000; Payne & Stine-Morrow, 2014), and inhibition (Arrington et al., 2014; Georgiou & Das, 2018; Kieffer et al., 2013). Additionally, many domain-specific influences have been identified: verbal fluency (Jenkins et al., 2003; Silverman et al., 2012; Speece et al., 2010), word decoding (Cutting & Scarborough, 2006; Landi, 2010; Macaruso & Shankweiler, 2010), and previous language experience (Braze et al., 2016; Cromley et al., 2010; MacDonald & Christiansen, 2002).

A common methodological approach to studying domain-general versus domain-specific contributors to reading comprehension is isolating and comparing the contributions of the underlying processes using latent variable modeling (LVM). The purpose of the current project is to explore two approaches to studying reading comprehension (and higher-order abilities more generally), by reanalyzing data using both LVM (*common cause*; Pearl, 2000) and psychometric network modeling (*network theory*; Barabási, 2012; Borsboom & Cramer, 2013). The merits and limitations of both approaches will be considered, with attention to current best practices. As a means of illustrating these points, we critique the data analytic approach adopted by Freed et al. (2017) and illustrate how different modeling approaches can lead to different theoretical conclusions about the

contributions of domain-general versus domain-specific influences on reading comprehension.

Study 1: Latent Variable Modeling

Over a century ago, Spearman (1904, 1927) developed LVM to account for the *positive manifold*. The positive manifold is the finding that participants who demonstrate higher test scores on one measure of cognitive ability (e.g., verbal ability) demonstrate higher test scores on others (e.g., fluid reasoning; Carroll, 1993; Conway & Kovacs, 2013). LVM techniques like factor analysis allow researchers to extract latent (i.e., unobserved) factors from the shared covariance (i.e., correlation) among observed variables. Latent factors account for, or explain, the covariance among similar manifest variables (i.e., tasks measuring the same psychological construct) and are considered to be reflective—that is, individual task performance depends on some higher-order latent factor. Researchers interested in examining some higher-order cognitive ability (e.g., reading comprehension) have frequently used LVM, as these statistical procedures illuminate how various cognitive ability measures converge with like-measures and diverge from unlike-measures (Gorsuch, 1983; 1990; Tabachnick & Fidell, 2013).

Advantages of Latent Variable Modeling

Several advantages make LVM appealing to researchers interested in cognitive individual differences. First, one of the major goals and benefits of LVM (e.g., principal component analysis) is data reduction. Many observations or measurements can be collapsed into a smaller number of components or factors without incurring loss of information (Gorsuch, 1983; 1990; Tabachnick & Fidell, 2013). Additionally, despite the

potency of LVM techniques for handling large samples of complex data, conducting these analyses via readily accessible software now requires little effort or training.

Second, LVM allows inferences about unobservable psychological constructs from objective and well validated psychometric measures (Howard, 2016; Klein, 2016; Tabachnick & Fidell, 2013). Furthermore, latent factor scores are often viewed as more reliable than individual scores on the observed tasks (Flora & Flake, 2017), due to the presence of measurement error in the latter but not the former. Relatedly, LVM can be used to test complex statistical and theoretical models representing specific and elaborate predictions for the constructs described within a given theory (Hayduk et al., 2007). In fact, multiple models characterizing separate theories can be tested and compared simultaneously, allowing for the statistical assessment of different scientific viewpoints. This process facilitates stronger scientific inferences than simply testing a single hypothesis in isolation (Platt, 1964).

Overall, LVM offers easy and effective procedures for defining and measuring latent constructs from observed data. By establishing latent factors that explain the covariation among observed variables, LVM techniques offer evidence for the underlying latent structure of the observed data; this knowledge can then be used to generate or modify operational definitions of, or nomological networks for, these latent components or factors (Gorsuch, 1983; 1990).

Disadvantages of Latent Variable Modeling

LVM has limitations, despite its strengths. First, researchers need to obtain several indicators per construct (at least 3, if not 4 or more) from a large participant sample (at a minimum, $N = 100$; Marsh et al., 2010). Although these standards are costly, failing to meet

them can lead to problematic inferences. Second, a frequent critique of LVM is that these analyses may capitalize on chance and their interpretations are subjective: Model parameter estimates vary across samples and model interpretations vary across researchers (Bollen, 2002; Gorsuch, 1983; 1990). Exploratory LVM (e.g., exploratory factor analysis, principal component analysis), for example, involves the subjective interpretation of extracted factors or components, the naming of which are based on the presumed construct(s) measured by the tasks or indicators chosen by the researchers. Often not enough consideration is given to whether the factor has conceptual cohesion and sufficient theoretical grounding (Flora & Flake, 2017; see also Bock et al., 2003; Borsboom et al., 2003). Finally, due to the general ease of use of LVM, analyses can be conducted without extensive knowledge of their nuance or may be deployed in a post hoc manner when other analyses have failed or are unavailable (Flora & Flake, 2017; Preacher & MacCallum, 2003; Tabachnick & Fidell, 2013). As a result, LVM can be misused by researchers that lack familiarity with best practices and statistical guidelines that ensure rigorous and accurate model testing (Costello & Osborne, 2005; Howard, 2016; Osborne, 2014) and transparent reporting (Schreiber et al., 2006).

One frequent error (Rhemtulla et al., 2019) is conducting both exploratory and confirmatory LVM techniques on the same sample of data (see also Flora & Flake, 2017; Fokkema & Greiff, 2017; Klein, 2016). Doing so neglects the fact that a measurement model structure will always fit the original sample data well because the model structure and estimates were based on the specific effects found within that sample. This would be analogous to sewing a pair of gloves to fit the exact proportions of your hands; then after trying them on and finding they fit your hands perfectly, you conclude that the gloves will

fit other people's hands well, too. You would not be able to generalize to other people, because the only information you have is that the gloves originally designed to fit your hands indeed fit your hands. Regrettably, there are many published examples of using exploratory LVM techniques followed by confirmatory LVM techniques on the same sample of data leading to misleading conclusions.

A related, erroneous assumption is that the goal of confirmatory LVM is isolating the model that (best) fits the data (Kline, 2016). The more appropriate goal for confirmatory LVM is to test and evaluate the validity of specific, *theoretically motivated* predictions or theoretical frameworks that may generalize to other samples and populations (Gorsuch, 1983; 1990). When researchers focus on finding the model with the best statistical fit, they may slip into questionable research practices (i.e., using modification indices post hoc, or specifying overly complex models) due to the misconception that improved fit is evidence of a correct model. The resulting models may accurately reproduce the associations in the original data ("fitting like [my] glove") but indicate nothing about future samples and ignore the theoretical implications of the model.

An Instructive Example: Freed, Hamilton, and Long (2017)

Perhaps the best way to examine the strengths and weaknesses of LVM is to re-examine a published dataset, here from the reading comprehension literature. Freed et al. (2017) conducted a large study on the extent to which reader characteristics (e.g., working memory capacity, word decoding, language experience, verbal fluency, perceptual speed, inhibition, and fluid reasoning) are associated with one another and with reading comprehension. A theoretical goal was to assess how the relationships among these latent variables changed depending on which factors were included when modeling reading

comprehension ability as the outcome of interest. The authors were particularly interested in the unique contributions of working memory capacity, vocabulary, and word knowledge to variation in reading comprehension. Using an exploratory LVM (i.e., principal component analysis; PCA) followed by confirmatory LVM (structural equation modeling; SEM), Freed et al. concluded that language experience (e.g., vocabulary) and fluid reasoning are most important for reading comprehension, and reader characteristics like working memory capacity and word knowledge are less important.

More specifically, Freed et al. (2017) conducted a PCA with varimax (orthogonal) rotation to establish the measurement model that was used in subsequent SEMs. This is problematic for several reasons. First, PCA should be used purely as a sample-specific, data-reduction technique (Costello & Osborne, 2005; Tabachnick & Fidell, 2013). PCA should not be used toward the goal of establishing the underlying latent structure of the data (Costello & Osborne, 2005; Osborne, 2014) or informing theories that involve latent variables (Fabrigar et al., 1999; Osborne, 2014). Additionally, PCA forces the assumption that the variance in each measure is completely explained by the latent components extracted from the data; PCA does not, therefore, calculate unique error variance (i.e., uniqueness values) because measures are assumed to be completely explained by their underlying latent components (Costello & Osborne, 2005; Gorsuch, 1983; 1990; Tabachnick & Fidell, 2013). This assumption is inappropriate when dealing with psychological measures, which always have some error (Ellingsen & Engle, 2020; Rietveld & van Hout, 1993).

Second, by using an orthogonal rotation technique, Freed et al. (2017) forced the latent components to be statistically independent (Gorsuch, 1983; 1990), which is highly

inappropriate in this case. Overwhelming evidence indicates that cognitive abilities like the ones measured here are positively correlated (e.g., Carroll, 1993; Mackintosh, 2011; Ritchie, 2012). Indeed, prior research on theoretical models of reading comprehension has favored measurement models with correlated latent variables (e.g., Christopher et al., 2012; Kershaw & Schatschneider, 2012; McVay & Kane, 2012; Micaela et al., 2012).

Third, as argued above, it is inappropriate to conduct both confirmatory LVM (SEM) and exploratory LVM on the same sample of data (Flora & Flake, 2017; Fokkema & Greiff, 2017). Doing so will often result in good to exceptional model fit, however, this fails to establish that the confirmed model is valid or meaningful beyond the specific sample.

Finally, it was unclear why Freed et al. (2017) chose to exclude the reading comprehension measures from their original PCA, when these measures were subsequently included as an endogenous factor (i.e., outcome variable) in their SEMs. Prior to conducting any confirmatory LVM, a fully specified measurement model should be provided to establish which measured variables can be predicted by the latent variables in the model (Klein, 2016; Tabachnick & Fidell, 2013). Thus, Freed et al. should have affirmed that reading comprehension variables loaded on their own distinctive component to support the validity of any SEM proposed to explain covariation in reading comprehension.

Method

We present only the details that are necessary to understand and evaluate our reanalysis of data collected by Freed et al. (2017). For more information regarding participants, measures, and procedures, see Freed et al.

Data and Participants

Upon our request, the original authors provided the raw data from 362 young adults tested by Freed et al. (2017). Sample size varied between analyses conducted on the original raw data ($N = 362$) and the cleaned data ($N = 345$; discussed further in the Results section).

Measures

The 31 cognitive measures assessed the following 8 latent constructs: *decoding*, *working memory capacity*, *inhibition*, *language experience*, *reasoning*, *perceptual speed*, *verbal fluency*, and the outcome variable, *reading comprehension*.

Decoding. In the *Phonological Decision* task, participants viewed two non-word letter strings, and determined which one, if spoken aloud, would sound like a real word (e.g., HOWSE). For *Non-Word Naming*, participants viewed 100 pronounceable non-words (e.g., *plambust*) and pronounced them as quickly as possible. For the *Orthographic Decision* task, participants viewed two letter strings and decided which was a correctly spelled word (e.g., DEAL vs. DEEL). Finally, in the *Phoneme Transposition* task, participants listened to words presented sequentially and for each word participants removed the initial phoneme, moved this phoneme to the end of the word, and added a long “a” sound.

Working Memory Capacity. *Reading Span* involved 15 sets of sentences, and 60 target words. After participants viewed a sentence, they decided whether it made sense or not followed by presentation of an unrelated target word. After a presentation of 2–7 sentences, participants recalled all target words in order. For *Alphabet Span*, participants viewed 25 lists of words, with 2–7 words presented sequentially across each list. Participants recalled each word listed in alphabetical order. *Minus Span* consisted of participants viewing 35 sets (2–8 numbers in each set) of random numbers; following

presentation of the number, participants subtracted a value of 2 from each number. Afterwards, participants reported the differences in order per set. Finally, for *Visual Number Span*, participants viewed 24 sets of digits (4–13 digits presented sequentially in each set). Participants recalled each digit list in reverse order of presentation.

Inhibition. In a *Stroop Interference* task, participants named the hue for 105 letter strings (one at a time) that consisted of either a color name or an unrelated word; color names appeared in either congruent or incongruent hues (e.g., GREEN in red hue). Participant's naming latencies were used for analysis. Additionally, the *Go/No-Go* task was administered where participants pressed a button in response to every letter presentation as long as it is not the letter 'x', when an 'x' was presented participants were to refrain from responding.

Language Experience. Language Experience was a broad factor including measures of vocabulary, background knowledge, and print exposure (also called reading frequency). Vocabulary was measured using: (a) the *Nelson-Denny Reading Test*, which required participants to complete sentences with the final word missing, and two more traditional, multiple-choice vocabulary tests from the Ekstrom Battery (Ekstrom et al., 1976), *Extended Range* and *Advanced Vocabulary*.

Background knowledge measures assessed expertise in specific domains across a range of topics. Cultural literacy was assessed using a test from *Test-Prep your IQ with the Essentials of Cultural Literacy* (Zahler & Zahler, 2003) about American History, Geography, Myth/Religion, Science, and Art. Science knowledge was tested with a novel measure, *Scientist Recognition Test*, in which participants distinguished names of scientists from foils. A total of 100 names were listed and half were Nobel Prize winners (scientists) the others

were names from the National Academy of Sciences (these were categorized by Freed et al., 2017 as “foils”). Two measures of print exposure assessed reading frequency: *Author Recognition Test* (Stanovich & West, 1989) and a measure from the *Reading Habits Questionnaire* (Scales & Rhee, 2001). In the former, participants distinguished real authors (from *New York Times* Best Seller List) from foils; in the latter, participants responded to a single, self-report scale about their reading habits.

Reasoning. Reasoning measures included the first two sets from *Raven's Progressive Matrices* (Raven, 1962) and measures from the *Ekstrom Battery of Factor-Referenced Tests* (Ekstrom et al., 1976). *Raven's Matrices* consisted of 48 items in which participants induced the missing element to complete a pattern. The *Ekstrom Battery* included (a) *Arithmetic Aptitude* (two sections of 15 arithmetic word problems); (b) *Mathematic Aptitude* (two sections of 15 algebraic, multiple-choice word problems); (c) *Necessary Arithmetic Operations* (participants asked to determine which numerical operations were necessary to solve two sections of 15 items).

Perceptual Speed. In *Letter Comparison*, participants identified whether two patterns were the same or different, from two timed lists of 21 pairs of letter strings. For *Pattern Comparison* participants determined whether two lists of 15 pairs of patterns were the same or different from each other in a 30 s time limit. In *Finding As*, participants had to find five words including the letter “a”, from five columns of words in 2 min. For the *Identical Pictures* measure, participants viewed rows of geometrical figures and determined which of them were identical to the first figure in the row. Participants completed 48 rows of these figures in 2 lists for 2 min per list. Finally, for *Number*

Comparison participants viewed pairs of digit strings and indicated whether the numbers were the same in each pair.

Verbal Fluency. For *Word Beginnings*, participants reported as many words as possible that started with a specified letter within 3 min per prompt (a total of 2 prompts were given). In *Word Endings*, participants reported as many words as possible that ended with a prompted letter (2 prompts given with 3 min per prompt). Finally, for *Word Beginnings and Endings* the procedures discussed previously for Word Beginnings and for Word Endings was combined. Again, two pairs of prompts were given with a 3 min time limit.

Reading Comprehension. Measures included the reading comprehension section of *Nelson-Denny Reading Test*, which consisted of reading a passage and answering questions from either Form F (36 questions) or Form G (38 questions; Brown, Bennett, & Hanna, 1980; Brown, Fisco, & Hanna, 1993). A novel measure, the *Investigator-Generated-Test*, presented 10 multiple-choice questions that assessed memory and understanding of the main ideas and themes from 10 texts.

Data Analysis Plan

The current reanalysis plan was three-fold. The first step included conducting a replication of the seven-component PCA presented by Freed et al. (2017) to corroborate the simple structure that they reported. Next, we extended this replication by including both reading comprehension measures (i.e., the outcome variable) in an eight-component PCA (Model 1) with varimax (orthogonal) rotation consistent with the procedure followed by Freed et al. The purpose of this analysis was to reassess the original measurement model underlying the SEMs presented by Freed et al. using their raw data; this allowed us

to determine whether the component structure is consistent with Freed et al.'s results when the outcome variable is included in the model. Thus, after we replicated Freed et al.'s seven-component PCA, it was necessary to conduct a PCA with an eight-component model (Model 1) to allow for replication of a distinct reading comprehension component within the measurement model, as presented by Freed et al.

For the second step, the data was submitted to rigorous screening and cleaning using regression diagnostics¹. These diagnostics assessed whether the data were consistent with general data-cleaning standards (treatment of missing data, outliers) and regression-specific assumptions (normality, linearity, homoscedasticity, and multicollinearity). Additionally, EFA-specific assumptions (sphericity, sampling adequacy, and the determinant) were assessed (Howard, 2016) as well as the level of internal consistency amongst the measures. When conducting exploratory or confirmatory statistical techniques like EFA, CFA, or SEM, it is vital that measures with strong psychometric properties are selected and that the reliability of these assessments are reported (Field, 2009; Klein, 2016). Although there is no gold-standard for assessing the appropriate magnitude of reliability coefficients, Klein (2016) recommends the following guidelines: approximately .90 implies "excellent" reliability, .80 implies "very good", and values larger than .70 are deemed as "adequate." More information about the screening and cleaning

¹ For the sake of transparency, these diagnostics were not originally conducted before Model 1 because we wanted the data to be as consistent as possible to the dataset provided by Freed et al. (2017). In their results section, Freed et al. discussed removing univariate outliers from the data (values that were 3.00 SDs above or below the mean) but did not indicate how many outliers were identified and in what manner they were removed. It seems as though the outliers were deleted pairwise because the sample sizes vary across Freed's published SEMs, however, no information was provided to confirm this, or the number of outliers removed from each variable. Additionally, the specific sample size for Freed et al.'s PCA was not reported, thus, the current PCA (Model 1) used all of the raw data provided by Freed et al. ($N = 362$).

process will be offered in the following section. Additionally, screening and cleaning must be conducted prior to EFA to establish which extraction method is most appropriate. For example, the use of maximum likelihood extraction techniques tend to only be appropriate when assumptions of normality remain unviolated (Fabrigar et al., 1999; Howard, 2016); when assumptions of normality are violated most experts advise using extraction techniques like principal axis factoring (see Costello & Osborne, 2005; Fabrigar et al., 1999), generalized-least-squares, or weighted-least-squares (see also Osborne, 2014). The former is synonymous to an iterative PCA, where communality values are refined in steps until the model converges, ostensibly explaining both the variance and the correlations/covariance amongst measures (Tabachnick & Fidell, 2013). The latter two techniques take into account the error (i.e., uniqueness) associated with each measure to prioritize fitting highly unique variables to factors over variables with smaller values of unexplained variance (i.e., more common variables).

For the third step, a modified model (Model 2) is offered resulting from the removal of all variables that either violated the diagnostic assumptions assessed previously or demonstrated problematic communality or factor loadings in the models. The purpose of this analysis was to reassess this data following the best practices as specified by extant literature (Costello & Osborne, 2005; Howard, 2016; Osborne, 2014) to explore the underlying measurement model of the data used by Freed et al. (2017). If we fail to replicate and confirm the measurement model of Freed et al. (2017) using the same data, then the reliability and validity of their results and conclusions may be called into question. For example, the lack of an established measurement model would invalidate the SEMs that had this factor pattern as their base, thereby nullifying the subsequent conclusions Freed et

al. made from these models. Although we agree with Freed et al., that the estimates obtained from models of reading comprehension can vary greatly depending on what measures and constructs are actually included, SEMs that do not begin with a fully specified measurement model (i.e., consisting of all manifest variables and their theorized relationship with the underlying latent factors/components) cannot be used to substantiate any theoretical perspectives. Thus, a lack of a replicable measurement model would automatically undermine the conclusion from Freed et al., that vocabulary and reasoning are more integral in predicting reading comprehension than working memory capacity or word decoding. Regardless whether this statement accurately describes the processes underlying reading comprehension or not, invalidated models cannot be used as support for either perspective.

Results and Discussion

Descriptive statistics for each measure including univariate normality (i.e., skewness and kurtosis) and internal consistency values can be found in Table 1. Of immediate concern, and not reported by Freed et al., 19 of the 31 (61.29%) variables' Cronbach's alpha (α) values calculated from the original dataset failed to demonstrate adequate reliability (i.e., $\alpha \geq .70$). Bivariate correlations between measures are reported in Table 2 and values exceeding the .05 level of statistical significance are bolded. Parallel analysis (Horn, 1965; see also Crawford et al., 2010; Keith et al., 2016) was conducted prior to the PCAs to statistically determine the number of components indicated by the correlation matrix of the raw data provided by Freed et al. (2017). These parallel analyses indicated that five components were appropriate to explain the data, regardless of whether

we chose to include all of the variables (31) or only the ones included in the analyses presented by Freed et al (26).

Replication: PCA from Freed (2017)

We did not use the five-component structure despite being suggested by the parallel analysis, because our goal was to replicate the simple structure of the original PCA from Freed et al. (see pg. 142-144; 2017); thus, we used a seven-component PCA and the 26 measures included in their original publication. The seven components accounted for 61.44% of the variance in the data (compared to the 61.73% reported by Freed et al.) Next, to avoid inadvertently forcing reading comprehension and language experience to converge as one component by constraining the PCA to seven components, we also conducted an eight-component PCA that allowed us to determine if reading comprehension formed a unique component.

Consistent with Freed et al. (2017), this eight-component PCA (Model 1) with varimax (orthogonal) rotation was conducted using the 26 measures originally reported and described by Freed et al. as well as the two reading comprehension variables (28 variables total). The eight components accounted for 63.76% of the variance in the data; for specific information about the component loadings, communalities, and uniqueness values see Table 3 (also see component loadings for replication of Freed et al. PCA in parentheses). We have chosen to label the eighth component as “visual processing/storage”, due to the high loading of *Visual Number Span* and mediocre loadings of *Raven’s Progressive Matrices* and *Identical Pictures*. However, this title should be seen as a placeholder, rather than a description of a proposed latent construct. Finally, five cross-loadings were present in the component matrix originally published by Freed et al. and

four of these were replicated in our reanalysis (*Phonological Decision*, *Alphabet Span*, and *Raven's Progressive Matrices*, and *Identical Pictures*).

Model 1 reveals a serious problem that calls into question all of the models and results reported by Freed et al. (2017). Specifically, the two measures of reading comprehension failed to load onto a distinct reading comprehension component and instead loaded highest on the language experience component (see Table 3). And this was true despite including enough components for the added reading comprehension measures to form their own component. Additionally, the reading comprehension measures demonstrated cross-loadings with the reasoning, working memory capacity, and decoding components. This means that the measure of reading comprehension is not valid because they failed to establish a pattern of convergent validity (the two measures of reading comprehension do not load on their own component) and discriminant validity (the measures of reading comprehension reveal cross-loadings). This problem retroactively impacts the models presented by Freed et al. because all confirmatory LVMs reported were based on a measurement model derived without reading comprehension measures and a distinct reading comprehension component. Additionally, the confirmatory LVMs do not include cross-loadings in the measurement model which means the latent variable called “reading comprehension” is actually a combination of reading comprehension and several other components included in the model.

Although exploratory analyses typically demonstrate acceptable indices of model fit, other aspects of the results must be examined to determine whether the model accurately characterizes the data. For example, two measures in the current model produced communality estimates (i.e., the proportion of variance in a measure that can be explained

by its respective factor loadings) lower than .50, with both of these values falling near .40, often considered to be the lowest boundary for acceptable communality values (Velicer & Fava, 1998). Again, we reiterate the importance of considering the cross-loadings present in the original Freed et al. (2017) measurement model and in our Model 1. These results suggest that modifying the model by removing unreliable measures, in addition to using methods more consistent with EFA best practices, could produce a more accurate and replicable measurement model.

Extension: Data Cleaning and EFAs

Data screening and cleaning was conducted on the original data provided by Freed et al. (2017) used in Model 1. All of the R code for the data cleaning process is available online (OSF project page: <https://osf.io/urkqp/>).

Data Cleaning: Missing Data and Outliers. Regarding data-cleaning, our analyses indicated that 25.03% of the data was missing, well exceeding the benchmark of less than 5-10% missing data that can be overlooked with a large enough sample size (Rubin, 1987; Schafer, 1999; Lee & Huber Jr, 2011). Given the current analyses, it was determined that the level of missing data was untenable, so values for the missing data were imputed using a multiple imputation-chained equation technique (via the *mice* package in R; Azur et al., 2011) that uses Bayesian regression-based linear prediction to impute all of the missing data points.

Regarding outliers, multivariate outliers were given priority as they are of greater concern than the less complex univariate outliers. Multivariate outliers were identified by generating Mahalanobis distance terms for each case (Tabachnick & Fidell, 2013). In total, 17 cases were identified as having a Mahalanobis distance greater than the associated

critical value, (e.g., $\chi^2(31) = 61.09$) and were deleted list-wise from the dataset. After removal of the multivariate outliers, univariate outliers were deemed negligible as the number of univariate outliers ranged from 0% to 2% of the data. Thus, given the size of the sample ($N = 345$), no other cases were removed.

Linearity. The first regression-based assumption tested was the assumption that each predictor has a significant linear relationship with the outcome variable (i.e., both measures of reading comprehension; see Table 2 for bivariate correlations). On the one hand, several measures were not significantly associated with *Investigator-Generated Comprehension* (*Orthographic Decision*, *Visual Number Span*, *Go/No-Go*, *Stroop Interference*, *Number Comparison*, *Pattern Comparison*, and *Word Endings*). Additionally, three measures were not significantly associated with *Nelson-Denny Comprehension* (*Visual Number Span*, *Go/No-Go*, and *Pattern Comparison*). Thus, *Visual Number Span*, *Go/No-Go*, and *Pattern Comparison* tasks are inappropriate for regression-based analyses for violating the assumption of linearity. In fact, because there were only two measures of reading comprehension, the argument could be made that regardless of the dataset being used, any measure that does not have a significant relationship with *both* comprehension measures should be eliminated from subsequent modeling. Otherwise, some of the model estimates produced would only reflect pairwise associations between individual predictors and *Nelson Denny Comprehension*, rather than associations among the latent construct of Reading Comprehension. However, for the sake of maintaining as many variables as possible, only the three measures (*Visual Number Span*, *Go/No-Go*, and *Pattern Comparison*) that lacked any association with reading comprehension measures were flagged for removal.

Normality. Univariate and multivariate normality were assessed by examining skew and kurtosis values and conducting several tests designed to assess multivariate normality. Prior to data imputation, several measures in the original Freed et al. (2017) data demonstrated violations to univariate normality due to extreme values of skew (more extreme than ± 3.00) and kurtosis (more extreme than ± 10.00 ; see Table 1). Additionally, multivariate normality was not demonstrated based on triangulation amongst various well-established multivariate normality tests (e.g., Mardia, Henze-Zirkler, Royston, and Zhou-Shao; see Alpu & Yuksek, 2016; Zhou & Shao, 2014). Following data imputation, all of the variables demonstrated skew and kurtosis values within the acceptable range. However, like the raw data, multivariate normality was still not demonstrated across the multivariate normality tests used (all $ps < .05$).

Homoscedasticity and Multicollinearity. Next, Breusch-Pagan Tests were conducted on the cleaned data, indicating that the residual variances were homoscedastic (*Investigator Generated Comprehension*: $\chi^2(29) = 24.72, p = .69$; *Nelson-Denny Comprehension*: $\chi^2(29) = 25.89, p = .63$). Thus, due to the lack of multivariate normality yet equal, uncorrelated error variance, a principal axis factoring extraction method was selected for the parallel analysis and the subsequent EFA (Costello & Osborne, 2005; Fabrigar et al., 1999). Finally, for multicollinearity, the variance inflation factors (VIFs) for all of the variables were less than 5, indicating that the assumption has been maintained (James et al., 2014).

EFA-Specific Assumptions. The final step prior to conducting the factor analysis was to test the assumptions that determine whether the data and underlying correlation matrix were suitable for EFA. First, Bartlett's Test of Sphericity indicated that there were

adequate associations among the variables to be conducive for EFA, $\chi^2(465) = 3941.74, p < .001$. Next, the Kaiser-Meyer-Olkin Sampling Adequacy Test was conducted to determine whether each variable had been adequately sampled by examining the proportion of pairwise correlations for each variable that can be explained by overlap with other variables in the model (Kaiser, 1974). Although the overall sampling adequacy across items was good ($MSA = .81$), several variables demonstrated sampling adequacy that failed to reach adequate levels ($< .70$; *Orthographic Decision*, *Visual Number Span*, *Finding As*, *Number Comparisons*, and *Pattern Comparisons*). Worse, other variables demonstrated unacceptable sampling adequacy ($< .50$; *Stroop Interference*, *Go/No-Go*). Thus, these variables were flagged as inappropriate for LVM. Keeping variables in a model that show mediocre sampling adequacy can be justified if there are a lot of measures and a large sample size; but the variables with unacceptable values should be eliminated from the model. Lastly, the determinant of the correlation matrix representing the observed variables provided by Freed et al. (2017) was approximately equal to the .00001 standard. This further indicates that there is no issue of multicollinearity amongst these overlapping measures (Field, 2009).

Exploratory Factor Analysis. Prior to conducting EFA, a parallel analysis (Horn, 1965; see also Crawford et al., 2010; Keith et al., 2016) was used to statistically determine the appropriate number of latent factors present based on the cleaned data using principal axis factoring extraction method. Per the parallel analysis, the seven-factor model was deemed appropriate to explain the covariance amongst the original observed variables described by Freed et al. (2017; 31 variables total). However, due to the troublesome variables flagged during data diagnostics, best practices would dictate the removal of these

measures prior to using an EFA. Thus, (a) *Visual Number Span* and *Pattern Comparison* were removed due to the violation of linearity with both Reading Comprehension outcome variables; (b) *Go/No-Go* and *Stroop Interference* were removed for demonstrating unacceptable levels of sampling adequacy ($< .50$); and (c) *Identical Pictures* was removed due to the reported communality values present in the Freed et al. replication EFA and Model 1 (i.e., consistently among the lowest communality values of all measures considered).

Next, the data was resubmitted to parallel analysis, which indicated that a five-factor model would be appropriate to explain the covariance amongst the remaining observed variables. The EFA (Model 2) was conducted using principal axis factoring extraction and *oblimin* (oblique) rotation (see Table 4 for factor loadings $> .30$, communalities, and uniqueness values). First, considering the factor loadings for Model 2 both reading comprehension measures loaded highest on the factor representative of language experience. This corroborates the component loading patterns demonstrated for language experience in Model 1 (see Table 3). Additionally, *Phoneme Transposition* was the only other measure that loaded onto this language experience latent factor besides the original variables described by Freed et al. (2017) to be representative of language experience and the reading comprehension measures.

Second, unlike Model 1 and the original PCA conducted by Freed et al. (2017) there was only one cross-loading present in Model 2 that exceeded the criteria used by the current authors (factor loadings $< .30$; Costello & Osborne, 2005). *Nelson-Denny Comprehension* cross-loaded on language experience (.39) and a latent factor potentially representative of perceptual speed (.31). Although Model 2 maintained a simple structure,

certain factors had the issue of inconsistency, such that there were both high and low factor loadings for the underlying items (see Table 4). For example, the factor loadings present for the latent factor representative of language experience range from .32 (barely above the cutoff criteria used by the current authors) to .80 (one of the largest factor loadings present in Table 4). This highlights another issue, only seven factor loadings within the model surpassed .70. This value (and higher) represents the ideal for factor loadings because it indicates that approximately 50% of the variance in the observed variable is explained by the underlying latent construct. Although very large factor loadings are not expected for cognitive variables, the fact that so many of them are quite low could be indicative of measurement problems.

Additionally, the *Non-Word Naming* task failed to load sufficiently on any factor resulting in the largest uniqueness value, i.e., proportion of variance left unaccounted for in the measure by its respective latent variable (see Table 4). Additionally, less than 50% of the variance in 19 out of the 26 variables could be accounted for by a correlated five-factor model. Finally, because Model 2 was conducted using oblique rotation, correlation values were estimated between each factor. The correlation between working memory capacity and reasoning factors was largest ($r = .41$) followed by the correlation between language experience and reasoning ($r = .34$) and language experience and working memory capacity ($r = .33$). All factor correlations can be found in Table 5.

Conclusions

The reanalyzed PCA (Model 1) conducted on the data provided by Freed et al. (2017) demonstrated a simple structure parallel to the measurement model presented in their original publication, which we also replicated. However, the primary issue with Freed

et al.'s measurement model was that the reading comprehension measures were not included in their PCA. Thus, the component structure for reading comprehension was never established in the original publication prior to being modeled as an outcome variable in several SEMs (Flora & Flake, 2017; also see Rhemtulla et al., 2019 for more information on problematic measurement models).

When the reading comprehension measures were included in both Model 1 and Model 2, each measure consistently loaded with the language experience variables. This represents and corroborates a good example of the perspective offered by Fried (2020), that using LVM to assume or extract a simple structure can be misleading and lead to problematic applications of simple measurement models. The use of confirmatory techniques like SEM and CFA can only be trusted when they are theoretically developed and defined with construct validity in mind. Thus, it is difficult to accept the validity of the SEMs presented by Freed et al. (2017) in light of this evidence, as language experience (and to a lesser extent, reasoning) simply must be strongly predictive of reading comprehension if these constructs have not been delineated from one another and are represented by a same factor or component. Furthermore, without a distinct latent variable for reading comprehension, all of the conclusions made by Freed et al. concerning theoretical models of adult reading comprehension and the role of the underlying processes cannot be substantiated; as there is no evidence that the measures used in the model accurately characterize reading comprehension, or some other related construct. As such, this renders the theoretical conclusions drawn by Freed et al. about the model unfounded. We cannot say whether language experience (i.e., vocabulary) and reasoning are more integral for

reading comprehension than are other variables such as working memory and word decoding.

A second issue was the choice of Freed et al. (2017) to employ PCA. This analysis is not an appropriate statistical technique for making theoretical arguments or proposing theoretical solutions to a dataset (Tabachnick & Fidell, 2013) and a problematic measurement model can lead to serious issues with eventual model interpretation (Fried, 2020; Rhemtulla et al., 2019). In fact, PCA reduces the dimensionality of a dataset with no regard to the underlying latent structure of the variables (Osborne, 2014). Thus, Model 2 was our attempt at demonstrating the best practices for conducting LVM (specifically EFA) as dictated by previous research (Costello & Osborne, 2005; Flora & Flake, 2017; Howard, 2016). However, to truly achieve best practices we would still need: (a) clearly defined operational definitions with set boundaries between constructs with conceptual overlap at the stage of measurement (e.g., language experience and reading comprehension; Fried, 2020; Flora & Flake, 2019); (b) at least 3 - 4 or more indicator measures per factor (e.g., reading comprehension needs more than two indicators); (c) to include a variety of measures to better triangulate the abilities like reasoning and language experience (e.g., Freed et al. used multiple similar vocabulary tasks for language experience, and three out of four reasoning measures were all arithmetic-based; Kline, 2016; Osborne, 2014); and (d) to use adequately reliable measures, as the majority included in Freed et al. (2017) were not (e.g., 19 variables had $\alpha < .70$; see Table 1).

We demonstrated that parallel analysis coupled with principal axis factoring extraction method and oblique rotation generated a similar, but not identical, measurement model to that produced by Freed et al. (2017; PCA). However, the fact that

reading comprehension measures (outcome variable) and language experience measures (predictor variables) share a latent factor, invalidates the SEMs presented by Freed et al. and subsequent conclusions drawn from them.

Recently, an alternative statistical technique to LVM, psychometric network modeling, has gained traction among psychometricians. Network models assume that psychological constructs are emergent properties that occur due to the overlapping nature of observed variables. Much like LVMs, network modeling uses a correlation matrix as input and outputs an estimated, interconnected display of how each observed variable is related with each other variable in the network. Unlike LVMs, network modeling generates this output without proposing the existence of unobserved, latent variables. In Study 2, we further analyze the Freed et al. data to determine the interrelations among the domain-general and domain-specific measures underlying reading comprehension.

Study 2: Psychometric Network Modeling

Reading comprehension and other higher-order cognitive abilities (i.e., abilities that require multiple lower-order cognitive processes) are typically examined using LVM, because these analyses offer a means of isolating and measuring the specific contributions of each latent construct. When Spearman (1904) developed reflective factor analysis from cognitive test data, it was under the assumption that there was a common latent cause underlying performance on similar cognitive tasks (the *common cause*; Pearl, 2000; see also Fried, 2020; Schmittmann et al., 2013). In other words, scores on two similar measures are correlated because both assess the same underlying construct that is directly *causing* the level of performance that is obtained on the individual measures.

Network Approach to Cognitive Abilities and Process Overlap Theory

However, an alternative perspective to the common cause approach has recently been gathering interest in psychology, known as *network theory* (van der Maas et al., 2006; Savi et al., 2019). This viewpoint interprets the positive manifold as the result of interactions between the many cognitive processes being tapped by each measure. Recently developed theories of intelligence are consistent with the network approach (Kovacs & Conway, 2016; van der Maas et al., 2006). For example, *Process Overlap Theory* (POT; Kovacs & Conway, 2016) posits multiple overlapping domain-general and domain-specific processes underlying all cognitive abilities, and every cognitive measure taps into a different selection of these processes. Although cognitive measures are never process-pure (Ellingsen & Engle, 2019), measures of the same construct tap into many of the same processes, and all cognitive abilities require some domain-general processes to be completed (Conway & Kovacs, 2013); this is reflected in significant correlations among the measures. In other words, POT posits that the positive manifold is due to shared processes, not overarching latent constructs. Tasks that require a similar selection of domain-general and domain-specific processes will result in larger associations between the measures than do those that require fewer shared processes.

Psychometric Network Modeling

Psychometric Network Modeling is an exploratory analysis that characterizes cognitive abilities as interconnected networks of overlapping domain-general and domain-specific processes (see Epskamp, Borsboom, & Fried, 2017; Epskamp & Fried, 2018). Rather than using latent variables, each observed variable is represented by a *node* and the partial-correlations among these measures are estimated and modeled via connections called *edges* (the thickness of the edge indicates the strength of the relationship). Partial-

correlations are modeled rather than zero-order correlations, such that the magnitude of the relationship between two nodes is specifically representative of the one-to-one association between the two variables; the noise from other variables in the network that may share overlapping variance has been controlled for and filtered out. Overall, network modeling is an alternative to LVM that is more compatible with recent theories of cognitive ability (Kovacs & Conway, 2016; van der Maas et al., 2006).

Advantages of Psychometric Network Modeling

Psychometric network modeling has many advantages for studying complex cognitive abilities. First, it can be used similarly to EFA: All the variables are simply entered to generate a model structure of the data, with little direction from the researcher. However, whereas LVM techniques use latent variables to provide a general summary of the associations within a dataset, network models estimate and display every non-spurious partial correlation among all observed variables (i.e., spurious in this context is a false positive, coincidental, or very weak association). This difference offers several benefits to network models: (a) The lack of latent variables means that network models do not assume the controversial principle of local independence² (Borsboom et al., 2003); (b) Because network models estimate all associations, they allow researchers to see the interconnected

² The *principle of local independence* (Henning, 1989) posits that reflective LVMs that pose a common factor to explain the covariance among associated tasks renders these associated tasks as statistically independent from one another. Consider a researcher interested in individual differences in comprehension ability. A general comprehension factor may be extracted from measures of vocabulary, verbal comprehension, and reading comprehension. As these tasks are likely interrelated by a miscellany of domain-general and domain-specific processes, it seems unrealistic to assume that the only thing these measures have in common is this latent comprehension factor. Even more unlikely is the assumption that were it not for this common factor, tasks of comprehension would not be related at all (for more discussion of this limitation, see Borsboom et al., 2003; Cramer et al., 2010; McNally, 2016). The principle of local independence complicates the use of reflective LVM because many find the assumption of causality for latent factors to be philosophically illogical and at odds with reality.

nature of the data and to investigate all of the sources of overlapping covariance among the measures (i.e., sources of the positive manifold), finally; (c) Displaying the data as a network of associations makes this technique a powerful visualization tool to explore data, even if just used in conjunction with other analyses. For example, network models can be used to inform the factor structure for LVM, by determining whether measures belonging to the same factor truly cluster more closely together compared to dissimilar measures. In sum, network models have many benefits that make them a good option to use in place of LVM models when investigating a perspective consistent with the network approach.

Disadvantages to Psychometric Network Analysis

Although network modeling offers many advantages, there are also limitations to consider. Similar to LVM and other analyses, the presence of measurement error will negatively impact the accuracy of network models (Epskamp, Rhemtulla, & Borsboom, 2017; Wang et al., 2012). Thus, if poorly constructed measures or substandard data are used to conduct the analysis, the resulting network model will be dubious (i.e., *garbage in, garbage out*; Roden et al., 2020). As well, the values in the underlying correlation or variance-covariance matrices must be relatively large for the estimation technique to produce interpretable networks (Hevey, 2018; see also Friedman et al., 2008). The estimation process for network models includes removing spurious correlations that do not meet a certain threshold, so variables that are only weakly associated can result in a model without any edges connecting the nodes. Finally, although tutorials have been published across various psychological sub-disciplines for conducting psychometric network modeling (Costantini et al., 2015; Epskamp & Fried, 2018; Hevey, 2018), there is still a lack of familiarity with network modeling across much of psychology, including

cognitive psychology, and this can lead to inconsistencies when conducting models, and confusion when interpreting them. Overall, many limitations to network modeling are similar to other techniques, in that they require valid and reliable measures and appropriate data to yield accurate models.

Current Study

The purpose of Study 2 was to further explore the data from Freed et al. (2017) by conducting psychometric network modeling. The specific aims were: (a) to provide an example of how network modeling can be used to study higher-order cognitive abilities, in conjunction with or instead of LVM; (b) to demonstrate the type of information that can be obtained from network modeling and how it compares to the information obtained from typical LVMs, as in Study 1; and (c) to suggest an alternative interpretation of the results reported by Freed et al., which is more consistent with recent theoretical approaches to individual differences in cognitive ability.

We conducted two network models with a similar procedure as the exploratory analyses conducted in Study 1. Similar to the PCA (Model 1), the first network model (Network 1) was conducted on the original raw data from Freed et al. (2017), but rather than excluding variables (as was done in Freed et al.'s PCA and the current Model 1; see Table 3), we opted to use all 31 variables in the first network model. This was done to demonstrate how a network model could have informed Freed et al.'s latent component structure, by providing a visualization of all of the relationships in the data. Similarly, this served as corroboration of how network modeling techniques can be used to identify potential problems with the model, or the variables included. Thus it was necessary to include the outcome measures for reading comprehension to inform the full measurement

model underlying the models from Freed et al. Additionally, the other variables that were eliminated from the Freed et al. model for inappropriate loadings were also included to demonstrate how network modeling can be used as a data-visualization tool to identify troublesome variables.

Next, to emulate the EFA conducted in Study 1 (Model 2), a second network model (Network 2) was conducted using the dataset that had been cleaned consistent with best practices. The model was conducted after removing any nodes from the first network model (Network 1) that were deemed inappropriate at face value, due to a lack of connections with other nodes. Using the clean data for the second model also allowed for comparisons with the network conducted on the raw data; this highlighted the improved interconnectedness of the second network.

Method

Data and Participants. As discussed in Study 1, sample size varied between analyses conducted on the original raw data ($N = 362$) and the cleaned data ($N = 345$). For more information on the differences between these datasets see Study 1.

Measures. All measures were the same from Study 1.

Data Analysis Plan. Analyses were conducted on the same dataset used in Study 1, originally collected by Freed et al. (2017). The first network model was conducted on the original, raw correlation matrix consistent with the current PCA conducted in Study 1 (Model 1; see Table 2), except that all 31 variables were included. This network model (Network 1) was to determine whether we obtain consistent results with the component structure submitted by Freed et al. It will also allow for the comparison between a network model conducted on the raw data (Network 1) versus a network model conducted on data

that has been cleaned consistent with best practices (Network 2). The second network model (Network 2) was conducted on the correlation matrix extracted from the clean data, however, four variables were removed based on a lack of edges in Network 1 (27 variables included). In particular, variables were removed from the network when they failed to demonstrate at least three partial correlations across the entire network. Thus, *Stroop Interference*, *Go/No-Go*, *Non-Word Naming*, and *Pattern Comparison* variables were removed prior to conducting Network 2. The analyses were conducted and visualized using the *qgraph* package in R (Epskamp et al., 2012).

For the current network models, the cognitive measures were assigned group labels consistent with the Freed et al. (2017) component structure (see Table 3), with each group coded by a color (e.g., all working memory capacity nodes are presented in *orange*). It would be expected that a node's thickest edges (largest partial-correlations) would be for measures that belong to the same factor or construct (consistent with node color), and these strong associations among multiple nodes would promote a tighter cluster. Color-coding the nodes facilitates noting trends in the data, such as whether a particular node is clustering more closely with measures of a different construct rather than its own group. This will allow us to further assess the component structure established by Freed et al., and whether the same information that was determined using LVM in Study 1, is corroborated by the outcome of network modeling.

Both network models were conducted to be consistent with the network model tutorial from Epskamp and Fried (2018). Prior to conducting the analysis, it is necessary to set two parameters: (a) gamma [γ], the hyperparameter determines whether the model will be more complex with many edges, or have a simple structure with fewer edges; (b)

and lambda [λ], the tuning parameter determines how strict the threshold is for determining which edges are spurious and subsequently removed. The current network models used the graphical least absolute shrinkage and selector operator (gLASSO) regularization method, which automatically incorporates the most conservative gamma setting (i.e., .50). Consistent with Epskamp et al. (2018), lambda was set moderately to remove false-positive edges while maintaining as many true edges as possible (i.e., .01).

Results and Discussion

For descriptive statistics and correlations amongst the variables included in the network model, see Table 1 and Table 2, respectively. The first network model (Network 1) was conducted on the original raw version of the data, containing all 31 variables (see Figure 1 for the network structure visualization). Per Kline (2016), Network 1 displayed excellent fit to the data, although this is to be expected given that it is an exploratory analysis.

Many aspects could be explored using network modeling, but for the purpose of the current study three characteristics were examined to assess the network: (a) whether nodes of the same color clustered closely together, to determine if the network layout corroborates the component structure established by Freed et al. (2017; see Table 3 for latent factor/component groupings); (b) the number of edges and strength (thicker and darker edges are of greater magnitude) of the relationship(s), to evaluate which nodes tend to be strongly interconnected (many edges/thick edges; centered) or less relevant to the network structure (few edges/thin edges; periphery); and (c) gauge whether any unusual or unexpected patterns exist within the network, e.g., not having an interconnected

network structure, such that the nodes are clustered with their own group, but not connected across groups.

When examining Network 1 (see Figure 1), many of the node clusters appeared to be fairly consistent with the component structure established by Freed et al. (2017), as nodes of the same color were mostly grouped together. However, when scanning the network structure for irregularities, there were several nodes that were not clustered with any other nodes and only had two or less, weak associations binding them to the rest of the network including: *Stroop Interference* (inhibition), *Go/No-Go* (inhibition), *Pattern Comparison* (perceptual speed) and *Non-Word Naming* (decoding). A lack of edges indicates that these nodes are not strongly related to other variables in the data, and likely inappropriate for this particular network of variables (for this sample at least). These findings were consistent with the problematic measures that were identified in Study 1, either through the diagnostic process or as a result from the models (see Model 1, Table 3 and Model 2, Table 4). For example, it was determined that some of these isolated nodes were also the measures that had deficient sampling adequacy (*Stroop Interference*, *Go/No-Go*) or factor loadings (*Pattern Comparison*) in Study 1. Although this node was not removed from the models in Study 1, a fourth measure (*Non-Word Naming*) did not have adequate factor loadings for any factor in the final model (see Model 2). This node would likely have been removed in the next step if we continued to further refine the model structure.

Although, the lack of interconnectedness for *Stroop Interference*, *Go/No-Go*, *Pattern Comparison*, and *Non-Word Naming* was fairly obvious, there were other more nuanced patterns in the network that should also be considered, involving: *Identical Pictures*

(perceptual speed), *Phoneme Transposition* (decoding), *Raven's Progressive Matrices* (reasoning), *Visual Number Span* (working memory capacity; see Figure 1). For *Identical Pictures*, this node had several more visible edges than the nodes discussed previously, however the associations appeared to be very small, especially compared to the connections among the other perceptual speed nodes. The lack of similarity between *Identical Pictures* and the other perceptual speed measures demonstrated in Network 1 may have contributed to the overall lack of strong factor loadings and the cross-loadings demonstrated between perceptual speed and pattern recognition present in Study 1 for *Identical Pictures*.

Next, *Phoneme Transposition* failed to cluster with the other decoding nodes, and clustered instead with the language experience nodes. This indicates that *Phoneme Transposition* has stronger associations with the language experience nodes, likely because they measure overlapping constructs (i.e., verbal abilities). Given the proximity and connections to the language experience nodes, it was unsurprising that in Study 1, *Phoneme Transposition* loaded with the other language experience measures in Model 2 (see Table 4).

Additionally, the reasoning node, *Raven's Progressive Matrices*, seemed to be somewhat split between the working memory capacity and reasoning clusters in the network; and this was consistent with the fact that *Raven's Progressive Matrices* double-loaded with both the working memory capacity and reasoning variables in Freed et al.'s PCA. Part of the issue is that all of the reasoning measures were arithmetic-based other than *Raven's Progressive Matrices*; using a broader mix of reasoning tasks may have resulted in a more cohesive reasoning cluster/factor.

Finally, although the *Visual Number Span* node has a lot of connections to the measures it is similar to (i.e., working memory capacity and reasoning tasks), it was not interconnected with other variables in the network, especially reading comprehension. This is consistent with the results of the diagnostics in Study 1, which determined that this variable did not have a linear relationship with any measure of reading comprehension. However, this is not an assumption for network modeling, so *Visual Number Span*'s lack of a relationship with the outcome variable does not automatically bar it from use in the network.

However, the most important result that can be taken from looking at this network is the corroboration of the results from Study 1, that the language experience measures belong to the same construct as the reading comprehension measures. The close clustering via strong connections between language experience and reading comprehension is consistent with the results from Model 1 and 2, indicating that all of these variables belong to the same component/factor. This definitely calls into question the conclusions made by Freed et al. (2017) about the associations between the latent factors representing language experience and reading comprehension, because these concepts were never clearly delineated from one another. Overall, although much of the original component structure established by Freed et al. was maintained in the network, there were still many aspects that contradicted their results and in general there was a lack of connectedness for many measures. For comparison, a second network model was conducted, after data-cleaning, diagnostics, and the removal of inappropriate nodes.

A second network model (Network 2; see Figure 2) was conducted on the cleaned data, using all the parameter settings as Network 1. Prior to conducting the analysis, the

four nodes that had two edges or fewer in the previous model were removed (*Stroop Interference*, *Go/No-Go*, *Pattern Comparison*, and *Non-Word Naming*). In general, the 27-variable network model conducted on the clean data (Network 2) appears to be more interconnected than Network 1. However, there were still nodes that were not integrated with the other nodes and seemed to only be bound to the network via associations with the measures of their same construct (*Orthographic Decision* [decoding], *Word Beginnings-and-Endings* [verbal fluency]). Additionally, the decoding nodes did not cluster with the fluency nodes, which may seem to contradict Study 1 at face value. Although the fluency and word decoding variables loaded onto the same factor (see Model 2 in Study 1), it was a poorly constructed factor (communalities ranged from .27 to .42); the network model accurately reflected this by modeling the numerous weak associations between the two constructs; yet the lack of clustering between these nodes corroborated that the decoding-fluency factor was likely not a very meaningful factor.

There were other nodes in Network 2 that did not cluster consistent with the initial component structure presented by Freed et al. (2017; see Table 3 and Figure 2). First, *Phoneme Transposition* (decoding) clustered more closely with the language experience nodes and had no edges connecting the measure to the other decoding measures; this was consistent with Study 1, as *Phoneme Transposition* loaded with the language experience variables (Model 2, see Table 4). Next, *Identical Pictures* did not truly cluster with any other nodes as it had only a handful of weak associations with nodes of various constructs (including perceptual speed); this outcome was also consistent with Study 1 as *Identical Pictures* was dropped from the initial EFA for failing to load adequately with any factor. Finally, the reading comprehension nodes were still clustered closely with the language

experience nodes, in fact the strongest association *Investigator-Generated Comprehension* had was with a language experience node (*Extended Range Vocab*). Again, these results are also reflected in Study 1, as the reading comprehension nodes and language experience nodes formed one component/factor (see Model 1 and Model 2).

Conclusions

The first goal of the study was to provide an overview of psychometric network modeling and how these techniques can be used to assess the processes underlying reading comprehension and other higher-order cognitive abilities. Indeed, network modeling provides a visualization of all relationships among the measures while also assessing the strength/direction of these associations via edge characteristics and clustering. The example illustrated how these characteristics can be used to inform/confirm a factor structure or to identify variables that are not associated with the expected constructs or with the rest of the network. The visualization aspect may also reveal certain patterns and associations in the data that have not been considered and may not be as distinguishable using traditional LVM. Psychometric network modeling can be used as a data-visualization tool prior to analysis, in order to triangulate results in conjunction with other analyses, or as a stand-alone modeling technique.

A secondary goal was to compare the information obtained from the network models with the outcomes obtained in Study 1 and by Freed et al. (2017). The network models allowed for identification of troublesome measures consistent with the majority of those found during the data-cleaning process and the LVMs in Study 1 (*Visual Number Span* is the one exception, but its lack of a relationship with the outcome variable was not problematic for network modeling). Additionally, the network model corroborated the

results obtained from the LVMs in Study 1 concerning the underlying structure of the data; including corroborating that language experience and reading comprehension would belong to the same component/factor. Ultimately this outcome would call into question the conclusions presented by Freed et al., because associations between language experience and reading comprehension can be attributed to them not being delineated as constructs. This is not to suggest that language experience is not actually related to reading comprehension, rather that the results from the models conducted on the current data cannot be taken as valid indicators of that relationship. As an aside, although we have demonstrated that the information obtained using network models is comparable to that of LVM, the differences between these analyses advantages network modeling: (a) Network modeling provides information that is obscured when using traditional LVM techniques. For example, the associations between individual variables that belong to different factors are not accounted for with LVM (beyond contributing to factor covariance), but these one-to-one relationships are displayed when using network modeling techniques. (b) The information provided by network modeling is more accessible and obvious. Experienced researchers may easily notice and interpret patterns amongst the factor loadings for an EFA, but the visual aspects of network modeling allow even those less familiar with expected loading values to notice associations and irregularities. Overall, the network models corroborated much of the component structure established by Freed et al. but was consistent with Study 1 in demonstrating that the outcome variable had not been properly differentiated from one of the predictors.

A tertiary goal was to present an alternative approach to examining the processes underlying cognitive abilities that is consistent with network theory as opposed to the

common cause approach (Epskamp, Borsboom & Fried, 2017; Fried, 2020). Although the assumption is baked into the design of LVM, very few researchers explicitly indicate that they endorse the common cause perspective. In truth, many researchers likely view LVM procedures in purely practical terms as a mathematical/statistical tool, while overlooking the underlying theoretical implications of causality that are saddled with these techniques. Perhaps if there were more analytical options in the field supporting alternative perspectives (i.e., network theory and associated analyses), there would be more researchers interrogating their theoretical perspectives and subsequently choosing analyses to be more consistent with their predictions, rather than defaulting to LVM. For example, theories like POT (Conway & Kovacs, 2013; Kovacs & Conway, 2016), that posit overlapping domain-general and domain-specific processes would be more accurately characterized and tested using psychometric network modeling, compared to LVM. Overall, the current study demonstrated how network models depict the interconnected nature of all of the associations within the data, making it particularly congruous with the network theory approach (Borsboom & Cramer, 2010) to cognitive abilities and theories like POT (Kovacs & Conway, 2016).

General Discussion

The purpose of the current project was to discuss the merits and limitations of two types of statistical modeling that can be used to study individual differences in reading comprehension. To highlight the similarities and differences between these techniques, a reanalysis of data provided by Freed et al. (2017) was conducted using both LVM (Study 1) and psychometric network modeling (Study 2). The data consisted of measures of domain-general processes (*perceptual speed, inhibition, working memory capacity, and reasoning*)

and domain-specific processes (*decoding, verbal fluency, language experience*) underlying reading comprehension. Study 1 demonstrated that, using PCA and EFA, the reading comprehension measures did not form a distinct factor or component separate from language experience; yet, even after data-cleaning and employing best practices, communalities and factor loadings indicated that the final factor structure did not fully suit the data. In Study 2, the psychometric network models largely confirmed the same troublesome variables and factor structure determined in Study 1, including the lack of a distinct reading comprehension construct.

In Study 1, the Freed et al. (2017) measurement model was reassessed by first replicating their initial seven-component structure and then adding in the outcome variable to generate an eight-component model (Model 1; see Table 3), all while maintaining consistency with Freed et al.'s approach to PCA. After rigorous cleaning and diagnostics were implemented on the data, the measurement model was further critiqued using an alternative EFA approach consistent with current best practices (Model 2; see Table 4). Both exploratory LVMs produced a similar structure to the original measurement model established by Freed et al., but both techniques, as well as data-cleaning, identified multiple variables that were inappropriate for the model. The models in Study 1 also confirmed that when including both reading comprehension measures in the model, language experience and reading comprehension formed one component or factor. Despite following best practices, the final EFA (Model 2) still produced factors with low communalities and factor loadings, and this points to a deeper underlying issue concerning the operational definitions and measures employed in the initial data collection. This conjecture is further confirmed by the lack of reliability in the observational measures used

by Freed et al. (2017; see Table 1), and this issue was compounded by the fact that more than two measures should have been used for the reading comprehension outcome construct. The data cleaning and assumption-checks also emphasized how models can produce very different estimates depending on the treatment of missing data, outliers, and non-normal data. In general, Study 1 demonstrated the importance of being specific when operationally defining constructs and to be discriminating when choosing multiple highly reliable measures for these constructs; and this should be done in addition to accounting for missing and non-normal data, as well as other assumption violations. Adhering to the existing best practices will make for more accurate estimates of the associations between the processes underlying reading comprehension.

In Study 2, an alternative perspective and analysis was offered for assessing the variables involved in reading comprehension, psychometric network modeling. The network models served not only as a reanalysis of the data and component structure assessed by Freed et al. (2017), but also demonstrated how these techniques can be used to study higher-order cognitive abilities and the processes underlying them. As such, rather than proposing a common cause underlying measures of the same construct, as is the case with reflective LVMs, network theory suggests that interactions between individual processes are the driving force behind associations among parallel measures (Fried, 2020; Epskamp, Borsboom, & Fried, 2017; Epskamp & Fried, 2018). This is consistent with POT, which proposes that the positive manifold can be attributed to a sampling of shared domain-general processes (Conway & Kovacs, 2013; Kovacs & Conway, 2016). Beyond offering an alternative philosophical perspective, the network models also demonstrated how this technique can be used to spot variables that do not have strong associations to

other variables in the data and/or are inconsistent with a proposed factor structure. As the current network models highlighted the same issues with particular measures included in the model and the lack of a distinct reading comprehension construct. As with Study 1, the difference in interconnectivity between the two networks also emphasizes the variation in estimates that can occur between analyses conducted on either raw versus clean data. Overall, Study 2 confirmed Study 1 with the issues found for specific measures in addition to also supporting the fact that language experience and reading comprehension formed one construct in the model.

In conjunction with one another, both studies highlight several key points. First, there are gains and losses associated with different analyses chosen to study cognitive abilities and these aspects must be acknowledged and considered when using these techniques. Second, network modeling, a relatively new statistical technique, can be used in ways that will benefit data-exploration and traditional statistical modeling. Third, network modeling is consistent with the network theory approach to cognitive abilities, an alternative to common cause, and therefore is a more useful analysis for assessing perspectives consistent with this approach (i.e., POT; Schmank et al., 2019). Fourth, although reanalysis of the Freed et al. (2017) data indicates that their component structure was somewhat replicated, there were many issues with their measurement model, such as the inclusion of inappropriate variables, low communalities, and poor reliability of the measures. However, the largest, and final, issue was that both LVMs in Study 1 and the network models from Study 2 indicated that there was not a clearly defined outcome variable, as the reading comprehension measures loaded/clustered with language experience measures. Rhemtulla et al. (2019) demonstrated that inappropriate

measurement models can lead to biased statistical claims and general misrepresentation of the various psychological constructs that are of interest. We believe that the failure to establish a measurement model with a distinct reading comprehension factor or component invalidates the theoretical claims made by Freed et al. based on their SEMs, such as "[t]he authors question the need to include [working memory capacity] in our theories of variability in adult reading comprehension and highlight the need to understand precisely how vocabulary facilitates comprehension" and "our results raise significant concerns about the unique role of [working memory capacity] in comprehension, suggesting that its contribution is secondary to general reasoning ability" mentioned in their Abstract and Conclusion, respectively. Such a strong conclusion cannot be made given that the measurement model did not accurately characterize the variables being studied and therefore were invalid. Indeed, the current study further established the necessity of adhering to best practices when developing measurement models; going forward researchers interested in assessing various theoretical models of reading comprehension (and other cognitive abilities) must be more deliberate in the treatment and analysis of data to be consistent with standards established within that particular research area.

Limitations and Future Directions

As with any study, particularly a secondary data analysis, there were limiting factors that could have impacted the results. For example, we had no control over how the latent constructs were operationally defined nor the number and type of measures selected for each component. More attention should have been given to delineating the language-related variables due to their conceptual overlap. Additionally, measures should have been

selected on the criteria of being both highly reliable and sharing construct validity with other measures of the same construct. However, it is also imperative that a variety of measurement approaches or modalities are pursued by researchers; this is because measures that are too similar would not provide new information to the model and might overlap in measurement error with other measures. In sum, the fact that the constructs were not clearly delineated, the majority of measures were not reliable, and the outcome variable only had two underlying indicators likely limited the accuracy of the model estimates.

Another limitation was that we did not have all the information necessary to claim the reanalysis in Study 1 was a *pure* reproduction of Freed et al. (2017). For example, we do not know the sample size used in the PCA conducted by Freed et al., nor which outliers were removed from the total dataset. Additionally, issues around normality and missing data were not accounted for by Freed et al., beyond indicating that the missing data were missing at random. Because of this, we cannot be certain that each step of our analysis was consistent with Freed et al. However, the replication was consistent with all of the information that was presented in the original study.

A final limitation for the current study was that psychometric network modeling is a relatively new analysis and has had limited use in cognitive psychology (Kan et al., 2019; Schmank et al., 2019; van der Maas et al., 2017). Although this study attempted to demonstrate best practices by remaining consistent with Epskamp and Fried (2018), the lack of previous cognitive-related research using network modeling allows that we may currently be ignorant of issues that specifically affect implementation of cognitive networks. In summation, there are not many examples of cognitive network models on

which to base the current models; thus, if the use of network modeling increases in cognitive psychology, the specific requirement of best practices will likely become more refined.

Future research should continue investigating the processes underlying cognitive abilities using multiple types of modeling to compare outcomes and the philosophical implications of different approaches. It can be difficult to think beyond perspectives that have dominated the field and have always been assumed to be correct, but research that assesses multiple theoretical frameworks at once is simply a more powerful method for evaluating cognitive phenomenon. Using network modeling in conjunction with other types of LVM may illuminate associations and missteps that would not otherwise be considered. Consistent with this, alternatives to the common cause approach to cognitive abilities should also be considered, along with theories that are consistent with network theory, such as POT. Allowing for greater exploration of these perspectives, researchers at the heart of the psychometric network modeling movement have expanded the scope of network modeling to include confirmatory statistical techniques as well as directed network models and multi-level networks. This growth reminds us to continue striving for novel ways of thinking about the constructs we study and to be open to multiple methods for exploring our data.

Overall, psychometric network modeling represents a perspective that could benefit cognitive psychology, as an alternative way of conceptualizing cognitive abilities. Rather than assuming a common underlying cause, network models depict the interactive nature of the processes underlying a complex ability, like reading comprehension. However, regardless of the choice in modeling technique, more attention needs to be paid to cleaning

data appropriately, mitigating issues in the data beyond solely acknowledging them, and ensuring that the assumptions specific to the analysis being used have been met. The researcher's chosen analysis plan should be grounded and explicitly justified by the results of the data diagnostics and previous research in the area. Relatedly the measures selected for inclusion must also be justified and demonstrated to be theoretically valid, reliable and necessary. The current project further confirms the need for these best practices; although the overall structure established by Freed et al. (2017) was replicated, it was revealed that there were multiple measurement issues and a lack of a distinguished latent constructs or an established measurement model. In sum, using best practices when cleaning and analyzing data, combined with using multiple modeling techniques will result in more accurate and consistent conclusions when studying higher-order cognitive abilities such as reading comprehension.

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Table 1

Descriptive Statistics from Cleaned Data Provided by Freed et al. (2017)

Variables	Latent Construct	<i>M</i>	<i>SD</i>	Skew	Kurtosis	α
Orthographic Decision	OD	0.76	0.14	1.02	3.05	.34
Phonological Decision	PD	2.68	1.15	1.18	3.42	.38
Non-Word Naming	NN	1.04	0.39	0.38	0.96	.47
Phoneme Transposition	PT	46.84	7.03	-1.90	8.95	.55
Reading Span	RS	38.40	8.63	-0.08	-0.37	.62
Alphabet Span	AS	73.53	12.56	-0.55	1.07	.58
Minus Span	MS	148.41	23.17	-1.16	3.64	.56
Visual Number Span	VNS	8.34	1.80	0.93	2.84	.70
Go-No-Go	GNG	8.62	8.77	0.93	0.87	.13
Stroop Interference	SI	0.10	0.09	0.54	2.24	.13
Author Recognition Test	ART	8.81	5.72	0.33	0.28	.84
Reading Habits Questionnaire	RQ	3.55	0.94	-0.22	-0.52	.86
Cultural Intelligence	CI	58.33	14.02	-0.23	-0.12	.82
Scientist Recognition Test	SRT	12.55	5.62	0.16	-0.12	.86
Extended Range Vocabulary	ERV	15.17	7.24	0.11	-0.35	.82
Advanced Vocabulary	AV	12.24	5.35	0.02	-0.20	.83
Nelson-Denny Vocabulary	NDV	58.13	15.63	-0.23	-0.39	.83
Raven's Progressive Matrices	RPM	34.91	6.38	-0.29	0.13	.87
Arithmetic Aptitude Test	AAT	15.01	5.86	0.11	-0.42	.85
Mathematic Aptitude Test	MAT	11.07	4.73	0.24	0.06	.74
Necessary Arithmetic Operations	NAO	15.48	5.40	-0.14	-0.39	.74
Letter Comparison	LC	35.25	8.76	0.28	2.64	.65
Finding As	FA	61.68	17.00	0.34	0.93	.69
Number Comparison	NC	27.23	6.29	0.39	0.80	.64
Pattern Comparison	PC	58.16	2.72	-1.19	4.47	.36
Identical Pictures	IP	68.51	13.84	-0.13	-0.36	.60
Word Beginnings	WB	26.20	7.86	0.40	0.11	.56
Word Endings	WE	31.89	7.95	-0.05	-0.17	.59
Word Beginnings & Endings	WBE	19.76	7.01	0.68	2.50	.60
Nelson-Denny Comprehension	NDC	0.80	0.16	-0.85	0.94	.47
Investigator-Generated Comprehension	IGC	0.77	0.09	-0.67	0.26	.47

Table 2

Correlation Matrices of Raw Data Provided by Freed et al. (2017) and Cleaned Data Used in Reanalysis

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
1. OD	—	.52	.26	-.20	.05	-.14	-.12	-.04	-.17	.05	-.18	.09	-.17	-.10	-.14	-.09	-.22	-.05	-.16	-.11	-.05	-.17	-.19	-.26	-.15	-.16	-.24	-.24	-.25	-.30	-.06
2. PD	.48	—	.20	-.23	-.07	-.11	-.10	.10	-.09	.13	-.22	.03	-.32	-.18	-.25	-.20	-.30	.07	-.06	-.06	-.09	-.13	-.14	-.06	-.07	-.16	-.31	-.30	-.29	-.34	-.22
3. NN	.24	.14	—	-.10	-.12	-.09	.00	-.02	-.15	.12	-.08	.00	-.21	-.07	-.16	-.13	-.25	.01	-.11	-.09	-.11	-.10	-.06	-.14	-.05	-.06	-.12	.00	-.13	-.19	-.11
4. PT	-.09	-.12	-.17	—	.17	.15	.23	.13	.07	-.09	.31	.20	.28	.10	.27	.22	.32	.19	.21	.21	.25	.13	.17	.07	.06	.06	.22	.09	.18	.29	.26
5. RS	.08	.01	-.14	.16	—	.45	.41	.21	-.12	.01	.12	.13	.27	.15	.29	.19	.28	.25	.28	.22	.31	.10	.06	.01	.12	.09	.18	.15	.19	.17	.35
6. AS	-.12	-.09	-.18	.24	.44	—	.27	.23	-.06	.02	.25	.00	.28	.20	.31	.25	.33	.32	.31	.26	.33	.12	.08	.04	.04	.13	.31	.33	.28	.16	.34
7. MS	-.14	-.08	-.12	.19	.46	.41	—	.33	.12	.00	.08	.04	.16	.10	.14	.05	.18	.33	.26	.29	.32	.09	.13	.15	.08	.10	.21	.15	.14	.18	.27
8. VNS	-.06	.09	-.03	.11	.16	.30	.34	—	-.01	-.03	.02	.04	-.03	-.01	-.01	-.01	-.03	.31	.22	.16	.11	.06	.11	.16	.03	.17	.11	.09	.09	-.03	-.04
9. GNG	-.18	-.02	-.20	.03	-.13	-.02	.13	-.04	—	-.10	-.06	-.02	.04	-.04	-.05	-.05	-.05	-.08	-.02	.03	-.02	.01	.00	.04	-.04	.08	-.05	-.08	-.04	.04	-.05
10. SI	.06	.15	.17	-.08	.00	-.02	.00	-.02	-.13	—	.00	.07	-.10	-.08	-.06	-.16	-.10	.04	-.11	-.02	-.08	-.05	-.17	-.11	-.04	.02	.16	-.02	-.03	-.22	-.04
11. ART	-.13	-.10	-.19	.35	.16	.30	.05	.06	-.05	-.03	—	.35	.51	.36	.58	.46	.54	.14	.13	.16	.24	.14	.16	.03	.15	.16	.24	.23	.26	.36	.37
12. RQ	.11	.04	-.10	.32	.18	.11	.04	.12	-.01	-.04	.42	—	.36	.17	.26	.28	.35	-.02	-.10	-.02	.03	.12	.11	-.08	-.06	.04	.07	-.04	-.06	.13	.21
13. CI	.00	-.24	-.29	.27	.25	.29	.14	.00	.00	-.07	.50	.38	—	.48	.64	.57	.65	.12	.27	.39	.37	.14	.13	-.05	.12	.22	.33	.23	.29	.49	.50
14. SRT	-.06	-.05	-.16	.12	.19	.31	.11	.07	.00	-.08	.37	.20	.44	—	.43	.36	.37	.15	.20	.22	.17	.02	.00	-.05	.14	.05	.18	.13	.21	.22	.31
15. ERV	.07	-.19	-.27	.23	.27	.30	.08	-.01	-.05	-.07	.55	.32	.64	.42	—	.67	.69	.22	.29	.35	.37	.17	.13	-.12	.18	.17	.37	.28	.37	.36	.59
16. AV	.01	-.12	-.17	.24	.16	.28	-.02	.07	-.12	-.13	.43	.35	.57	.39	.63	—	.57	.14	.24	.26	.28	.14	.06	-.04	.12	.14	.32	.24	.29	.38	.47
17. NDV	-.16	-.24	-.28	.33	.25	.27	.14	.01	-.09	-.16	.52	.39	.64	.35	.66	.52	—	.21	.30	.29	.43	.08	.14	-.05	.16	.24	.38	.23	.36	.50	.51
18. RPM	-.05	.06	-.06	.18	.24	.34	.31	.28	-.02	.01	.17	.07	.16	.22	.22	.18	.22	—	.49	.51	.45	.11	.11	.16	.19	.26	.15	.18	.23	.24	.27
19. AAT	-.12	-.08	-.13	.17	.25	.31	.25	.29	-.04	-.08	.17	-.03	.25	.22	.25	.28	.26	.48	—	.71	.63	.21	.13	.25	.03	.17	.25	.16	.24	.27	.29
20. MAT	-.03	-.04	-.14	.19	.23	.31	.29	.25	-.06	.05	.20	.05	.38	.26	.33	.32	.31	.47	.72	—	.61	.19	.15	.21	.09	.20	.26	.20	.22	.31	.35
21. NAO	-.01	-.07	-.09	.22	.31	.34	.26	.18	-.11	-.05	.28	.13	.38	.21	.39	.34	.41	.37	.62	.63	—	.21	.18	.17	.11	.22	.24	.27	.22	.38	.36
22. LC	-.16	-.15	-.03	.12	.13	.15	.16	.10	-.03	-.01	.14	.11	.14	.01	.20	.13	.10	.20	.21	.20	.26	—	.33	.23	.00	.18	.17	.11	.01	.18	.10
23. FA	-.12	-.11	-.03	.13	.04	.16	.14	.06	-.08	-.08	.11	.10	.10	-.03	.10	.06	.09	.09	.16	.14	.22	.42	—	.38	.11	.26	.27	.18	.16	.18	.12
24. NC	-.27	-.06	-.10	.06	.05	.14	.11	.19	.01	.00	.12	-.10	-.07	-.01	-.13	-.03	-.02	.08	.25	.20	.15	.27	.36	—	.05	.20	.12	.14	.12	.24	-.03
25. PC	-.05	.06	.02	.03	.12	.04	.09	.09	-.17	.10	.12	.02	.12	.23	.17	.16	.14	.24	.13	.17	.11	-.03	.03	.00	—	.08	.17	.12	.18	.08	.08
26. IP	-.13	-.13	-.05	.15	.12	.17	.05	.16	.04	.07	.14	.08	.20	.03	.16	.14	.25	.24	.20	.17	.27	.24	.23	.24	.05	—	.19	.20	.16	.16	.13
27. WB	-.12	-.26	-.11	.27	.14	.28	.27	.18	-.09	.14	.30	.13	.35	.21	.35	.32	.39	.24	.30	.32	.26	.19	.22	.13	.16	.19	—	.48	.44	.26	.29
28. WE	-.16	-.28	.01	.12	.11	.30	.17	.11	-.14	.00	.29	.03	.20	.07	.20	.14	.18	.11	.15	.13	.23	.07	.14	.11	.00	.20	.43	—	.44	.20	.19
29. WBE	-.20	-.26	-.13	.15	.20	.25	.20	.11	-.07	.00	.28	-.01	.29	.24	.35	.26	.35	.19	.28	.24	.19	.06	.06	.12	.11	.17	.42	.39	—	.21	.25
30. NDC	-.24	-.33	-.17	.33	.20	.20	.18	.03	.00	-.24	.38	.16	.46	.17	.36	.39	.44	.29	.29	.35	.35	.29	.18	.22	.10	.11	.27	.16	.20	—	.48
31. IGC	-.01	-.16	-.16	.28	.32	.37	.22	-.03	-.05	-.10	.28	.25	.48	.30	.56	.48	.50	.34	.32	.37	.43	.20	.18	.02	.10	.16	.26	.08	.18	.47	—

Note. Values above the diagonal represent correlations from the raw, uncorrected data. Values below the diagonal represent correlations from the corrected data. Bolded values indicate statistically significant correlation coefficients ($p < .05$)

PROCESSES UNDERLYING READING COMPREHENSION

Table 3

Model 1 Principal Component Loadings, Communalities (h^2), and Uniqueness (u^2) Values Following Addition of Reading Comprehension Measures

	Decoding	WMC	Inhibition	Language Experience	Reasoning	Perceptual Speed	Fluency	Pattern Recognition	h^2	u^2
OD	.76 (.71)								.68	.32
PD	.62 (.63)						-.36 (-.38)		.64	.36
NN	.69 (.71)								.57	.43
RS		.79 (.73)							.70	.30
AS		.53 (.54)					.34 (.32)		.53	.47
MS		.64 (.68)							.56	.44
VNS		(.63)						.81	.74	.26
SI			.94 (.94)						.89	.11
ART				.73 (.71)					.59	.41
RQ				.60 (.61)			(-.31)		.68	.32
CI				.78 (.79)					.70	.30
SRT				.56 (.54)					.43	.57
ERV				.79 (.80)					.73	.27
AV				.74 (.74)					.62	.38
NDV				.77 (.77)					.71	.29
RPM					.67 (.66)			.36	.62	.38
AAT					.84 (.85)				.75	.25
MAT					.84 (.85)				.76	.24
NAO					.73 (.74)				.69	.31
LC						.68 (.59)			.55	.45
FA						.75 (.76)			.66	.34
NC						.54 (.60)			.57	.43
IP						.33 (.47)		.38	.44	.56
WB							.61 (.59)		.63	.37
WE							.79 (.77)		.68	.32
WBE							.67 (.68)		.60	.40
NDC*	-.32			.45	.31				.55	.45
IGC*		.36		.58					.60	.40
Prop. Var.	.07	.06	.05	.17	.11	.06	.08	.04	Total Var.	.64

Note. Values presented in parentheses are from the pure replication of Freed et al. (2017) prior to the inclusion of reading comprehension measures. * indicate reading comprehension variables not originally included in the PCA published by Freed et al. (2017). Model fit: $\chi^2(182) = 757.78$, $p < .001$; $\chi^2/df = 4.16$; TLI = .55; RMSEA = .10; RMSEA 90%CI [.10, .11]; BIC³ = 713.06.

³ Sample Size adjusted BIC was calculated for each model using the following equation:

$$BIC = \chi^2 - (df * \frac{\log(N + 2)}{24})$$

PROCESSES UNDERLYING READING COMPREHENSION

Table 4*Model 2 (EFA) Principal Axis Factor Loadings, Communalities (h^2), and Uniqueness (u^2) Values*

	Decoding & Fluency	WMC	Language Experience	Reasoning	Perceptual Speed	h^2	u^2
OD	-.59					.40	.60
PD	-.60					.42	.58
WB	.33					.35	.65
WE	.41					.27	.73
WBE	.46					.40	.60
NN						.11	.89
RS		.64				.45	.55
AS		.57				.46	.54
MS		.70				.49	.51
PT			.32			.22	.78
ART			.63			.45	.55
RQ			.59			.41	.59
CI			.77			.65	.35
SRT			.43			.29	.71
ERV			.80			.68	.32
AV			.72			.55	.45
NDV			.75			.64	.36
RPM				.44		.35	.65
AAT				.86		.75	.25
MAT				.82		.72	.28
NAO				.58		.56	.44
LC					.55	.36	.64
FA					.57	.36	.64
NC					.43	.32	.68
NDC			.39		.31	.43	.57
IGC			.49			.46	.54
Prop. Var.	.06	.07	.17	.10	.05	Total Var.	.44

Note. Model fit: $\chi^2(205) = 376.31$, $p < .001$; $\chi^2/df = 1.84$; TLI = .82; RMSEA = .07; RMSEA 90%CI [.06, .08]; BIC = -171.30.

PROCESSES UNDERLYING READING COMPREHENSION

Table 5

Model 2 (EFA) Principal Axis Factor Correlations

	1	2	3	4	5
1. Decoding & Fluency	1.00				
2. WMC	.12	1.00			
3. Language Experience	.15	.33	1.00		
4. Reasoning	.18	.41	.34	1.00	
5. Perceptual Speed	.16	.13	.13	.20	1.00

PROCESSES UNDERLYING READING COMPREHENSION

Figure 1

Psychometric Network Model of the Original Raw Data Provided by Freed et al. (2017)

Note. Model fit: $\chi^2(346) = 743.37, p < .001$; $\chi^2/df = 2.15$; TLI = .86; RMSEA = .06; RMSEA 90%CI [.05, .06]; BIC = -197.44.

PROCESSES UNDERLYING READING COMPREHENSION

Figure 2

Psychometric Network Model of the Cleaned Data Provided by Freed et al. (2017)

Note. Model fit: $\chi^2(229) = 504.01$, $p < .001$; $\chi^2/df = 2.20$; TLI = .88; RMSEA = .06; RMSEA 90%CI [.05, .07]; BIC = -107.71.