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Structural and Convergent Validity of Intelligence Composites: Integrating Evidence From Three Analysis Levels

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ABSTRACT

Despite extensive evidence of the reliability and validity of general intelligence (*g*) composites, current theoretical intelligence models—and with them also recent intelligence tests—de-emphasize *g* and instead focus more on *broad abilities*, such as fluid reasoning and processing speed. This although broad ability composites have been shown to be much less reliable and valid compared to *g* composites. In practice, both *g* and broad ability composites are interpreted for individuals and used to inform high-stakes decisions. Therefore, it is important to further clarify the validity of their interpretation for current intelligence tests not only at the *group level*, but also at the *individual level*. This dissertation thus aims to determine to what extent *structural* and *convergent validity evidence* provided at different analysis levels (i.e., the *total sample*, *subgroup*, and *individual level*) supports the interpretation of (a) *g* composites and (b) broad ability composites.

Structural validity evidence provided by Studies 1, 2, and 3 supports a strong and predominant *g* factor and weak broad ability factors for two concurrent intelligence tests at the total sample level as well as—for one of these tests—at the level of subgroups differing in sex and age (Study 2). Most of the postulated broad abilities were confirmed for these tests, but Visual Processing and Fluid Reasoning collapsed to one factor in all three studies. Of the confirmed broad ability composites, however, only two were (sometimes) reliable enough to justify their interpretation. Convergent validity evidence provided by Studies 4 and 5 reveals high correlations and small mean differences in *g* composites of multiple tests at the total sample level, but the *g* and broad ability composites from different intelligence tests (Study 4) and different *g* composites from the same tests (Study 5) sometimes showed large score differences at the individual level. These were predicted by IQ level and age, suggesting systematic differences across subgroups that differ in these characteristics. Even after taking measurement error into account by investigating the overlap of confidence intervals (CIs), there was still considerable incomparability. In Study 5, we thus examined if using more accurate reliability coefficients for CIs could increase comparability. Indeed, comparability was substantially improved if test–retest reliabilities or age- and IQ-level-specific internal consistencies were used for 95% CIs instead of one overall internal consistency. Finally, results from Study 5 suggested that the number, *g* factor loadings, and content of subtests might also influence the comparability of *g* composites.

The studies of this dissertation provide further support for the validity of the interpretation of *g* composites—but only if 95% CIs based on accurate reliability estimates are used—and against the validity of the interpretation of most broad ability composites from concurrent intelligence tests. Consequently, score interpretation should focus primarily, if not exclusively, on the *g* composite, which should consist of a sufficient number of subtests of heterogeneous content and with high *g* factor loadings. Moreover, especially for high-stakes decisions, at least two tests should be administered that are selected and interpreted with respect to testee characteristics and test content. Explanations and further implications of the findings of this dissertation as well as future directions for intelligence assessment are discussed in light of the goals pursued with intelligence assessments.

1. Introduction

General intelligence (*g*)[1] is defined as a general mental ability to reason, plan, solve problems, comprehend complex ideas, and learn from experience (Gottfredson, 1997a). It is a universal phenomenon in humans (Warne & Burningham, 2019) and highly predictive for a wide range of important life outcomes, including academic achievement (e.g., Deary et al., 2007; Roth et al., 2015), occupational success (e.g., Hunter & Hunter, 1984; Schmidt & Hunter, 2004), socioeconomic status and income (e.g., Gottfredson, 2004; Murray, 1998), relationship success (e.g., Aspara et al., 2018), political attitudes and participation (e.g., Deary et al., 2008a, 2008b), and health and longevity (e.g., Calvin et al., 2010; Gottfredson & Deary, 2004).

Despite this extensive evidence for the importance of *g*, several influential intelligence models, starting with Thurstone's (1938b) theory of *primary mental abilities* and culminating in the *Cattell–Horn–Carroll (CHC) model* (McGrew, 1997, 2009; Schneider & McGrew, 2018), have de-emphasized *g* and instead focused more on *broad abilities*, such as visual processing or processing speed. One major reason for this is that the information on individual strengths and weaknesses provided by broad abilities was deemed more useful than one *g* estimate. Consequently, there has been an increase in the number of broad abilities purportedly measured by intelligence tests over the last 70 years (Beaujean & Benson, 2019; Frazier & Youngstrom, 2007), and many concurrent intelligence tests focus more on the assessment of broad abilities than on that of *g* (Canivez & Youngstrom, 2019).

This development is problematic for at least two reasons: (a) There are numerous studies supporting the validity and utility of *g* composites[2] (e.g., Canivez & Youngstrom, 2019; Deary, 2014; Gottfredson, 1997b; Roth et al., 2015; Schmidt & Hunter, 2004), and (b) evidence is accumulating that broad ability composites are less reliable, less valid, less useful for diagnostics and treatment planning, and often possess little incremental validity for important life outcomes compared to *g* composites (Brown et al., 2006; Canivez & Youngstrom, 2019; McGill et al., 2018; Schmidt & Hunter, 2004).

Despite this evidence, a focus on broad abilities is still common in practice (Kranzler et al., 2020), where intelligence tests are frequently used as a basis for high-stakes decisions (Goldstein et al., 2015), for example, in school psychology to identify students with special needs, or in personnel psychology to identify promising candidates. It is therefore important to further clarify the validity of both *g* and broad ability estimates from concurrent intelligence tests. As intelligence test scores are interpreted for individuals, evidence should be provided not only at the *group level* (i.e., total samples or subgroups), but also at the *individual level*.

The aim of this dissertation thus was to determine to what extent validity evidence provided at different analysis levels supports the interpretation of (a) *g* composites and (b) broad ability composites.

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[1] Throughout this dissertation, I use *g* merely as an abbreviation of the term "general intelligence" and not to refer to general intelligence as conceptualized by specific theories, such as the two-factor theory (Spearman, 1904).

[2] Throughout this dissertation, I use the term "composite" as shorthand for "composite score" to refer to a test score composed of (usually unitarily weighted) subtest scores.

To this end, I present evidence on two aspects of validity—the *structural* and *convergent* aspect (American Educational Research Association [AERA] et al., 2014)—at three different analysis levels—the *total sample*, *subgroup*, and *individual level*. Study 1 (Grieder & Grob, 2020), Study 2 (Grieder et al., 2021), and Study 3 (Canivez et al., 2021) investigate the structural validity aspect for two concurrent intelligence tests for children and adolescents, one of them in two language versions, at the total sample level with large representative samples. Study 2 additionally investigates the structural validity aspect for one of these tests at the level of relevant subgroups from the reference populations that differ in sex and age. Study 4 (Bünger et al., 2021) and Study 5 (Grieder et al., in press) investigate the convergent validity aspect at all three analysis levels for multiple tests for children, adolescents, and adults, with Study 5 additionally exploring ways to improve the validity of the interpretation of *g* composites.

In Section 2 of this dissertation, I present relevant intelligence models and the fundamentals of intelligence measurement, introduce the concept of validity, and review empirical validity evidence. In Section 3, I introduce the research questions. In Section 4, I outline the methods of the studies included in this dissertation, and in Section 5, I provide a synopsis of the study results. Finally, in Section 6, I discuss the results and their implications and suggest future directions for intelligence assessment.

2. Theoretical Background

2.1 Definition and Theoretical Models of Intelligence

Research on intelligence has a long and prolific history that dates back as far as Plato (Beaujean, 2019), but it was Charles Spearman (1904) who developed the first formal definition and theoretical model of intelligence. In an attempt to explain the *positive manifold* (i.e., all-positive correlations) of cognitive test outcomes, Spearman's (1904) *two-factor* or *g theory* postulates that each cognitive task measures *g* as well as something specific to the task that is independent of *g*, which he termed *S*. In addition to a mathematical definition, Spearman verbally defined *g* as "the one great common Intellective Function" (Spearman, 1904, p. 272). He refrained from providing a more detailed verbal definition until research would reveal more about the nature of *g* (Spearman, 1905).

In the years to follow, intelligence research has grown rapidly and intelligence has become one of the best-researched constructs in psychology to date (Rost, 2009). Although there is still no universally accepted definition of intelligence, there is a widely accepted verbal definition of *g* agreed upon by 52 leading intelligence scholars that was published by Gottfredson (1997a) and more recently reiterated by Nisbett et al. (2012):

> Intelligence is a very general mental capability that, among other things, involves the ability to reason, plan, solve problems, think abstractly, comprehend complex ideas, learn quickly and learn from experience. It is not merely book learning, a narrow academic skill, or test-taking smarts. Rather, it reflects a broader and deeper capability for comprehending our surroundings—"catching on", "making sense" of things, or "figuring out" what to do. (Gottfredson, 1997a, p. 13)

In contrast to *g*, the specific factors played a lesser role in Spearman's two-factor theory. Spearman defined these as everything that is measured with a specific cognitive task apart from *g*. If the tasks are sufficiently diverse, the specific factors should all be independent of each other (Spearman, 1904). If any tasks do share variance beyond *g*, then they may "be brought together as a more or less unitary power" (Krueger & Spearman, 1906, p. 103). Spearman was thus aware of other possible attributes in the intelligence domain that we now call *group factors* or *broad abilities* and also devoted some time to study them (e.g., Krueger & Spearman, 1906). However, he thought most of these broad abilities were too unstable to represent unitary attributes (Beaujean, 2019).

A major opponent of Spearman's theory was Louis Thurstone. In contrast to Spearman, Thurstone thought that *g* was too unstable across studies (Beaujean, 2019) and attempted to explain the intercorrelations of cognitive tasks with a set of seven independent factors he called *primary mental abilities* (Thurstone, 1938b). As it became clear that the independence of the factors in Thurstone's analysis was an artefact of his exclusive use of orthogonal factor rotation methods and a variance-restricted sample (Thurstone, 1938a), he later allowed correlations between the factors and found that a higher-order factor could be extracted to account for the factors' intercorrelations (Thurstone & Thurstone, 1941). This marked the birth of the era of hierarchical intelligence models, which are still the dominant conception of intelligence to date.

Thurstone's adoption of a *higher-order g* factor is often regarded as reconciliation of his theory with Spearman's (Thurstone, 1947). However, it is important to note that this higher-order *g* factor is not interpreted the same as Spearman's *g* factor. In the two-factor theory, specific factors (and their systematic clusters, the broad abilities) are thought of as independent of *g* and of each other (Krueger & Spearman, 1906; Spearman, 1904). In the presence of broad abilities, this conception is best represented by a *bifactor model* (Holzinger & Swineford, 1937; Reise, 2012), where each task is influenced by (a) a *g* factor common to all tasks, (b) a broad ability common to a subset of the tasks that is independent of the other broad abilities and of the *g* factor, and (c) a unique variance term. In higher-order models, on the other hand, the *g* factor does not represent an attribute that is independent of the broad abilities but instead represents whatever the broad abilities have in common. Thus, in higher-order models, *g*'s influence on the tasks is fully mediated by the broad abilities, while, in a bifactor model, it directly influences the tasks. Which type of model best represents intelligence structure is still controversial, and I revisit the current state of research on this below.

Since Thurstone's work, many intelligence theories and models have been developed. Two theories that are still highly influential to date are Cattell and Horn's extended *Gf-Gc theory* (Cattell, 1941; Horn, 1991; Horn & Cattell, 1966) and Carroll's *three-stratum theory* (Carroll, 1993). Both of these theories include a number of highly comparable broad abilities at Stratum II and narrow abilities at Stratum I, but they again differ with respect to the status of *g*; while Carroll's model includes a *g* factor at Stratum III, Cattell and Horn's model does not. Cattell and Horn argued that "a concept of *g* does not provide a sound basis for understanding human cognitive functioning because different

intellectual abilities have different patterns of change in adulthood" (Horn, 1991, p. 224). While some broad abilities (e.g., Fluid Reasoning [Gf]) decline with age in adulthood, others (e.g., Comprehension–Knowledge [Gc]) remain stable or increase (Horn, 1991; see also investment theory by Cattell, 1963).

Despite this major difference, *Gf-Gc* theory and the three-stratum theory have been integrated into a comprehensive framework—the *CHC model* (McGrew, 1997, 2009; Schneider & McGrew, 2018). The CHC model is a higher-order model[3] with over 80 narrow abilities on Stratum I and at least 14 broad abilities on Stratum II, including Gf, Gc, Visual Processing (Gv), Processing Speed (Gs), Working Memory Capacity (Gwm), Auditory Processing (Ga), Learning Efficiency (Gl), Retrieval Fluency (Gr), Quantitative Knowledge (Gq), and Reading and Writing (Grw; Schneider & McGrew, 2018). Reflecting the disagreement of Cattell–Horn and Carroll, a *g* factor is usually included at Stratum III, but *g* is de-emphasized and not regarded as a useful construct (Schneider & McGrew, 2018). This ambiguous status and de-emphasis of *g* constitute major criticisms of the CHC model, together with failures to replicate the CHC structure in independent studies, lack of parsimony and falsifiability, insufficient neurobiological underpinnings, and lack of evidence for the reliability and validity of CHC broad ability profiles (see Wasserman, 2019, for an overview).

The theories and models discussed so far are largely grounded in psychometric and factor-analytic evidence. However, there exist alternative theories that are more process based and deal with some of the criticisms introduced above. A popular example of such theories is the *planning, attention–arousal, simultaneous and successive (PASS) theory* (Das et al., 1994). PASS theory is grounded in evidence from neuroscience and cognitive psychology and holds that intelligence is best viewed as a set of independent but related systems and processes. It challenges the idea of *g* and, in this respect, agrees with alternative explanations of the positive manifold, as provided by the *mutualism model* (van der Maas et al., 2006) or by *process overlap theory* (Kovacs & Conway, 2016, 2019). These theories back up the CHC theorists' claim of *g* not being a real construct and seem to contradict the extensive evidence of the usefulness of *g* composites (e.g., Canivez & Youngstrom, 2019). I revisit and try to tackle this paradox in the Discussion, as it has important implications for how to proceed with intelligence assessment in the future.

Still, despite criticism and the presence of plausible alternative models, the CHC model is currently the one most widely referred to in intelligence research and test construction (Alfonso et al., 2005; McGill & Dombrowski, 2019; Schneider & McGrew, 2018) and was also the basis for most tests included in the studies of this dissertation. Having established the history and current theoretical status of intelligence, I now elaborate how intelligence measurement has evolved up to now.

2.2 Measurement of Intelligence

The first modern intelligence test was developed by Binet and Simon (1905) as a means to identify children with intellectual disabilities who needed special education, which is still a major

[3] This although Carroll's model is best represented through a bifactor model (Beaujean, 2015).

reason for the application of intelligence tests to date. They developed a set of cognitive tasks and soon realized that the probability of solving a task increased not only with lower levels of "abnormality" (as they assumed) but also with a child's age. As we now know, the latter reflects the development of abilities due to brain maturation and increasing knowledge during childhood and adolescence (Rost, 2009).[4] Considering this, Binet and Simon ordered the tasks by increasing difficulty, and a child's "score" consisted of the discrepancy between their chronological and mental age, the latter being estimated with the level of tasks usually completed by children at that age (Binet & Simon, 1907).

Since then, psychometrics has developed, but the core idea has remained unchanged. Contemporary intelligence tests use composites of age-standardized subtest scores that are determined with large representative samples and scaled onto an IQ metric. IQ scores are normally distributed in the population ($M = 100$, $SD = 15$) and are estimates of the relative intelligence level compared to other individuals of the same age (Rost, 2009; Wechsler, 1939). These scores are used for both *g* and broad abilities and are often classified into categories, with values between 85 and 115 ($M \pm 1$ *SD*) classified as average, values between 71 and 84, and 116 and 129 as below and above average, respectively, values 70 and below ($\leq M - 2$ *SD*s) as *intellectual disability* (World Health Organization, 2020), and values 130 and above ($\geq M + 2$ *SD*s) as *intellectual giftedness* (Carman, 2013).

While the first intelligence tests (e.g., Binet–Simon, Army Alpha, and Army Beta; Yoakum & Yerkes, 1920) were designed to measure some form of *g*, there has been a growing emphasis on broad abilities and profile analysis since Thurstone introduced his primary mental abilities (Thurstone, 1938b), and this is also where the major emphasis lies in many contemporary intelligence tests (Canivez & Youngstrom, 2019). The main idea behind this is that rather than the interindividual position in relation to individuals of the same age, an intraindividual perspective (e.g., using profile analyses and difference scores) would be more informative for diagnostics and treatment planning, as this enables the identification of an individual's relative strengths and weaknesses (McGill et al., 2018).

An example of this development is provided by the Wechsler scales, which are among the most-administered psychological tests in the world to date (Evers et al., 2012; Oakland et al., 2016; Rabin et al., 2016). David Wechsler's first intelligence test—the Wechsler–Bellevue Intelligence Scale (Wechsler, 1939)—was intended for administration to adults. It includes a full scale encompassing all 10 subtests, and two subscales—a Verbal and a Performance Scale—which consist of five subtests each and which Wechsler thought of as measuring different aspects of *g* (Goldstein et al., 2015). Since these beginnings, different age versions have been developed and adapted, and the latest version of the Wechsler Intelligence Scale for Children, the WISC-V (Wechsler, 2014), refers to the CHC model and includes 16 subtest scores on which are based a *g* composite, five broad ability composites, five ancillary composites, three complementary composites, 10 process scores, and 31 difference scores at

[4] Despite this lack in mean-level stability, however, *g* estimates exhibit high rank-order stability from childhood on (correlations between .50 and .80 from around 11 years to old age; Deary, 2014).

the subtest and composite score level. This plethora of scores most likely violates the rule that "there should never be more scores to interpret than there are attributes being measured" (Beaujean & Benson, 2019, p. 134) and raises the question of which of these scores can be interpreted validly.

In practice, the main emphasis typically lies on the interpretation of broad ability and *g* composites (Kranzler et al., 2020). Given that these scores are often used for diagnostic purposes and as a basis for high-stakes decisions, it is crucial to ensure they are reliable and that their interpretation—be it inter- or intraindividual—is valid for the intended purposes.

2.3 Validity of Intelligence Test Score Interpretations

Multiple guidelines exist for the use and evaluation of psychological tests (e.g., AERA et al., 2014; Diagnostik- und Testkuratorium, 2018; Geisinger et al., 2013; International Test Commission, 2001), and all of them include *validity* as a criterion that needs to be established for any test. It can be defined as "the degree to which evidence and theory support the interpretations of test scores for proposed uses of tests" (AERA et al., 2014, p. 11; see also Messick, 1989).[5] Thus, validity is not a property of the test itself, but an evaluative judgment of the meaning of test scores (Messick, 1989).

Although validity is a unitary concept, there are different types of validity evidence, namely, evidence based on *test content*, *response processes*, *internal structure*, and *relations to other variables*, the latter including *convergent* and *discriminant* evidence and evidence from *test–criterion relationships* (AERA et al., 2014). Which of those aspects are relevant depends on the claims regarding the interpretation of a test score. For example, imagine that one makes the claim that a *g* composite of a specific test can be used to determine the need for special education because it is a measure of *g*, which in turn is known to be predictive of their future learning success and academic achievement. In this case, evidence should be provided that (a) the composite is indeed a measure of *g* (test content evidence, but also convergent and discriminant evidence), (b) the subtests of which the score is composed are indeed good indicators of *g* (internal structure evidence, henceforth: *structural validity evidence*), and (c) the composite is predictive of future learning success and achievement. On the other hand, it is less important which cognitive processes underlie test performance, and thus evidence based on response processes is not necessary in this case. Often, test scores are used for multiple different purposes, requiring a variety of validity evidence.

The present dissertation focuses on two types of validity evidence—namely, structural and convergent—for the most commonly interpreted intelligence test scores—namely, *g* and broad ability composites. Providing these types of evidence is necessary (although not sufficient) to justify the interpretation of *g* and broad ability composites for most applied purposes.

Because intelligence test scores are interpreted for individuals and results from the group level are not necessarily transferable to the individual level (e.g., Molenaar & Campbell, 2009), validity

[5] There exist other definitions of validity, for example, by Borsboom et al. (2004), which I come back to in the Discussion. However, the definition mentioned here represents a broader consensus in the measurement literature and is therefore adopted in the present dissertation.

evidence should whenever possible be provided not only at the group but also at the individual level. Establishing structural validity evidence at the individual level is difficult, however, as it requires many administrations of the test in question to the same individuals (see Borkenau & Ostendorf, 1998, for an example). To still be able to judge generalizability across individuals of the reference population, structural validity evidence should also be established at the level of *relevant subgroups of the reference population* differing in characteristics that might influence the validity of test score interpretations, such as age, sex, or language skills (AERA et al., 2014). Convergent validity evidence, however, can be established at both the group and individual level. Establishing convergent validity evidence at the individual level is especially important because intelligence test scores are typically used interchangeably. That is, a practitioner selects one intelligence test and interprets its scores as if they had been the same (or at least very similar, considering measurement error) on any other test purporting to measure the same construct. The present dissertation thus considers validity evidence at three different analysis levels, namely, two group levels (i.e., the total sample and relevant subgroups) and the individual level.

Of course, test score interpretations cannot be valid if the scores are not reliable. Therefore, *reliability* (i.e., the proportion of true score variance in the observed score variance) and its different estimations (Cronbach, 1947; McDonald, 1999; Schmidt et al., 2003) are also considered in the present dissertation as a prerequisite for valid test score interpretations. In particular, it includes examinations of *model-based reliability* estimates (e.g., Gignac, 2014; Reise, 2012; see below for details), which are related to structural validity evidence, and *internal consistency* and *test–retest reliability* estimates (Schmidt et al., 2003) as influencers of convergent validity evidence. Just like structural validity evidence, reliability estimates can and should also be provided at the level of relevant subgroups of the reference population (AERA et al., 2014), which is what some studies of this dissertation did.

Structural Validity Evidence. This type of evidence refers to "the degree to which the relationships among test items and test components conform to the construct on which the proposed test score interpretations are based" (AERA et al., 2014, p. 16). Structural validity evidence for intelligence tests is usually provided using exploratory and/or confirmatory factor analyses (EFA and CFA, respectively) of age-standardized subtest scores. Previous studies on major intelligence tests consistently confirmed a hierarchical structure that sometimes (e.g., Canivez et al., 2020; Canivez & Watkins, 2010) but not always (e.g., Canivez et al., 2017; Dombrowski, McGill, & Canivez, 2018) conformed with the theoretically proposed structure. Often, not all proposed broad abilities are confirmed (e.g., Canivez, 2008; Canivez et al., 2016, 2017; Keith & Reynolds, 2010), and/or the allocation of subtests to broad ability factors is not as proposed by the theoretical model (e.g., Dombrowski, 2014; Dombrowski, Beaujean, et al., 2018). In these cases, the interpretation of at least some broad ability composites is not supported on the basis of structural validity evidence. In contrast, the interpretation of a *g* composite is virtually always supported.

Since the revival of the bifactor model (Reise, 2012), bifactor structures have been increasingly tested and found to fit best for many intelligence tests (Cucina & Byle, 2017). Some have also argued that the bifactor model was superior because it was theoretically more parsimonious than the higher-order model (e.g., Gignac, 2008). Thus, it is likely that *g* and broad abilities are best conceptualized as independent of each other. I revisit this controversy in the discussion of directions for future intelligence assessment. However, as the conceptualization of intelligence as higher-order or bifactor is not important for answering the research questions of this dissertation, I do not go into further detail here.

Given that both *g* and broad abilities seem to influence subtest scores, it is important to clarify the unique contributions of the two. For this purpose, model-based reliability estimates, such as McDonald's omegas (Gignac, 2014; McDonald, 1985, 1999; Reise, 2012; Zinbarg et al., 2006), are well suited. They make it possible to partition a composite's true score variance (estimated with ω_t) into variance explained by the *g* factor (estimated with ω_h) and variance explained by its respective broad ability factor (for broad ability composites) or by all broad ability factors (for the *g* composite; estimated with ω_s). Many authors caution against universal thresholds for ω, but a preliminary suggestion of .50—with .75 being preferred—has been made for ω_h for the whole scale (here: the *g* composite) and for ω_s for the subscales (here: the broad ability composites; Reise et al., 2013). For most major intelligence tests, model-based reliability analyses provided evidence of a strong, dominant *g* factor ($\omega_h > .75$) and weak broad ability factors ($\omega_s < .50$ for most of them; e.g., Canivez et al., 2017; Cucina & Howardson, 2017; Dombrowski, McGill, & Canivez, 2018; Watkins, 2017). Thus, model-based reliability evidence virtually always supports the interpretation of the *g* composite as the primary estimate of *g*, but not the interpretation of most broad ability composites as primary estimates of the respective broad abilities.

Although structural validity evidence—and model-based reliability estimates that draw on it—converges for many different intelligence tests, validity evidence needs to be established for the scores of any new test, which was one aim in Studies 1, 2, and 3. All three studies examined this validity aspect at the total sample level, and Study 2 also at the subgroup level. Finally, model-based reliabilities have typically been investigated for whole (standardization) samples but, to my knowledge, not for relevant subgroups of the reference population, which was another aim in Study 2.

Convergent Validity Evidence. This type of evidence refers to "relationships between test scores and other measures intended to assess the same or similar constructs" (AERA et al., 2014, pp. 16–17). Convergent validity evidence for intelligence tests typically stems from examining the correlations of scores from different intelligence tests (i.e., from group-level analyses). Correlations are usually high (around .50–.80) and highest for the *g* composites (e.g., Floyd et al., 2008; Grob et al., 2019b; Grob & Hagmann-von Arx, 2018b). However, as argued above, convergent validity evidence should also be provided at the individual level.

The few studies that did this provided evidence for limited convergent validity at the individual level (i.e., limited score comparability) for both *g* (Floyd et al., 2008; Hagmann-von Arx et al., 2018) and broad ability (Floyd et al., 2005) composites across different intelligence tests, even after

controlling for measurement error (e.g., by comparing confidence intervals [CIs]). These results suggest that either the reliability estimates used did not sufficiently capture measurement error and/or at least some of the test scores did not provide valid estimates of the construct they purportedly measured. All three studies concluded that any two intelligence tests do not render comparable *g* or broad ability composites at the individual level, even if they are highly correlated at the group level.

In sum, convergent validity has almost exclusively been established at the group level and rarely at the individual level. We thus further investigated the individual-level comparability, with a larger focus on predictors of incomparability, for *g* composites (Studies 4 and 5), and two types of broad ability composites (Study 4). Analogous to previous studies, Study 4 compared composites from different tests, while Study 5 used a new approach and compared composites within the same tests to rule out between-test variability. Additionally, Study 5 examined ways to improve the interpretation of *g* composites for individuals by using more accurate reliability estimates.

3. Research Questions

The aim of this dissertation is to integrate evidence from three analysis levels (i.e., the total sample, subgroup, and individual level) to evaluate the validity of the interpretation of *g* and broad ability composites and seek ways to improve validity. Table 1 illustrates the dissertation concept and how the studies relate to it. The research questions (RQs) are listed below.

Table 1. *Dissertation Concept: Structural and Convergent Validity Evidence at Three Analysis Levels*

	Total Sample		**Subgroup**		**Individual**	
	Structural	Convergent	Structural	Convergent	Structural	Convergent
General Intelligence	RQ1a: (1) (2) (3)	RQ3: (4) (5)	RQ2a: (2)	RQ4: (4) (5)		RQ5a: (4) (5) RQ6: (5)
Broad Abilities	RQ1b: (1) (2) (3)		RQ2b: (2)			RQ5b: (4)

Note. The numbers refer to the studies included in the present dissertation: (1): Grieder & Grob, 2020; (2): Grieder et al., 2021; (3): Canivez et al., 2021; (4): Bünger et al., 2021; (5): Grieder et al., in press. RQ = research question.

3.1 Structural Validity Evidence

RQ1. Does structural validity evidence at the total sample level, based on the standardization samples from the German and Dutch versions of the Intelligence and Development Scales–2 (IDS-2; Studies 1 and 2) and from the German WISC-V (Study 3), support the interpretation of (a) *g* composites and (b) broad ability composites?

RQ2. Does structural validity evidence at the level of subgroups that differ in age and sex support the interpretation of (a) *g* composites and (b) broad ability composites from the German and Dutch IDS-2 (Study 2)?

3.2 Convergent Validity Evidence

RQ3. Does convergent validity evidence at the total sample level support the interpretation of *g* composites from multiple tests (Studies 4 and 5)?

RQ4. Does convergent validity evidence at the level of subgroups differing in age, bilingualism, IQ level (Studies 4 and 5), sex, and attention-deficit(/hyperactivity) disorder (AD[H]D) diagnostic status (Study 5) support the interpretation of *g* composites from multiple tests?

RQ5. Does convergent validity evidence at the individual level support the interpretation of (a) *g* composites and (b) broad ability composites from multiple tests (Studies 4 and 5)?

RQ6. How could the validity of the interpretation of *g* composites be improved (Study 5)?

4. Method

4.1 Studies and Samples

Study 1. This study included the standardization and validation sample of the German IDS-2 (Grob & Hagmann-von Arx, 2018a). Data on the intelligence domain (*g* and seven broad abilities) were available for 1,991 participants aged between 5 and 20 years, and data on the intelligence and basic skills domains (+ two broad abilities) were available for 1,741 participants aged between 7 and 20 years.

Study 2. This study included the standardization samples of the German and Dutch IDS-2 (Grob et al., 2018; Grob & Hagmann-von Arx, 2018a; final N = 1,405 and 1,423, respectively), with participants aged between 7 and 20 years and 7 and 21 years, respectively.

Study 3. This study included the standardization sample of the German WISC-V (Wechsler, 2017; N = 1,087), with participants aged between 6 and 16 years.

Study 4. This study included the validation samples of the German IDS-2 and the German version of the Stanford–Binet Intelligence Scales–Fifth Edition (SB5; Grob et al., 2019a; N = 383), with participants aged between 4 and 20 years. Besides the IDS-2 and/or the SB5, participants were also administered a subset of the German versions of other intelligence tests (see below).

Study 5. This study included the standardization samples of the German IDS-2, SB5, and Reynolds Intellectual Assessment Scales (RIAS; Hagmann-von Arx & Grob, 2014; final N = 1,622, 1,829, and 2,145, respectively), with participants aged between 5 and 20 years, 4 and 83 years, and 3 and 99 years, respectively.

4.2 Measures

In the following, the relevant measures used in the five studies are briefly introduced. All these measures are individually administered tests rendering multiple age-standardized subtest scores that are integrated in multiple unit-weighted composites.

IDS-2. The German and Dutch IDS-2 assess cognitive (intelligence and executive functions) and developmental (psychomotor skills, social-emotional skills, basic skills, and motivation and attitude) functions with 30 subtests. The intelligence domain includes 14 subtests used to create three different *g* composites—an Extended Battery IQ (EBIQ), a Full-Scale IQ (FSIQ), and an Abbreviated Battery IQ (ABIQ)—as well as seven broad ability composites. The latter correspond to Gv, Gs, auditory and visual-spatial Gwm (in the CHC model, these are differentiated at the level of narrow abilities), Gf, Gc, and Glr (a combination of Gl and Gr, as in prior versions of the CHC model; Schneider & McGrew, 2012). The basic skills domain includes four subtests, of which one corresponds to Gq, two

correspond to Grw, and one contains aspects of Ga. Studies 1 and 2 included the 14 intelligence subtests and the three basic skills subtests tapping Gq and Grw, Study 4 included the FSIQ and the Gf and Gc composites, and Study 5 included the EBIQ, FSIQ, and ABIQ.

WISC-V. The German WISC-V assesses intelligence with 15 primary and secondary subtests and postulates a *g* composite and five broad ability composites corresponding to Gc, Gv, Gf, Gwm, and Gs.[6] Study 3 included all 15 primary and secondary subtests.

SB5. The German SB5 assesses intelligence with 10 subtests used to create two *g* composites—an FSIQ and an ABIQ—a verbal and a nonverbal intelligence index (VI and NVI, respectively), and five broad ability composites corresponding to Gf, Gc, Gq, Gv, and Gwm. Study 4 included the FSIQ, VI, and NVI, and Study 5 included the FSIQ and ABIQ.

RIAS. The German RIAS assesses intelligence with four subtests and memory with two subtests. The four intelligence subtests are used to create a VI and an NVI (corresponding to Gc and Gf, respectively), and two *g* composites—an FSIQ and an ABIQ. Study 4 included the FSIQ, VI, and NVI, and Study 5 included the FSIQ and ABIQ.

Other Intelligence Tests. The following other tests are relevant for the present dissertation: the German versions of the Snijders Oomen Nonverbal Intelligence Test 6–40 (SON-R 6-40; Tellegen et al., 2012), the Wechsler Adult Intelligence Scale–Third Edition (WAIS-III; von Aster et al., 2006), the Wechsler Intelligence Scale for Children–Fourth Edition (WISC-IV; Petermann & Petermann, 2011), and the Wechsler Preschool and Primary Scale of Intelligence–Third Edition (WPPSI-III; Petermann, 2009). Of all these tests, Study 4 used the FSIQ and (if available) a VI and/or an NVI.

4.3 Statistical Analyses

Studies 1, 2, and 3. In these studies, we examined structural validity with factor-analytical methods. We used EFA in Studies 1 and 3 and CFA in Studies 2 and 3. In all three studies, analyses were performed at the total sample level, and in Study 2, additional measurement invariance analyses were performed across language versions and sex with multiple group CFAs (e.g., Steenkamp & Baumgartner, 1998), and across age with local structural equation modeling (Hildebrandt et al., 2009, 2016). Moreover, McDonald's omegas (McDonald, 1985, 1999) were calculated for the total sample in all three studies, and across language versions, sex, and age in Study 2.

Studies 4 and 5. In these studies, we examined convergent validity at the total sample, subgroup, and individual level. While *g* composites (i.e., FSIQs), VIs, and NVIs from different tests were compared in Study 4, different *g* composites of the same tests (i.e., EBIQ, FSIQ, and ABIQ) were compared in Study 5. For the total sample-level analyses, we calculated mean differences and correlations. For the individual-level analyses, we calculated intraindividual absolute differences in IQ points and overlaps of CIs and nominal IQ categories. For the subgroup-level analyses, we performed

[6] Note that the composites used in practice are created from subsets of these 15 subtests, namely, of the 10 primary subtests for the broad ability composites and of seven primary subtests for the *g* composite (the FSIQ).

regression analyses to explore possible predictors of IQ differences, including age, bilingualism, IQ level (i.e., below average, average, above average), sex, and AD(H)D diagnostic status.

5. Synopsis of Results

5.1 Structural Validity Evidence

The postulated factor structures for the German and Dutch IDS-2 (Studies 1 and 2) and for the German WISC-V (Study 3) were partly supported. All studies supported a *g* factor. For the IDS-2, the Gs and auditory and visual-spatial Gwm factors were confirmed in Studies 1 and 2, and the Gc and Glr factors in Study 2. For the WISC-V, the Gc, Gwm, and Gs factors were confirmed. In all three studies, however, the Gf and Gv factors were not separable. The intelligence and basic skills domains were also not separable for the IDS-2 in Studies 1 and 2, resulting in an additional Grw factor and the subtest Logical-Mathematical Reasoning loading on the collapsed Gf/Gv factor. Notably, the factor structure identified in Study 2 was shown to be invariant across the German and Dutch IDS-2 versions.

Model-based reliability analyses revealed a strong *g* factor for all three tests ($\omega_h > .75$ for the *g* composites) and weak broad ability factors ($\omega_s < .50$ for all broad ability composites, except the Gs composite for the WISC-V). For some broad ability composites (especially Gf/Gv and Glr), the error variance was even larger than the true score variance explained by the broad ability factor ($1 - \omega_t > \omega_s$).

Regarding subgroup-level analyses (Study 2), the factor structure identified for the German and Dutch IDS-2 was also largely supported across sex and an age span of 7 to 20 years. McDonald's omegas varied somewhat across sex and age, but ω_h was consistently above .75 for the *g* composites, and ω_s was below .50 for all broad ability composites, with the exception of auditory Gwm exceeding .50 for females for the German IDS-2.

5.2 Convergent Validity Evidence

Despite high correlations and small mean differences in test scores at the total sample level, the *g* composites, VIs, and NVIs from different intelligence tests (Study 4) and different *g* composites from the same tests (Study 5) sometimes showed large score differences at the individual level. These differences were predicted by IQ level (i.e., larger at the tails of the IQ distribution; Studies 4 and 5) and age (i.e., larger for younger individuals; Study 5), or their interaction (Study 5), suggesting some systematic differences in reliability and/or validity across subgroups that differ in these characteristics. Even after taking measurement error into account by investigating the overlap of CIs, there was still considerable incomparability. In Study 5, we therefore investigated if the use of more accurate reliability coefficients for CIs could increase comparability. We found that this was indeed the case; Comparability was substantially improved if 95% CIs based on test–retest reliabilities or age- and IQ-level-specific internal consistencies instead of 95% CIs based on one overall internal consistency or nominal IQs were used. However, these improvements came at the cost of precision, as the CIs were often much larger because of lower reliabilities. Finally, results from Study 5 suggested that higher numbers of highly *g*-loaded subtests and a greater overlap in subtest content might also increase comparability of *g* composites.

Table 2. *Support (Yes) or Lack of Support (No) for the Interpretation of General Intelligence and Broad Ability Composites From Structural and Convergent Validity Evidence at Three Analysis Levels*

	Total Sample		Subgroup		Individual	
	Structural	Convergent	Structural	Convergent	Structural	Convergent
General Intelligence	RQ1a: Yes	RQ3: Yes	RQ2a: Yes	RQ4: No		RQ5a: No RQ6: Yes
Broad Abilities	RQ1b: No		RQ2b: No			RQ5b: No

Note. RQ = research question.

6. General Discussion

6.1 Structural Validity Evidence

Structural validity evidence at the level of large, representative samples (Studies 1, 2, and 3) as well as of relevant subgroups of the reference populations (Study 2) supports the interpretation of *g* composites (see Table 2 for simplified answers to the RQs). In contrast, the little true score variance due to the broad ability factors calls into question the utility of most broad ability composites, with the possible exceptions of Gs and auditory Gwm. These findings are in line with previous studies on other major intelligence tests that have also found evidence for a valid interpretation of *g* composites and against one for most broad ability composites from a structural validity perspective (Canivez et al., 2016, 2017, 2019; Canivez & Watkins, 2010; Canivez & Youngstrom, 2019; Cucina & Howardson, 2017; Dombrowski, 2014; Dombrowski, McGill, & Canivez, 2018; Fenollar-Cortés & Watkins, 2019; Lecerf & Canivez, 2018; Nelson et al., 2013; Watkins, 2017; Watkins et al., 2018). These results speak against the focus on broad ability composites and the de-emphasis of *g* composites proposed by CHC theorists (e.g., Schneider & McGrew, 2018) and suggest a risk of misinterpretation of factor profiles. Consequently, score interpretation for most contemporary intelligence tests—including those examined in this dissertation—should focus mainly, if not exclusively, on the *g* composite.

The finding of collapsed Gv and Gf factors for the German and Dutch IDS-2 and the German WISC-V is also in line with previous research on other major intelligence tests, including the U.S., U.K., Canadian, French, and Spanish WISC-V (Canivez et al., 2016, 2017, 2019; Fenollar-Cortés & Watkins, 2019; Lecerf & Canivez, 2018; Watkins et al., 2018), the SB5 (Canivez, 2008; DiStefano & Dombrowski, 2006), the Woodcock–Johnson III (WJ-III; Dombrowski, 2013), the Kaufman Assessment Battery for Children–Second Edition (KABC-II; Keith & Reynolds, 2010; McGill, 2020), and cross-battery assessments of the WJ-III with the Differential Ability Scales and with the KABC-II (Keith & Reynolds, 2010). This collapse of Gv and Gf contradicts the CHC model, where these two are defined as separate constructs. I discuss possible explanations for this finding and the usefulness of the CHC model for further research and test development below.

6.2 Convergent Validity Evidence

Convergent validity evidence (Studies 4 and 5) at the total sample level supports the interpretation of *g* composites, but subgroup- and individual-level evidence casts doubt on it. Results on VIs and NVIs (corresponding to Gc and Gf for most, but not all tests; Study 4) suggest that it is

similar for broad ability composites. This is in line with results from previous studies investigating individual-level convergent validity that also speak against the interpretation of exact scores, and even traditional 90% and 95% CIs, for *g* and broad ability composites (Floyd et al., 2005, 2008; Hagmann-von Arx et al., 2018). Broad ability and *g* composites from different tests, and *g* composites from the same test, are thus not necessarily exchangeable for individuals, even if they are highly correlated at the group level. Consequently, individual interpretation of exact scores should be avoided, and CIs should be interpreted instead. Moreover, for high-stakes decisions, at least two tests should be used that are selected and interpreted in light of individual testee characteristics and test content (see below).

The fact that satisfactory score comparability at the individual level was not achieved even after controlling for measurement error by comparing CIs suggests that either the reliability estimates used did not sufficiently capture measurement error and/or at least some of the test scores did not provide valid estimates of the construct they purportedly measured. Our results suggest that both may be the case. The use of one overall internal consistency coefficient for CIs misses certain kinds of measurement error (Schmidt et al., 2003) and does not consider that measurement error varies with certain individual characteristics, such as age and IQ level. Therefore, CIs based on this overall reliability estimate are too small for many individuals, and they get wider and comparability thus increases if more accurate reliability coefficients (e.g., test–retest reliability or the coefficient of equivalence and stability; Cronbach, 1947; Schmidt et al., 2003; or at least age- and IQ-level-specific internal consistencies) are used (Study 5). Consequently, these more accurate CIs should be interpreted in practice. Test developers should provide and promote such CIs for interpretation in future tests and update CIs for existing tests accordingly. Especially for tests using a digital scoring program, more appropriate CIs that are conditional on individual characteristics of the testee could easily be implemented.

Besides inappropriate reliability estimates, overlap in subtest content, *g* loadings, and the number of subtests likely also influenced individual-level score comparability. These aspects have also been shown to affect the accuracy of *g factors* (Farmer et al., 2020; Floyd et al., 2009; Major et al., 2011). Consequently, *g* composites should consist of a sufficient number of subtests of heterogeneous content and with high *g* factor loadings to achieve psychometrically sound estimates of *g* (Farmer et al., 2020; Jensen & Weng, 1994; Major et al., 2011). Composites of two or three subtests are likely not accurate enough and should not even be used for screening purposes (Study 5, Farmer et al., 2020). Four subtests might be enough, but accuracy seems to increase up to 12 to 13 subtests (Farmer et al., 2020). More important than the sheer number of subtests, however, is adequate and diverse content sampling (Farmer et al., 2020; Floyd et al., 2009; Major et al., 2011). I further discuss possible reasons for the variability and limited comparability of *g* composites and *g* factors below.

6.3 Future Directions for Intelligence Assessment

Although a focus on the *g* composite and on more accurate CIs is recommended as an immediate practical remedy for the highlighted problems in existing intelligence tests, the long-term goal must be to tackle the validity issues revealed in the present dissertation and related research by creating more

reliable and valid intelligence measures. To discuss how this could be achieved, I would like to fall back on two stances in philosophy of science that differ in how they define the ultimate goals of science and in the implications for how measures should be designed so that they are valid for these goals, namely, *realism* and *instrumentalism*.

Scientific realism refers to the view that there are entities in the world that exist independent of one's thoughts, language, or point of view and that it is a goal of science to understand and describe these real entities (Godfrey-Smith, 2003). In contrast, instrumentalism holds that the purpose of science is not to describe the hidden structures responsible for patterns of observations, as one can never be sure to have accurately described the actual world and causal structures, but to predict observations. In this view, it is not important whether an entity really exists in the real world, as long as it is useful for describing observations (Godfrey-Smith, 2003). Historically, these opposing views have influenced scientific inquiries in different fields within and outside psychology, including intelligence research. For example, Spearman held a realist view of intelligence and devoted much of his research to the nature of intelligence, while Thurstone held an instrumentalist view and thought that a construct (such as his primary mental abilities) should be useful to summarize and describe observations, and not necessarily be part of the real world (Beaujean, 2019).

Yarkoni and Westfall (2017) resume this distinction in the form of *explanation* (compatible with a realist stance) versus *prediction* (compatible with an instrumentalist stance). The common view is that theories and models that help explain the processes underlying a behavior will also lead to better predictions of future behavior. However, this is not necessarily the case (Yarkoni & Westfall, 2017). Especially in psychology, where we study complex cognitive, emotional, and behavioral phenomena, the causal processes underlying these phenomena might never be fully understood using models that are comprehensible to humans. Scientists thus have to choose between explaining some processes underlying the outcome—at the cost of limited predictive value—and accurately predicting outcomes of interest—at the cost of limited explanatory value. In the following, I discuss the relevance of these two stances or goals for the future of intelligence assessment.

Realist View. First, I focus on the realist stance and on the goal of explaining behavior. This is the major goal that most research in psychology, including intelligence research, has been and still is pursuing. We pursue this goal, for example, when we look for neurological or cognitive processes underlying intelligence, or when we test which structural model best fits intelligence test data. Hence, the construct of intelligence is typically interpreted in a realist sense. In this case, however, we have to rethink our definition of validity. Borsboom et al. (2004) argue that, if we accept a realist stance, a measure is valid if and only if (a) the construct it intends to measure exists in the real world and (b) variations in this construct causally produce variations in the measurement outcome. They explicitly reject validity definitions such as the one adopted in this dissertation (i.e., an evaluative judgment regarding score interpretation; AERA et al., 2014; Messick, 1989) and instead view validity as a property of the test. Validation should thus be concerned with providing evidence for points (a) and (b)

introduced above, which is mainly achieved by substantive theory. If we adopt a strict realist view, empirical evidence such as that presented in this dissertation, and in most of intelligence research so far, is not useful to establish validity (Borsboom et al., 2004).

Instead, validation should start from the question: Are *g* and the broad abilities reflective constructs (i.e., entities that exist in the real world)? As Borsboom et al. (2004) argue, the answer to this question requires substantive theories with narrow, process-based definitions of the constructs to measure that can guide the development of tasks to measure them (cf. Beaujean & Benson, 2019). The CHC model that is widely referred to for test construction (Schneider & McGrew, 2018) likely cannot live up to this, as it is a framework largely developed with factor analysis (which is inappropriate validity evidence according to Borsboom et al., 2004) and because it provides only verbal (as opposed to technical/mathematical) definitions of the constructs that are not narrow enough to guide test construction and that are not sufficiently linked to (neurocognitive) processes (Beaujean & Benson, 2019; Wasserman, 2019). Instead, research on neural correlates and cognitive processes underlying *g* and the broad abilities could help clarify their status as real constructs.

Neural correlates identified for *g* include, for example, brain volume, cortical thickness, and white matter tract integrity (Colom et al., 2006, 2010; Gignac et al., 2003; Haier et al., 2004; McDaniel, 2005; Schubert & Frischkorn, 2020). Jung and Haier (2007) integrated such evidence and developed the parieto-frontal integration theory (P-FIT) that states that both structural components of a network of specific frontal and parietal brain regions and more efficient communication between these regions provide a neurobiological foundation for *g* (but also for Gf and Gwm). Cognitive processes underlying *g* have also been studied, with attention or executive control processes (Burgoyne & Engle, 2020; Kovacs & Conway, 2016) and higher-order information processing speed (Schubert et al., 2017) as promising candidates for explaining interindividual differences in *g*. Linking evidence on cognitive processes and neural correlates, Schubert and Frischkorn (2020) proposed a model where (in line with the P-FIT) differences in brain structure give rise to differences in network structures, which in turn give rise to differences in the speed of higher-order information processing and evidence accumulation as a basis for *g* differences. These are all promising avenues for a deeper understanding of the neurocognitive processes underlying *g*.

Evidence for the processes underlying broad abilities is less extensive. A few studies suggest that there might be some brain correlates that are unique to certain broad abilities (Colom et al., 2013; Johnson et al., 2008; Tang et al., 2010), but part of the evidence is weak (Tang et al., 2010) and others found no correlates independent of *g* (Karama et al., 2011). Hence, the neurocognitive roots of most broad abilities are weak (Wasserman, 2019) and clearly more research is needed in this area.

This kind of research brings us closer to the process-based understanding of *g* and the broad abilities needed to establish them as reflective constructs, but there are still some issues that need to be addressed. First, most of the evidence reviewed above is correlational and thus cannot reveal the causal processes involved in task performance (as Borsboom et al.'s, 2004, definition of validity requires). To

this end, experimental research is needed. Second (and relatedly), there seems to be a substantial overlap in the processes involved in *g*, Gf, Gwm, and executive function tasks (e.g., Colom et al., 2010; Kovacs & Conway, 2016; Schubert & Frischkorn, 2020), and more research is needed on the separability of these constructs. And third, although all these findings are consistent with an interpretation of *g* as a reflective construct in a realist sense (i.e., a unitary process or set of processes that causally influence performance in all cognitive tasks and lead to the positive manifold), they are also consistent with an interpretation of *g* as a formative construct (i.e., a consequence rather than the cause of the positive manifold). If the latter were true, however, what would then cause the positive manifold?

Two theories that provide possible answers to this question are the mutualism model by van der Maas et al. (2006) and process overlap theory by Kovacs and Conway (2016, 2019). The mutualism model explains the positive manifold with positive reciprocal interactions between initially independent cognitive processes during development. Process overlap theory explains it with overlapping subsets of a small set of domain-general executive processes that are tapped by cognitive tasks (but not necessarily the same subset is tapped by every task). Both theories are mathematically formalized and can explain many findings in intelligence research, such as *age* and *ability differentiation*, the *Flynn effect*, and the *worst performance rule* (Kovacs & Conway, 2016; van der Maas et al., 2006). Conceptualizing *g* as a formative variable also explains why *g* composites (Studies 4 and 5, Floyd et al., 2008; Hagmann-von Arx et al., 2018), *g* factors (Farmer et al., 2020; Floyd et al., 2009; Major et al., 2011), and even neural correlates (Haier et al., 2009) sometimes vary considerably between different tests. If *g* were reflective, subtests that tap *g* should be interchangeable to a large degree and still result in comparable *g* factors. If it were formative, however, subtest content would have a greater influence on the resulting *g* factors in that it determines which domain-general executive processes are tapped. Consequently, the more diverse the subtests included in a *g* composite, the more likely it is that many of these domain-general executive processes are tapped and thus the more comparable the *g* composites are.

The two theories are not necessarily mutually exclusive, and neither has been falsified yet, but process overlap theory has more support from neurobiology and cognitive psychology, and it can also explain the high interrelations between *g*, Gf, Gwm, and executive functions (Kovacs & Conway, 2016). It might also explain the common finding of Gf and Gv not being separable in hierarchical intelligence models where Gf is measured exclusively with visual-spatial tasks (e.g., Studies 1, 2, and 3, Dombrowski, 2013; Keith & Reynolds, 2010; McGill, 2020). Typical Gf tasks, such as matrices, largely tap domain-general executive processes (Kovacs & Conway, 2016) and additionally some visual processes, while Gv tasks tap mainly visual processes but also some domain-general executive processes.

Hence, both theories support the criticism put forth by Cattell–Horn and CHC theorists that *g* is not a construct in a realist sense. However, some broad abilities could be, and Kovacs and Conway (2019) argue that interpretation and research should focus on these. Although little is known about the neurocognitive processes underlying most broad abilities, preliminary evidence suggests that the neural

correlates of some broad abilities overlap more strongly across tests than those for *g* (Haier et al., 2009). Thus, a realist interpretation might more likely be justified for broad abilities. Before their potential can be further explored, however, they need to be measured more accurately.

To achieve this, we have to challenge the notion of intelligence as hierarchically structured, as this precludes a satisfactory measure of both *g* and broad abilities. Because subtests need to be indicators of both, the minimal threshold of 50% (preferably 75%) true score variance (Reise et al., 2013) cannot be achieved for both *g and* broad ability composites in any test, even if measurement error is minimal. Consequently, we should try to measure *g* and the broad abilities as independently of each other as possible to maximize the true score variance for all of them (Beaujean & Benson, 2019; Luecht et al., 2006). This only makes sense, of course, if we understand *g* and the broad abilities as independent components of intelligence (i.e., a bifactor conception of intelligence, as adopted by Spearman or Carroll: see Beaujean, 2015, 2019). This conception has also been suggested as a more reasonable alternative to a higher-order conception (e.g., Gignac, 2008) because a realist interpretation of higher-order factors is problematic and because there is no evidence or reasonable argument for why the influence of *g* on task performance should be fully mediated by the broad abilities (Gignac, 2008).

Thus, we should try to come up with more distinct definitions and purer measures of *g* and broad abilities. Given previous findings, I would predict that, in doing so, a separation of what we now call *g* and Gf would become less and less evident. The two are usually highly, and often even perfectly, correlated (Kan et al., 2011; Kovacs & Conway, 2016; Matzke et al., 2010). Even their verbal definitions are highly overlapping ([abstract] reasoning, solving [novel] problems, independence from prior knowledge; Gottfredson, 1997a; Schneider & McGrew, 2018). Process overlap theory offers an explanation of these findings in that Gf most purely measures the domain-general executive processes that give rise to the positive manifold, rendering it a good candidate for what we actually aspire to measure with *g* composites (see also Reynolds, 2013). Future studies should therefore explore if Gf could be measured more diversely (e.g., also with verbal analogies instead of only with visual tasks) and test the psychometric properties and usefulness of such diverse Gf measures compared to traditional *g* measures. Such Gf measures might also enable a better separation of Gf and Gv, and they could more likely be interpreted in a realist sense compared to *g* composites (Kovacs & Conway, 2019).

To conclude, from a realist stance, the validity of neither *g* nor broad ability composites has been sufficiently established to date. Furthermore, if we accept a formative interpretation of *g*, its measures can never be valid in a realist sense (Borsboom et al., 2004). However, if we approach the study of intelligence from a strict realist view only, then it is likely that we have a long way to go until we arrive at valid intelligence assessments (if valid assessments in this sense can be achieved at all for any but the narrowest psychological constructs; cf. Yarkoni & Westfall, 2017). Does this mean we should stop using intelligence tests? I would argue that it does not, if we adopt an instrumentalist stance.

Instrumentalist View. In an instrumentalist view, where constructs are judged according to their usefulness in predicting real-world outcomes, the definition of validity adopted in the present

dissertation is valid, and validity evidence as presented in the included studies is important to determine the usefulness of a measure. There are good reasons to believe that, despite an incomplete understanding of the processes underlying intelligence test performance, intelligence tests can still be useful, namely, to predict real-world behavior and external criteria. After all, this was also the goal with which the first intelligence test—the Binet–Simon (1905)—was developed, and with which current intelligence tests are usually applied in practice.

The extensive external validity evidence alluded to in the Introduction (e.g., Gottfredson & Deary, 2004; Roth et al., 2015; Schmidt & Hunter, 2004) supports the usefulness of current *g* composites, while current broad ability composites have been shown to demonstrate little diagnostic and treatment utility (McGill et al., 2018) and little incremental external validity for academic achievement (e.g., Canivez et al., 2014; Freberg et al., 2008; Nelson et al., 2013; Youngstrom et al., 1999), although there is some evidence that they could be useful for gifted individuals (e.g., Breit & Preckel, 2020). These findings support conclusions from the structural and convergent validity evidence presented in this dissertation and in related research and suggest that, from an instrumentalist view and for current intelligence tests, it is usually best to focus on the interpretation of *g* composites.

For future test construction, in this view, we should try to further enhance predictive validity by developing and selecting indicators based on their ability to *out-of-sample* predict outcomes we are interested in (cf. Yarkoni & Westfall, 2017). Proceeding this way, future studies could use machine learning techniques (see Kuhn & Johnson, 2013, for an introduction) to find models that maximize predictive power for individual life outcomes. These could be any variables we are interested in, for example, indicators of academic, occupational, or treatment success. Characteristics that define relevant subgroups of the reference population, such as sex, age, and skills in the test language, could also be included as predictors in the model, as could any other variables that are relevant for the outcome of interest. As a consequence, practitioners would no longer interpret test scores directly, but rather predicted outcomes. This would reduce their burden, as they would no longer have to choose between the many scores provided by contemporary intelligence tests for interpretation (see the example of the WISC-V above, with over 40 different scores), and likely lead to better diagnostic and treatment decisions (Canivez, 2013; Dawes et al., 1989; Meehl, 1954; see Grove et al., 2000, for a meta-analysis).

With this approach, we would not learn much about the causal processes leading to test outcomes (see Borsboom et al., 2004) but would instead focus on a test's predictive value or utility. Similarly, theories would be developed and judged not on the basis of their (seeming) explanatory value but on their ability to accurately predict future outcomes. Disadvantages of this approach include the risk of introducing social biases into the model (O'Neil, 2016) and the large sample sizes and amounts of data needed to develop and validate the complex machine learning models often required to obtain accurate out-of-sample predictions. Still, given the chances for better decision making in practice and the prospects for a better understanding of the determinants of real-life outcomes (Yarkoni & Westfall, 2017), such an approach could well be worthwhile to pursue.

Summary. In sum, we have to decide on the goals we want to pursue with intelligence assessment in the long run so that we can design and apply measures that are valid for these purposes. If our goal is to learn more about the causal processes underlying intelligence test outcomes (i.e., to maximize verisimilitude), we should aim for more process-based measures, for example, using a *neurocognitive psychometrics* approach (Schubert & Frischkorn, 2020). In this vein, we should probably also abandon *g* in favor of Gf, as the latter might be more likely to be interpreted in a realist sense than the former. If, on the other hand, our goal is to maximize the practical utility of intelligence tests (i.e., to maximize predictive validity), we should select and design tasks that are most predictive for the real-world outcomes we are interested in and combine these with other sources of information in statistical models, the outcomes of which practitioners can use for better-informed decisions.

There have been attempts at combining the two goals in test construction, for example, *automatic item generation* (Gierl & Haladyna, 2013), or the Cognitive Assessment System–Second Edition (Naglieri et al., 2014) guided by PASS theory. However, it is arguably difficult to optimize both goals at the same time. The more narrowly defined and process based the constructs are, the more likely they are to be interpretable in a realist sense, but the less predictive their measures usually are for real-world outcomes (Yarkoni & Westfall, 2017). On the other hand, if predictive validity is maximized, we likely learn little about the underlying cognitive processes, and the test scores are not interpretable in a realist sense. I would therefore argue that the best avenue for future intelligence assessment is to focus on one goal at a time to arrive at more valid intelligence score interpretations for both of these purposes.

6.4 Conclusion

This dissertation integrates validity evidence for intelligence test score interpretations from different analysis levels—from the total sample through the subgroup to the individual level—that has immediate practical implications. It provides further support for the validity of the interpretation of *g* composites—but only if they are based on a sufficient number of diverse and highly *g*-loaded subtests, and only if appropriate 95% CIs are used—and does not find support for the validity of the interpretation of most broad ability composites from concurrent intelligence tests. Individual-level results also suggest that intelligence test scores are not as interchangeable as is often assumed and that subtest content might have a larger influence on composites than previously thought. In the long run, we should thus create intelligence assessments in a way that ensures valid test score interpretations for individuals. I argue that to achieve this, we should tailor the construction of intelligence tests to the goal we want to achieve with them, that is, either to get a more basic understanding of the cognitive processes underlying test performance or to optimize the prediction of real-world outcomes and diagnostic and treatment decisions. Psychometric models such as the CHC model will probably not be sufficient to achieve any of these goals. Instead, we will likely have to abandon hierarchical measures of intelligence altogether and create more distinctive measures of *g* (or, for the former goal, Gf) and broad abilities. Ultimately, more separable tasks are important for both the study of underlying processes and potential incremental validity for real-world outcomes.

7. References

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APPENDIX A: Study 1

Grieder, S. & Grob, A. (2020). Exploratory factor analyses of the Intelligence and Development Scales–2: Implications for theory and practice. *Assessment, 27*(8), 1853–1869. https://doi.org/10.1177/1073191119845051

Exploratory Factor Analyses of the Intelligence and Development Scales–2: Implications for Theory and Practice

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Author Note

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Abstract

The factor structure of the intelligence and scholastic skills domains of the Intelligence and Development Scales–2 was examined using exploratory factor analyses with the standardization and validation sample (N = 2,030, aged 5 to 20 years). Results partly supported the seven proposed intelligence group factors. However, the theoretical factors Visual Processing and Abstract Reasoning as well as Verbal Reasoning and Long-Term Memory collapsed, resulting in a five-factor structure for intelligence. Adding the three scholastic skills subtests resulted in an additional factor Reading/Writing and in Logical–Mathematical Reasoning showing a loading on Abstract Visual Reasoning and the highest general factor loading. A data-driven separation of intelligence and scholastic skills is not evident. Omega reliability estimates based on Schmid–Leiman transformations revealed a strong general factor that accounted for most of the true score variance both overall and at the group factor level. The possible usefulness of factor scores is discussed.

Keywords: Cattell-Horn-Carroll, Intelligence and Development Scales–2, exploratory factor analysis, Schmid–Leiman transformation, omega coefficient, general factor

Introduction

Intelligence is one of the best researched constructs in psychology (Lubinski, 2004) and is highly predictive for a wide range of important outcomes, including academic achievement (Lubinski, 2004; Roth et al., 2015), occupational success, socioeconomic status, and income (Batty, Gale, Tynelius, Deary, & Rasmussen, 2009; Gottfredson, 2004; Lubinski, 2004) as well as health and longevity (Batty et al., 2009; Gottfredson & Deary, 2004). One major line of research focuses on the structure of intelligence. The long and fruitful research history on intelligence structure spans from Spearman's (1904) momentous two-factor or *g* theory to the most recent attempt to integrate two influential intelligence models—Cattell and Horn's Gf–Gc theory (Cattell, 1941; Horn, 1991; Horn & Cattell, 1966) and Carroll's (1993) three-stratum theory—into the Cattell–Horn–Carroll (CHC) model (McGrew, 1997, 2009). The CHC model postulates a hierarchical intelligence structure with over 80 narrow abilities or subtests on stratum I and 10 broad abilities or group factors on stratum II, namely, Fluid Reasoning, Comprehension–Knowledge, Short-Term Memory, Visual Processing, Auditory Processing, Long-Term Storage and Retrieval, Cognitive Processing Speed, Decision and Reaction Speed, Reading and Writing, and Quantitative Knowledge. As Carroll (1993) and Cattel and Horn (Cattell, 1941; Horn & Cattell, 1966) disagreed on the matter, the existence of a general factor on stratum III is left open to debate in CHC (McGrew, 2009). The CHC model is largely invariant across sex and culture (Keith, 2005) as well as across the life span (Keith & Reynolds, 2010). Since its introduction in the late 1990s, the CHC model has been widely referred to as a comprehensive framework in intelligence research and test construction (Alfonso, Flanagan, & Radwan, 2005; McGrew, 2009). Despite CHC's popularity, however, the combining of Cattell–Horn's and Carroll's theories into one framework is not universally accepted and there is increasing evidence against the usefulness of this integration.

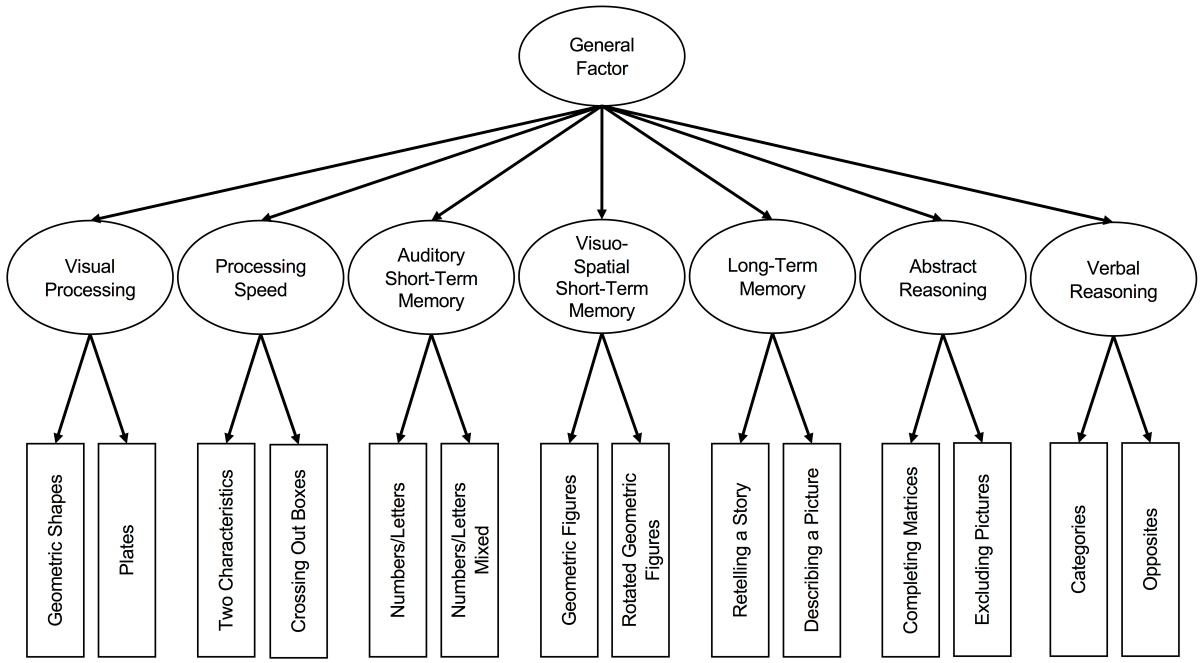
In a recent article, Canivez and Youngstrom (in press) point out several challenges to the CHC model, mainly concerning its focus on group factors and its de-emphasis on *g*. They review evidence on the poor reliability, (incremental) validity, and diagnostic value of group factor scores of prominent intelligence tests purposed to measure CHC broad abilities and highlight that the general factor accounts for the major part of true score variance in almost all of these group factor scores. Like Cucina and Howardson (2017), Canivez and Youngstrom (in press) conclude that Cattell–Horn's and Carroll's theories are incongruent and that the abundant evidence on the dominance of the general factor over broad abilities is more in line with Carroll than Cattell-Horn. These caveats notwithstanding, the CHC model is still widely regarded as the current state-of-the-art of intelligence structure.

A recently developed test battery that is partly based on the CHC model are the Intelligence and Development Scales–2 (IDS-2; Grob & Hagmann-von Arx, 2018a). The IDS-2 is the completely revised and extended successor of the Intelligence and Development Scales (IDS; Grob, Meyer, & Hagmann-von Arx, 2009) and assesses cognitive (intelligence, executive functions) and developmental (psychomotor skills, socioemotional competence, scholastic skills, and attitude toward work) functions

in 5- to 20-year-olds. Additional to their original standardization and validation in German-speaking countries, the IDS-2 are currently adapted and standardized in 11 more countries in Europe and South America.

The IDS-2 intelligence domain is largely grounded in CHC. It includes 14 subtests that enable the assessment of seven group factors corresponding to six of the 10 CHC group factors: Visual Processing, Processing Speed, Auditory Short-Term Memory, Visuospatial Short-Term Memory, Long-Term Memory, Abstract Reasoning, and Verbal Reasoning (see Figure 1). Additionally, a full-scale IQ is derived from all 14 subtests (Profile-IQ).[1]

Figure 1. *Theoretical Structure for the Intelligence Domain of the Intelligence and Development Scales–2 (IDS-2; Grob & Hagmann-von Arx, 2018a) With 14 Subtests on Stratum I, Seven Group Factors on Stratum II, and a General Factor on Stratum III*



The IDS-2 also measure scholastic skills with the subtests Logical–Mathematical Reasoning, Reading, and Writing. These subtests are based on the Swiss curriculum and resemble scholastic aptitude tests. However, they do not measure scholastic achievement directly. In the IDS-2, scholastic skills are included as developmental functions and not for the measurement of intelligence. This conceptual decision is reflected in evidence that scholastic abilities depend on prior knowledge (Bodovski & Farkas, 2007; Tarchi, 2010) and are associated with the socioeconomic and educational background of children's parents (Organization for Economic Co-operation and Development, 2016;

[1] Additionally, the IDS-2 include an IQ score without a factor profile (IQ) based on the first seven subtests (one per factor) as well as an intelligence screening (Screening-IQ) based on the two subtests with the highest general factor loadings in a confirmatory factor analysis of the first seven subtests (i.e., Completing Matrices and Categories; Grob & Hagmann-von Arx, 2018b).

Sirin, 2005). Nevertheless, scholastic skills represent important cognitive skills and are included as group factors in the CHC model. We therefore decided to integrate the intelligence and scholastic skills subtests in the subsequent analyses.

Validation of a test is crucial to determine its usefulness and ability to measure the assumed construct. One important aspect of validity is factorial or structural validity, that is, the presence of a theoretically postulated factor structure. This can be tested using exploratory and/or confirmatory factor analysis (EFA and CFA, respectively). Especially for new test batteries the use of both EFA and CFA is useful (Brown, 2015; Carroll, 1993; Reise, 2012; Schmitt, 2011). Information on the factor structure obtained by EFA can subsequently be used for informed CFA (Reise, 2012). Hence, a reasonable strategy for structural validation of a test is first to conduct EFA and then to test the factor structure found in EFA against possible alternative models using CFA.

Adopting this strategy, Canivez, Watkins, and Dombrowski (2016, 2017) recently examined the factor structure of the Wechsler Intelligence Scale for Children–Fifth Edition (WISC-V; Wechsler, 2014), using the correlation matrix for the whole standardization sample reported in the WISC-V manual. In their first study, EFA was used to determine the optimal number of factors and to investigate the variance portions explained by the general factor versus the group factors (Canivez et al., 2016). Contrary to the CFA results published in the WISC-V manual favoring a five-factor structure, EFA results suggested a four-factor solution as being most appropriate. The theoretical factors Visual Spatial and Fluid Reasoning were not separable and converged to a single factor Perceptual Reasoning. The resulting structure thus corresponded to that of the WISC-V precursor: the WISC-IV (Wechsler, 2003). Additionally, omega (ω) reliability analyses revealed that the general factor explained most of the true score variance in the indicators, while the reliable group-factor variance was presumably too small for individual interpretation. Canivez et al. (2017) subsequently sought to replicate results from EFA and test alternative models using CFA. Results from CFA confirmed those from EFA. The bifactor model with four group factors had the best fit, outperforming simpler models with fewer factors as well as the theoretical five-factor model. In fact, different versions of the five-factor model produced inadmissible or otherwise unsatisfactory solutions (e.g., negative variance for Fluid Reasoning). With the information from previous EFAs, these results were readily understood and the solution (combining the Visual Spatial and Fluid Reasoning factors) was obvious.

This finding of indistinct Fluid Reasoning and Visual-Spatial factors is consistent with other studies on the US WISC-V (Dombrowski, Canivez, & Watkins, 2018; Dombrowski, Canivez, Watkins, & Beaujean, 2015; M. R. Reynolds & Keith, 2017) as well as on other WISC-V versions (French: Lecerf & Canivez, 2018, Canadian: Watkins, Dombrowski, & Canivez, 2017, Spanish: Fenollar-Cortés & Watkins, 2018). As an alternative to a combined Perceptual Reasoning factor, M. R. Reynolds and Keith (2017), exclusively relying on CFA, found a bifactor model with five factors and correlated Fluid Reasoning and Visual-Spatial factors to be the best-fitting model and argued against combining these two factors. They assumed an intermediate Nonverbal Reasoning factor that accounts for the

correlation. Watkins et al. (2017) and Fenollar-Cortés and Watkins (2018) replicated the finding of this modified five-factor model being the best-fitting one. However, they argued against adopting this as the final model and instead interpreted a bifactor model with four factors and a combined Perceptual Reasoning factor, as this model was more parsimonious (had simple structure), is easier to interpret, and showed good global fit. In fact, in all three studies (Fenollar-Cortés & Watkins, 2018; M. R. Reynolds & Keith, 2017; Watkins et al., 2017) there was no substantial difference in fit between the modified five-factor model and the four-factor model (Burnham & Anderson, 2004; Chen, 2007; Cheung & Rensvold, 2002; Gignac, 2007).

This debate indicates that even for tests with a clearly hypothesized structure such as the WISC-V additional factor analytical studies (exploratory and confirmatory) are useful to get further insights in the factor structure. Especially, independent EFA can help avoiding confirmation bias due to adopting a well-fitting but less parsimonious model by finding the best data-driven model.

There are several studies that applied EFA to examine the structural validity of various other intelligence test batteries, including the Kaufman Assessment Battery for Children (KABC; Kaufman, 1993; Kaufman & Kaufman, 1983), the Kaufman Adolescent and Adult Intelligence Test (KAIT; Kaufman, 1993; Kaufman & Kaufman, 1993), the Reynolds Intellectual Assessment Scales (RIAS; C. R. Reynolds & Kamphaus, 2003; Dombrowski, Watkins, & Brogan, 2009), the Stanford–Binet Intelligence Scales–Fifth Edition (SB5; Roid, 2003; Canivez, 2008), the Wechsler Adult Intelligence Scale–Fourth Edition (WAIS-IV; Wechsler, 2008; Canivez & Watkins, 2010a, 2010b), the Woodcock–Johnson III (WJ-III; Woodcock, McGrew, & Mather, 2001; Dombrowski, 2014; Dombrowski & Watkins, 2013), and the WJ-IV (Schrank, McGrew, Mather, & Woodcock, 2014; Dombrowski, McGill, & Canivez, 2017, 2018b). For most of these test batteries, only CFAs are reported in the manual. However, results from independent EFA studies as those mentioned above often diverge from CFA results (Canivez, 2008; Dombrowski, 2014; Dombrowski et al., 2017; Dombrowski & Watkins, 2013) and, in line with the findings of Frazier and Youngstrom (2007), indicate substantial overfactoring for major intelligence test batteries.

Some of the above studies computed ω coefficients. Omega is a model-based reliability estimate used to determine the relative importance of the general factor versus group factors and has been suggested as a more appropriate alternative to coefficient alpha for estimating factor reliability of multidimensional scales (Crutzen & Peters, 2017; Gignac, 2014). It makes it possible to divide a composite's true score variance into general-factor variance (ω hierarchical [ω_h]) and group-factor variance (ω subscale [ω_s]). ω_h is the squared correlation of a composite with the general factor, and ω_s is the squared correlation of a composite with the group factor, net of general-factor variance. The total true score variance of a group factor composite or the whole scale, ω total, is denoted ω_t and refers to the squared correlation of a composite with the corresponding factor without removing unrelated factor variance (for the whole scale: including group-factor variance and for the group factor composites: including general-factor variance). Many authors caution against universal benchmarks for ω (Crutzen

& Peters, 2017; Gignac, 2014; Reise, Bonifay, & Haviland, 2013). A preliminary suggestion has been made for a minimum of .50—with .75 being preferred—for ω_h as well as ω_s (Reise et al., 2013). However, especially for ω_s, it is still unclear from which level individual interpretations of factor scores are supported (Gignac, 2014). Notably, ω_s has been found to be far less than .70 for factor scores of major intelligence test batteries (Canivez et al., 2016; Cucina & Howardson, 2017; Dombrowski, 2014; Nelson, Canivez, & Watkins, 2013; Watkins, 2017).

Structural validity evidence in the IDS-2 manual on theory, interpretation, and psychometric criteria (Grob & Hagmann-von Arx, 2018b) is given for the intelligence domain only and is based on CFA with the standardization sample (N = 1,672). Both the Profile-IQ model (see Figure 1) and the IQ model with seven subtests loading directly on a general factor displayed good fit (comparative fit index [CFI] = .97, Gamma Hat [GH] = .98, McDonald's noncentrality index [NCI] = .93, root mean square error of approximation [RMSEA] = .04, and CFI = .97, GH = .99, NCI = .98, RMSEA = .05, respectively). However, no alternative models (i.e., simpler models with fewer group factors or bifactor models) were tested and no EFAs were conducted. Moreover, the structure of the other (cognitive) IDS-2 domains and their (structural) interrelations were not examined in the manual.

To close this gap, in the present study we examined the factor structure of two IDS-2 domains—intelligence (14 subtests) and scholastic skills (three subtests)—using EFA. Four research questions were addressed: (1) How many factors should be extracted for the IDS-2 intelligence domain? (2) How many factors should be extracted for an integrative analysis of the IDS-2 domains intelligence and scholastic skills? (3) Does the theoretical separation of the IDS-2 domains intelligence and scholastic skills hold true? (4) How much (true score) variance is explained by the general factor versus the group factors? We expected results to be consistent with theoretical considerations on the IDS-2. Additionally, taking previous evidence from conceptual validation studies into account, we expected higher proportions of true score variance due to the general factor compared to the group factors.

Method

Participants

Data from the whole IDS-2 standardization and validation sample (N = 2,030) were used. German-speaking participants were from Switzerland (n = 1,097), Germany (n = 806), Austria (n = 83), Liechtenstein (n = 1), and France (n = 1). The subsamples for (a) intelligence and (b) intelligence and scholastic skills are described below.

Data on intelligence were available for 1,991 participants (M_{age} = 12.22 years, age range: 5.02–20.97 years, 52.0% female). About one third (36.0%) of participants' mothers had a university degree.

Data on intelligence and scholastic skills were available for 1,741 participants (M_{age} = 13.24, age range: 7.01–20.97 years, 51.9% female). This age range is restricted because the minimum age of 7 years was required for completing the Reading and Writing subtests. About one third (35.8%) of participants' mothers had a university degree.

Materials and Procedure

The IDS-2 is an individually administered test battery that assesses cognitive (intelligence, executive functions) as well as developmental (psychomotor skills, socioemotional competence, scholastic skills, and attitude toward work) functions in 5- to 20-year-olds with 30 subtests. Brief descriptions of the 14 subtests for intelligence and three subtests for scholastic skills can be found in Table S1 in the online supplemental material.

Participants were recruited via schools and psychosocial institutions for children and adolescents in Switzerland, Germany, and Austria. Administration took between 3.5 and 4.5 h and, if necessary, could be split into two sessions no more than 1 week apart. Written consent was obtained from children and adolescents (for 10- to 20-year-olds) and/or their parents (for 5- to 15-year-olds). Demographics were obtained from parents or adolescents using a personally administered questionnaire at the beginning of the (first) session. Participants received a gift card of their own choice worth 30 Swiss francs (Switzerland) or 25 euros in cash (Germany and Austria) for participation. Ethical approval was obtained from the Ethikkommission Nordwest- und Zentralschweiz [Ethics Commission Northwest and Central Switzerland] and from the responsible local ethics committees in Switzerland, Germany, and Austria.

Statistical Analyses

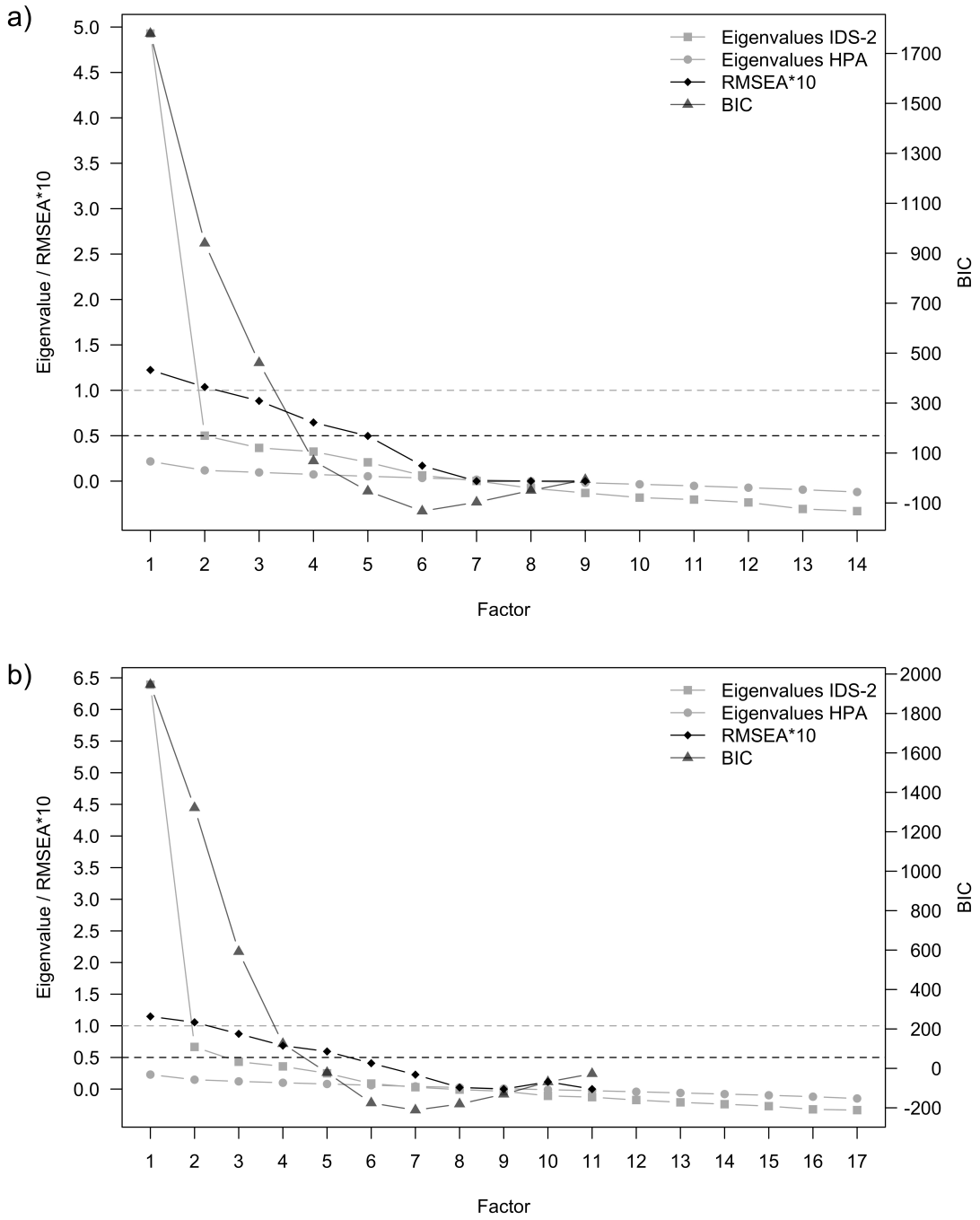
Analyses were conducted with adapted versions of functions from the psych package in R (R Core Team, 2016; Revelle, 2017), using the EFAdiff package (Grieder & Steiner, 2019b), following the suggestions of Grieder and Steiner (2019a). A stepwise procedure was adopted. First, only subtests from the intelligence domain were included. In a second step, the scholastic skills subtests were added.

Multiple criteria are available for determining the number of factors to extract. However, not all criteria are equally accurate. The Kaiser criterion (eigenvalue ≥ 1; Kaiser, 1960) and the scree test (Cattell, 1966), although frequently used, have been found to be arbitrary (Kaiser criterion), subjective (scree test), and often inaccurate (Fabrigar, Wegener, MacCallum, & Strahan, 1999; Hayton, Allen, & Scarpello, 2004) and therefore were not used in the present study. In contrast, Horn's parallel analysis (HPA; Horn, 1965) has been suggested as one of the most accurate extraction criteria (Hayton et al., 2004) and was therefore applied. Additionally, RMSEA (Browne & Cudeck, 1992; Steiger & Lind, 1980) and the Bayesian information criterion (BIC; Schwarz, 1978) were considered. For the RMSEA, it has been proposed that one should retain the solution with the lowest number of factors with RMSEA < .05 (Preacher, Zhang, Kim, & Mels, 2013). For the BIC, the smallest value indicates the best model. In addition to statistical criteria, factor solutions should always be interpreted in light of theory and current research (Fabrigar et al., 1999; Preacher et al., 2013; Watkins, 2018).

Principal axis (PA) factor analyses with promax rotation were conducted with the suggested number of factors. To account for the hierarchical data structure, Schmid–Leiman transformations (Schmid & Leiman, 1957) were subsequently performed. As a first step in this procedure, a second-order factor analysis is conducted on the factor correlations of the oblique solution of the PA factor

analysis. The factors from the second-order factor solution are then orthogonalized, which results in a factor solution with proportionality constraints. This makes it possible to determine the relative importance and reliability of the general factor versus the group factors. For this purpose, ω_t, ω_h, and ω_s were computed using the psych package in R (R Core Team, 2016; Revelle, 2017).

Figure 2. *Initial Eigenvalues and Factor Extraction Criteria for Exploratory Factor Analyses of the Intelligence and Development Scales–2 (IDS-2) Subtests for (a) Intelligence and (b) Intelligence and Scholastic Skills*



Note. Root mean square error of approximation (RMSEA) was multiplied by 10 to display it on a common scale with the eigenvalues. The gray dashed line indicates an eigenvalue of 1, and the black dashed line indicates the cutoff value for the RMSEA (.05 × 10). HPA = Horn's parallel analysis; BIC = Bayesian information criterion.

Results

We first report results for the intelligence domain and then for the intelligence and scholastic skills domains. Subtest intercorrelations, means, standard deviations, and reliabilities can be found in the Appendix (see Tables A1 and A2). Initial eigenvalues (Table S2) as well as results for the nonfinal PA solutions (Tables S3 to S6) are included in the online supplemental material. For simplicity, we refer to pattern coefficients of the rotated solutions as loadings.

Intelligence Domain

Seven intelligence group factors are postulated in the IDS-2 manual. Of the statistical criteria, HPA and BIC suggested six factors, and RMSEA suggested five factors (see Figure 2a). We therefore examined the seven-, six-, and five-factor PA solutions.

Table 1. *Five-Factor Solution of a Principal Axis Factor Analysis With Promax Rotation for the IDS-2 Intelligence Domain (*N *= 1,991)*

Subtest	General Factor [a]	Semantic Long-Term Memory	Visuospatial Short-Term Memory	Auditory Short-Term Memory	Processing Speed	Abstract Visual Reasoning	h^2	u^2
Geometric Shapes	.629	.021(.483)	-.049(.472)	-.018(.400)	.024(.473)	**.757(.743)**	.553	.447
Plates	.473	.017(.370)	.105(.405)	-.002(.306)	-.006(.338)	**.442(.517)**	.274	.726
Two Characteristics	.601	.013(.452)	-.033(.444)	.040(.358)	**.828(.806)**	-.043(.483)	.652	.348
Crossing Out Boxes	.597	-.062(.425)	-.036(.445)	-.014(.334)	**.733(.774)**	.164(.550)	.610	.390
Numbers/Letters	.731	.004(.564)	.003(.513)	**.926(.944)**	.022(.426)	.009(.543)	.893	.107
Numbers/Letters Mixed	.682	.085(.555)	.082(.514)	**.736(.817)**	.006(.398)	-.026(.497)	.680	.320
Geometric Figures	.521	-.085(.396)	**.707(.651)**	.029(.344)	-.047(.336)	.020(.421)	.429	.571
Rotated Geometric Figures	.604	-.107(.456)	**.815(.753)**	.020(.390)	-.032(.403)	.029(.491)	.574	.426
Completing Matrices	.637	.077(.517)	.190(.561)	-.017(.400)	.052(.478)	**.450(.649)**	.456	.544
Excluding Pictures	.564	.138(.482)	.171(.497)	.012(.371)	.010(.399)	**.333(.548)**	.342	.658
Categories	.654	**.752(.743)**	-.158(.449)	.016(.449)	-.060(.398)	.188(.543)	.573	.427
Opposites	.674	**.770(.765)**	-.132(.471)	.066(.487)	-.049(.408)	.114(.533)	.600	.400
Retelling a Story	.610	**.703(.703)**	.131(.519)	-.048(.374)	.077(.428)	-.161(.405)	.512	.488
Describing a Picture	.408	**.312(.426)**	**.331(.437)**	-.100(.208)	.080(.311)	-.147(.269)	.242	.758
% Variance	36.56	13.17	9.84	10.66	9.07	10.04	52.78	47.22
Factor correlations								
Semantic Long-Term Memory		1.000						
Visuospatial Short-Term Memory		.676	1.000					
Auditory Short-Term Memory		.582	.528	1.000				
Processing Speed		.564	.575	.425	1.000			
Abstract Visual Reasoning		.650	.664	.557	.625	1.000		

Note. Pattern (and structure) coefficients are displayed. Factor loadings ≥ .30 are in bold. Model fit: Bayesian information criterion = -52.61 and root mean square error of approximation = .050, 90% confidence interval [.043, .057]. IDS-2 = Intelligence and Development Scales–2; h^2 = communality; u^2 = uniqueness.
[a] First factor from an unrotated solution.

The seven-factor solution did not converge after 1,000,000 iterations. It resulted in a Heywood case and contained two factors with only one substantial loading each (Rotated Geometric Figures and Geometric Shapes), hence showing symptoms of overfactoring (see Table S3). The six-factor solution resulted in two Heywood cases as well and was therefore rejected (see Table S4). The five-factor solution was plausible and showed a minimum of two substantial loadings for each factor (see Table 1). The theoretically postulated factors Auditory Short-Term Memory, Processing Speed, and Visuospatial Short-Term Memory emerged. The theoretical factors Visual Processing and Abstract Reasoning merged into one factor, called Abstract Visual Reasoning, and the theoretical factors Verbal Reasoning and Long-Term Memory also collapsed to one factor, called Semantic Long-Term Memory. Describing a Picture showed a cross-loading on Visuospatial Short-Term Memory. High factor intercorrelations (.425-.676, median = .579) and loadings on the first factor from an unrotated solution (.408-.731, median = .607) suggest a strong general factor. Total variance explained by the factors ranged from 9.07% (Processing Speed) to 13.17% (Semantic Long-Term Memory). The whole model explained 52.78% of the variance. Thus, we proceeded with the five-factor solution for S–L transformation.

S–L transformation yielded the same loading pattern as above, but Excluding Pictures and Describing a Picture only showed near-substantial loadings on their factors. The loading of Excluding Pictures on Abstract Visual Reasoning was .185, the loading of Describing a Picture on Semantic Long-Term Memory was .178 and on Visuospatial Short-Term Memory .194. A strong general factor explained 32.25% of total and 61.06% of common variance (see Table 2). Total variance explained by the group factors ranged from 2.67% (Abstract Visual Reasoning) to 5.68% (Auditory Short-Term Memory). The whole model explained 52.82% of the variance.

We found that ω_t ranged from .652 for Visuospatial Short-Term Memory to .914 for the whole scale, ω_h for the general factor was .809, and ω_s for the group factors ranged from .140 (Abstract Visual Reasoning) to .436 (Auditory Short-Term Memory). For all composites, a higher proportion of true score variance was due to the general factor than to the respective group factor itself.

Table 2. *Schmid–Leiman Solution With Five Group Factors for the IDS-2 Intelligence Domain (*N = *1,991)*

	General Factor		Semantic Long-Term Memory		Visuospatial Short-Term Memory		Auditory Short-Term Memory		Processing Speed		Abstract Visual Reasoning			
Subtest	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	h^2	u^2
Geometric Shapes	.611	.373	.012	.000	-.029	.001	-.013	.000	.017	.000	**.421**	**.177**	.552	.448
Plates	.459	.211	.010	.000	.062	.004	-.002	.000	-.005	.000	**.246**	**.061**	.275	.725
Two Characteristics	.557	.310	.007	.000	-.020	.000	.030	.001	**.588**	**.346**	-.024	.001	.658	.342
Crossing Out Boxes	.562	.316	-.036	.001	-.021	.000	-.011	.000	**.521**	**.271**	.091	.008	.598	.402
Numbers/Letters	.643	.413	.003	.000	.002	.000	**.693**	**.480**	.016	.000	.005	.000	.894	.106
Numbers/Letters Mixed	.607	.368	.049	.002	.048	.002	**.551**	**.304**	.004	.000	-.015	.000	.676	.324
Geometric Figures	.505	.255	-.049	.002	**.415**	**.172**	.022	.000	-.034	.001	.011	.000	.432	.568
Rotated Geometric Figures	.587	.345	-.061	.004	**.478**	**.228**	.015	.000	-.023	.001	.016	.000	.577	.423
Completing Matrices	.616	.379	.044	.002	.111	.012	-.013	.000	.037	.001	**.250**	**.063**	.457	.543
Excluding Pictures	.542	.294	.079	.006	.100	.010	.009	.000	.007	.000	***.185***	***.034***	.345	.655
Categories	.613	.376	**.430**	**.185**	-.093	.009	.012	.000	-.043	.002	.105	.011	.583	.417
Opposites	.628	.394	**.440**	**.194**	-.077	.006	.049	.002	-.035	.001	.064	.004	.602	.398
Retelling a Story	.572	.327	**.402**	**.162**	.077	.006	-.036	.001	.055	.003	-.089	.008	.507	.493
Describing a Picture	.391	.153	***.178***	***.032***	***.194***	***.038***	-.075	.006	.057	.003	-.082	.007	.238	.762
% Total variance		32.25		4.22		3.49		5.68		4.50		2.67	52.82	47.18
% Common variance		61.06		7.99		6.61		10.75		8.53		5.06		
Omega hierarchical		.809		.537		.390		.440		.385		.571		
Omega subscale		.090		.233		.261		.436		.379		.140		
Omega total		.914		.770		.652		.876		.765		.710		

Note. Group factor loadings ≥ .20 are in bold. Excluding Pictures and Describing a Picture had no substantial loading on any group factor, although the loadings were near .20 for Factor 5 (Excluding Pictures) and Factors 1 and 2 (Describing a Picture; indicated in italics). For the calculation of omegas, Excluding Pictures was assigned to Abstract Visual Reasoning and Describing a Picture was assigned to Verbal Reasoning and Long-Term Memory. IDS-2 = Intelligence and Development Scales–2; h^2 = communality; u^2 = uniqueness.

Intelligence and Scholastic Skills Domains

Eight factors should emerge based on information in the IDS-2 manual: seven group factors for intelligence and one factor for scholastic skills. Of the statistical criteria, BIC suggested seven factors and HPA and RMSEA suggested six (see Figure 2b). We therefore examined the eight-, seven-, and six-factor PA solutions.

The eight- and seven-factor solutions both resulted in two Heywood cases and were therefore rejected (see Tables S5 and S6). The six-factor solution was plausible and the loading pattern was similar to the five-factor solution above, with two exceptions: (a) the cross-loading of Describing a Picture on Visuospatial Short-Term Memory was no longer substantial and (b) Categories and Opposites both showed a cross-loading on Abstract Visual Reasoning (see Table 3).

A Reading/Writing factor emerged. Logical–Mathematical Reasoning loaded on the Abstract Visual Reasoning factor and had an additional small but insubstantial loading on the Reading/Writing

factor. High factor intercorrelations (.390-.677, median = .581) and loadings on the first factor from an unrotated solution (.423-.724, median = .635) suggest a strong general factor. Total variance explained by the factors ranged from 7.07% (Visuospatial Short-Term Memory) to 13.41% (Abstract Visual Reasoning). The whole model explained 55.75% of the variance. Thus, we proceeded with the six-factor solution for S–L transformation.

Table 3. *Six-Factor Solution of a Principal Axis Factor Analysis With Promax Rotation for the IDS-2 Domains Intelligence and Scholastic Skills (*N *= 1,741)*

Subtest	General Factor [a]	Abstract Visual Reasoning	Auditory Short-Term Memory	Reading/ Writing	Semantic Long-Term Memory	Processing Speed	Visuospatial Short-Term Memory	h^2	u^2
Geometric Shapes	.618	**.827(.720)**	- .021(.409)	- .085(.381)	- .096(.439)	.060(.448)	- .011(.437)	.533	.467
Plates	.486	**.471(.523)**	- .038(.321)	.014(.319)	- .029(.371)	.013(.330)	.127(.396)	.283	.717
Two Characteristics	.615	.009(.525)	.048(.388)	.055(.426)	.032(.455)	**.778(.819)**	- .045(.413)	.680	.320
Crossing Out Boxes	.598	.187(.556)	- .014(.348)	- .008(.381)	- .013(.427)	**.719(.790)**	- .048(.412)	.639	.361
Numbers/Letters	.724	.023(.592)	**.929(.924)**	- .022(.570)	- .035(.517)	.032(.383)	.004(.472)	.856	.144
Numbers/Letters Mixed	.698	-.020(.562)	**.791(.848)**	.028(.560)	.033(.529)	.000(.357)	.066(.479)	.725	.275
Geometric Figures	.507	.036(.444)	.018(.371)	.049(.280)	- .018(.408)	- .066(.310)	**.670(.675)**	.461	.539
Rotated Geometric Figures	.581	.077(.517)	.026(.421)	.023(.319)	.008(.471)	- .040(.372)	**.685(.739)**	.553	.447
Completing Matrices	.637	**.582(.679)**	- .046(.414)	- .033(.400)	.018(.500)	.058(.457)	.161(.528)	.483	.517
Excluding Pictures	.545	**.460(.570)**	.028(.381)	- .102(.330)	.124(.459)	.003(.355)	.109(.447)	.345	.655
Categories	.689	**.359(.649)**	.025(.496)	.106(.604)	**.528(.719)**	- .093(.339)	- .177(.356)	.594	.406
Opposites	.693	**.303(.638)**	.062(.517)	.113(.606)	**.518(.719)**	- .102(.334)	- .129(.380)	.583	.417
Retelling a Story	.635	-.096(.503)	- .039(.407)	.043(.503)	**.724(.740)**	.062(.401)	.081(.468)	.556	.444
Describing a Picture	.423	-.169(.319)	- .049(.243)	- .107(.245)	**.529(.502)**	.114(.326)	.236(.433)	.313	.687
Logical-Mathematical Reasoning	.721	**.464(.709)**	.049(.536)	.224(.601)	.020(.576)	.065(.477)	.031(.468)	.547	.453
Reading	.670	-.093(.517)	- .027(.509)	**.814(.829)**	.064(.590)	.105(.419)	.008(.314)	.697	.303
Writing	.641	.020(.520)	- .003(.513)	**.787(.791)**	- .014(.547)	- .056(.318)	.075(.328)	.630	.370
% Variance	38.69	13.41	9.18	8.99	9.49	7.62	7.07	55.75	44.25
Factor correlations									
Abstract Visual Reasoning		1.000							
Auditory Short-Term Memory		.631	1.000						
Reading/Writing		.633	.630	1.000					
Semantic Long-Term Memory		.704	.573	.677	1.000				
Processing Speed		.587	.390	.423	.486	1.000			
Visuospatial Short-Term Memory		.623	.501	.354	.581	.501	1.000		

Note. Pattern (and structure) coefficients are displayed. Factor loadings ≥ .30 are in bold. Model fit: Bayesian information criterion = -175.46 and root mean square error of approximation = .041, 90% confidence interval [.035, .047]. IDS-2 = Intelligence and Development Scales–2; h^2 = communality; u^2 = uniqueness.
[a] First factor from an unrotated solution.

The S–L solution resulted in the same loading pattern as above, except that Categories and Opposites no longer showed cross-loadings (see Table 4). The general factor explained 34.86% of total and 62.51% of common variance. The group factors explained between 2.54% (Semantic Long-Term Memory) and 4.32% (Processing Speed) of total variance. The whole model explained 55.77% of the variance.

We found that ω_t ranged from .670 for Visuospatial Short-Term Memory to .936 for the whole scale, ω_h for the general factor was .842, and ω_s for the group factors ranged from .130 (Abstract Visual Reasoning) to .417 (Processing Speed). Except for Processing Speed, a higher proportion of true score variance was due to the general factor than to the respective group factor itself.

Table 4. *Schmid–Leiman Solution With Six Group Factors for the IDS-2 Domains Intelligence and Scholastic Skills (*N *= 1,741)*

	General Factor		Abstract Visual Reasoning		Auditory Short-Term Memory		Reading/ Writing		Semantic Long-Term Memory		Processing Speed		Visuospatial Short-Term Memory			
Subtest	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	*b*	b^2	h^2	u^2
Geometric Shapes	.602	.362	**.388**	**.151**	-.015	.000	-.058	.003	-.054	.003	.047	.002	-.008	.000	.521	.479
Plates	.469	.220	**.221**	**.049**	-.026	.001	.009	.000	-.016	.000	.010	.000	.094	.009	.278	.722
Two Characteristics	.562	.316	.004	.000	.033	.001	.037	.001	.018	.000	**.611**	**.373**	-.033	.001	.693	.307
Crossing Out Boxes	.552	.305	.088	.008	-.009	.000	-.005	.000	-.007	.000	**.565**	**.319**	-.035	.001	.633	.367
Numbers/Letters	.676	.457	.011	.000	**.635**	**.403**	-.015	.000	-.019	.000	.025	.001	.003	.000	.862	.138
Numbers/Letters Mixed	.652	.425	- .009	.000	**.541**	**.293**	.019	.000	.018	.000	.000	.000	.049	.002	.721	.279
Geometric Figures	.476	.227	.017	.000	.012	.000	.033	.001	-.010	.000	-.052	.003	**.495**	**.245**	.476	.524
Rotated Geometric Figures	.547	.299	.036	.001	.018	.000	.016	.000	.005	.000	-.031	.001	**.506**	**.256**	.558	.442
Completing Matrices	.615	.378	**.273**	**.075**	-.032	.001	-.023	.001	.010	.000	.046	.002	.119	.014	.471	.529
Excluding Pictures	.529	.280	**.216**	**.047**	.019	.000	-.069	.005	.069	.005	.002	.000	.081	.007	.343	.657
Categories	.674	.454	.168	.028	.017	.000	.072	.005	**.295**	**.087**	-.073	.005	-.131	.017	.597	.403
Opposites	.675	.456	.142	.020	.043	.002	.077	.006	**.289**	**.084**	-.080	.006	-.096	.009	.583	.417
Retelling a Story	.611	.373	-.045	.002	-.026	.001	.029	.001	**.405**	**.164**	.049	.002	.060	.004	.547	.453
Describing a Picture	.405	.164	-.079	.006	-.034	.001	-.072	.005	**.295**	**.087**	.090	.008	.175	.031	.302	.698
Logical–Mathematical Reasoning	.688	.473	**.218**	**.048**	.033	.001	.152	.023	.011	.000	.051	.003	.023	.001	.548	.452
Reading	.619	.383	-.044	.002	-.019	.000	**.553**	**.306**	.036	.001	.082	.007	.006	.000	.699	.301
Writing	.596	.355	.009	.000	-.002	.000	**.535**	**.286**	-.008	.000	-.044	.002	.055	.003	.647	.353
% Total variance		34.86		2.57		4.15		3.79		2.54		4.32		3.53	55.77	44.23
% Common variance		62.51		4.60		7.45		6.80		4.56		7.75		6.33		
Omega hierarchical		.842		.633		.494		.446		.605		.374		.342		
Omega subscale		.071		.130		.388		.357		.178		.417		.328		
Omega total		.936		.763		.882		.803		.784		.791		.670		

Note. Group factor loadings ≥ .20 are in bold. IDS-2 = Intelligence and Development Scales–2; h^2 = communality; u^2 = uniqueness.

Discussion

The aim of this study was to examine the factor structure of the IDS-2 intelligence and scholastic skills domains with EFA. Three of the seven theoretically proposed group factors for the IDS-2 intelligence domain are supported. However, Visual Processing and Abstract Reasoning as well as Verbal Reasoning and Long-Term Memory are not separable, rendering a five-factor structure for intelligence with Abstract Visual Reasoning, Processing Speed, Auditory Short-Term Memory, Visuospatial Short-Term Memory, and Semantic Long-Term Memory. Adding scholastic skills resulted in a six-factor structure with an additional factor Reading/Writing. Logical–Mathematical Reasoning had a loading on Abstract Visual Reasoning and the highest general factor loading. A strong general factor counters a strict separation of the intelligence and scholastic skills domains, and ω reliability estimates based on S–L solutions confirmed our assumption that for most group factor composites a higher proportion of true score variance is actually due to the general factor.

Intelligence

Three of the seven theoretically proposed intelligence group factors emerged in all analyses. Auditory Short-Term Memory, Visuospatial Short-Term Memory, and Processing Speed were confirmed as group factors in the IDS-2.

In line with Baddeley's (2003) working memory model, Auditory and Visuospatial Short-Term Memory were separate factors in all analyses. This may indicate that the traditional measurement with digit span tasks misses an important aspect of short-term memory (see also Alloway, Gathercole, & Pickering, 2006) and reflects the finding that visual memory tasks often fail to load on a common short-term memory factor with verbal memory tasks (Keith & Reynolds, 2010).

Contrary to theoretical assumptions, the factors Abstract Reasoning and Visual Processing collapsed to a single factor throughout all analyses, providing evidence that the four corresponding subtests could in fact tap the same construct. This finding is in line with previous research on major intelligence test batteries (Canivez et al., 2016, 2017; Keith & Reynolds, 2010). For example, as stated earlier, EFAs conducted by Canivez et al. (2016) for the WISC-V did not support the theoretically proposed separation of a Visual Spatial and a Fluid Reasoning factor.

Additionally, the factors Verbal Reasoning and Long-Term Memory collapsed to one factor as well. This is not in line with theoretical expectations, but it is plausible given the nature of the subtests involved. The Long-Term Memory subtests both require memorization and recall of verbal information, and the Verbal Reasoning subtests require retrieval of verbal information from long-term memory. The higher loading of Retelling a Story on Semantic Long-Term Memory reflects its exclusively verbal content. Describing a Picture had a weaker loading on Semantic Long-Term Memory, and it showed a cross-loading of approximately the same size on Visuospatial Short-Term Memory in some of the analyses. This subtest requires memorization and recall of both verbal and visuospatial information, therefore tapping both verbal and visuospatial memory. It would be interesting to see if, analogous to the separation of Auditory and Visuospatial Short-Term Memory, Verbal and Visuospatial Long-Term

Memory could be separated on stratum II as well. A separate Visuospatial Long-Term Memory measure could be especially beneficial for the measurement of long-term memory in individuals with language disorder, insufficient knowledge of the test language, and dual-language learners; provided that incremental validity to the general factor and the other group factors is demonstrated. However, we were not able to further examine this possibility since only one subtest each assessed the verbal and visuospatial components of Long-Term Memory. Finally, the low number of subtests per factor and strong general factor may have contributed to the collapse of theoretically meaningful factors here, as we discuss later.

Adding Scholastic Skills

As expected based on the CHC model, a homogenous factor Reading/Writing emerged that (together with *g*) explained most of the variance in the two subtests. The relatively high general factor loadings of these subtests and high factor intercorrelations in the PA solution suggest Reading/Writing and the intelligence group factors are part of the same overreaching domain.

Logical–Mathematical Reasoning showed a substantial loading on Abstract Visual Reasoning. This is in line with CHC, where Quantitative Reasoning is a narrow ability under Fluid Intelligence (McGrew, 2009). Although the CHC model includes Quantitative Knowledge (the acquired store of mathematical knowledge) as a separate group factor, reasoning with this knowledge is not part of this broad ability (McGrew, 2009). Additionally, Logical–Mathematical Reasoning displayed the highest general factor loading of all subtests in the S–L solution and did not show particularly high uniqueness. This finding of high general factor and relatively low group factor influence on a mathematical subtest is consistent with studies on the WISC-V (Canivez et al., 2016, 2017; M. R. Reynolds & Keith, 2017; Watkins et al., 2017), the WAIS-IV (Canivez & Watkins, 2010b), the SB5 (Canivez, 2008), and the WJ IV (Dombrowski, McGill, et al., 2018b) for example. Hence, Logical–Mathematical Reasoning is a potent indicator of general mental ability. However, we were not able to test the possibility of a separate Quantitative Reasoning factor due to the fact of a single subtest on mathematical skills.

Finally, in both the PA and S–L solutions, Logical–Mathematical Reasoning showed a small but insubstantial loading on the Reading/Writing factor, suggesting a small additional clustering of the curriculum-based scholastic skills subtests.

Factor Reliability

We found that ω_h is above .80 for the general factor for both S–L solutions; thus, the true score variance due to the general factor is probably sufficient to build a full-scale IQ. In contrast, ω_s is below .50 for all group factors in both analyses, leaving doubts about the justification of factor scores (Reise et al., 2013). In fact, a comparison of ω_h and ω_s for the group factor composites shows that an important—and in most cases even larger—amount of true score variance is actually due to the general factor and not the group factor itself. These results are in line with findings on major intelligence test batteries, for example, the Differential Ability Scales (DAS; Elliott, 1990; Cucina & Howardson, 2017), the KABC (Kaufman & Kaufman, 2004; Cucina & Howardson, 2017), the KAIT (Cucina &

Howardson, 2017), the WAIS-IV (Nelson et al., 2013; Watkins, 2017), the WISC-V (Canivez et al., 2016), the WJ-III (Cucina & Howardson, 2017; Dombrowski, 2014), and the WJ-IV (Dombrowski et al., 2017; Dombrowski, McGill, & Canivez, 2018a). In these studies, ω_h for the general factor ranged from .74 for the WAIS-IV (Nelson et al., 2013) to .91 for the WJ-III (Cucina & Howardson, 2017). However, with the exceptions of Processing Speed for the WAIS-IV (.64; Nelson et al., 2013) and WISC-V (.51; Canivez et al., 2016) and Long-Term Memory for the WJ-III (.52; Dombrowski, 2014), ω_s was below .50 for all group factors in the aforementioned studies.

To further determine the usefulness and justification of group factors, future studies should systematically vary sample heterogeneity as well as age and mean level of intelligence. It has been found that a general factor explains more variance in more heterogeneous samples and at lower levels of abilities (Kan, Kievit, Dolan, & van der Maas, 2011; Kovacs & Conway, 2016; Tucker-Drob, 2009). Thus, factor differentiation could have a profound influence on the amount of true score variance attributable to the general factor versus the group factors. Another approach to this would be to examine the effect of intraindividual variability in factor scores directly, assuming that the higher the intraindividual variability in a particular sample, the higher the group-factor reliability.

Heywood Cases

The seven- and six-factor solutions for the intelligence domain and the eight- and seven-factor solutions for the intelligence and scholastic skills domains all resulted in Heywood cases. The solutions with the theoretically proposed number of factors (seven and eight, respectively) not only showed Heywood cases, but the seven-factor model for intelligence did not convergence after 1,000,000 iterations, and both solutions had factors with only one substantial loading. Hence, these solutions are not plausible. The solutions with one factor less than hypothesized, however (i.e., the six- and seven-factor solutions, respectively), both converged and had a minimum of two substantial loadings on each factor. Both consistently showed a collapse of Abstract Reasoning and Visual Processing, but a separation of Long-Term Memory and Verbal Reasoning, although with a cross-loading of Retelling a Story on Verbal Reasoning. Nevertheless, there were Heywood cases for Crossing Out Boxes and Numbers/Letters (six-factor intelligence) and for Numbers/Letters and Rotated Geometric Figures (seven-factor intelligence and scholastic skills).

Simulation studies showed that Heywood cases can indicate both model misspecification (in this case a wrong number of factors) and poor factor recovery (Briggs & MacCallum, 2003; de Winter & Dodou, 2012; Kolenikov & Bollen, 2012; Ximénez, 2009). At least two potential issues could have influenced the performance of factor analysis in our study: the presence of weak factors and a low number of indicators (two per factor).

It has been shown that PA factor analysis is better able to recover weak factors and with this is less prone to Heywood cases compared to, for example, maximum likelihood (Briggs & MacCallum, 2003; de Winter & Dodou, 2012; Fabrigar et al., 1999; Ximénez, 2009). Thus, this might have been less of an issue here.

Concerning the second issue, simulation studies found that a low number of indicators per factor can lead to unstable solutions or underidentification and can prevent a correct factor recovery (Costello & Osborne, 2005; de Winter & Dodou, 2012; MacCallum, Widaman, Zhang, & Hong, 1999; Velicer & Fava, 1998). For these reasons, a minimum number of three (Costello & Osborne, 2005; Velicer & Fava, 1998) or four (Fabrigar et al., 1999) indicators per factor has been recommended.

To conclude, one should be careful not to overinterpret the collapse of potentially meaningful (if weak) factors in this study in terms of the underlying theory. It could be useful to adapt the current instrument to achieve higher overdetermination of factors and then reinvestigate its factor structure. Of course, the resulting expansion of the test would have to be justified by a substantial gain in information on a group factor level.

Implications

The present results based on the IDS-2 standardization and validation sample provide evidence for some of the CHC-based group factors. However, as stated above, a strong general factor and the low group factor reliabilities are more consistent with Carroll's (1993) conception of the general factor-to-broad abilities relationship than with Cattell–Horn's model (Cattell, 1941; Horn & Cattell, 1966). This result, together with findings on several other major intelligence test batteries, therefore challenges the usefulness of the CHC model over and above Carroll's model (Canivez & Youngstrom, in press; Cucina & Howardson, 2017).

Inspection of model-based reliabilities suggests that individual interpretation should be based primarily on the full-scale IQ and that factor scores should be interpreted with caution. Further studies are needed to determine the circumstances under which factor profiles could still be particularly useful, for example, for individuals with expected strong intraindividual variability in factor scores. To achieve this, diagnostic and treatment utility as well as incremental validity of factor scores should be determined. Additionally, Latent Profile Analysis of various normative groups might deliver further insights into the potential usefulness of factor profiles. Finally, the domination of the general factor for this test suggests that an additional IQ based on a small number of subtests (e.g. four, as in the RIAS-2; C. R. Reynolds & Kamphaus, 2015) could be useful for a reliable and valid, but time-saving measurement of general mental ability.

Strengths and Limitations

A strength of this study is the large representative sample. Second, the IDS-2 include a large number of group factors, enabling a comprehensive estimate of cognitive abilities and a fair test of the underlying CHC model. Third, ω reliability analyses allowed for a separation of the true score variance of the group factor composites as well as the whole scale in general-factor and group-factor variance. Despite these advantages and despite growing evidence of its superiority over Cronbach's alpha as a reliability estimate of multidimensional scales (Crutzen & Peters, 2017; Gignac, 2014; Reise et al., 2013; Watkins, 2017), ω is still less reported (Padilla & Divers, 2016; Reise et al., 2013).

Our study has at least two limitations. First, the hypothesized intelligence factors are represented by only two subtests each, which is suboptimal (Costello & Osborne, 2005; Fabrigar et al., 1999; Watkins, 2018). However, from a practical perspective, to reduce the time investment of the participant, only a limited number of subtests can be included in a test. While constructing the IDS-2, it was decided that a higher number of factors with a lower number of indicators was more beneficial for a holistic measurement of intelligence than a low number of factors with more indicators. However, as stated above, developing more subtests per factor should be a goal for future adaptations of the test. Second, we used exploratory (i.e., data driven) methods, which made it impossible to test between theoretically specified models as, for example, with CFA. However, the benefit of EFA is precisely that it is unaffected by prior theoretical considerations of the investigator. It is therefore useful to use EFA as well as CFA to determine the factor structure of a given construct. Hence, as a next step, the EFA-based structure found here will be tested against possible alternative models using CFA, and age and sex invariance will be examined.

Conclusion

In conclusion, the theoretical structure of the IDS-2 intelligence domain was partly supported, but the hypothesized factors Visual Processing and Abstract Reasoning collapsed to Abstract Visual Reasoning and Verbal Reasoning and Long-Term Memory collapsed to Semantic Long-Term Memory. Additionally, a data-driven separation of the intelligence and scholastic skills domains did not hold, as the related subtests were encompassed by a strong general factor explaining most of the true score variance. For most group factor composites, the true score variance explained by the group factor was considerably smaller than that explained by the general factor. Thus, it remains to be empirically investigated under which circumstances (e.g., high intraindividual variability due to developmental abnormalities) the interpretation of individual factor scores could still be useful.

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Appendix

Table A1. *Intercorrelations of Scaled Subtest Scores for the IDS-2 Intelligence Domain (N = 1,991)*

Subtest	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Geometric Shapes	-	.40	.35	.43	.39	.36	.31	.35	**.47**	.38	.41	.40	.31	.18
2. Plates	**.40**	-	.26	.28	.30	.29	.23	.29	.36	.30	.27	.28	.25	.22
3. Two Characteristics	.35	.26	-	**.62**	.35	.33	.26	.31	.38	.31	.33	.33	.33	.24
4. Crossing Out Boxes	.43	.28	**.62**	-	.34	.30	.27	.33	.38	.32	.31	.32	.31	.22
5. Numbers/Letters	.39	.30	.35	.34	-	**.77**	.34	.37	.40	.37	.43	.46	.37	.21
6. Numbers/Letters Mixed	.36	.29	.33	.30	**.77**	-	.32	.38	.37	.33	.41	.45	.38	.22
7. Geometric Figures	.31	.23	.26	.27	.34	.32	-	**.53**	.34	.30	.28	.29	.29	.24
8. Rotated Geometric Figures	.35	.29	.31	.33	.37	.38	**.53**	-	.40	.35	.31	.34	.33	.29
9. Completing Matrices	**.47**	.36	.38	.38	.40	.37	.34	.40	-	.44	.39	.38	.36	.26
10. Excluding Pictures	.38	.30	.31	.32	.37	.33	.30	.35	**.44**	-	.37	.36	.33	.25
11. Categories	.41	.27	.33	.31	.43	.41	.28	.31	.39	.37	-	**.63**	.48	.25
12. Opposites	.40	.28	.33	.32	.46	.45	.29	.34	.38	.36	**.63**	-	.51	.25
13. Retelling a Story	.31	.25	.33	.31	.37	.38	.29	.33	.36	.33	.48	**.51**	-	.40
14. Describing a Picture	.18	.22	.24	.22	.21	.22	.24	.29	.26	.25	.25	.25	**.40**	-
M	10.07	10.06	9.95	9.96	10.38	10.40	10.17	10.09	10.26	10.23	10.33	10.35	10.06	10.27
SD	3.20	3.18	3.13	3.15	3.20	3.11	3.06	3.07	3.14	3.18	3.22	3.16	3.16	3.08
Skewness	-0.11	-0.14	-0.31	-0.18	-0.20	-0.24	0.28	0.11	0.01	-0.08	-0.49	-0.47	-0.40	0.07
Kurtosis	0.34	0.30	0.81	0.79	0.51	0.63	0.43	0.91	-0.04	0.31	0.59	0.52	0.45	0.34
Cronbach's alpha	.97	.97	.97	.96	.96	.96	.92	.93	.98	.96	.98	.98	.96	.95
McDonald's omega	.97	.97	.97	.97	.96	.96	.92	.93	.98	.96	.98	.98	.96	.95

Note. The highest correlations (row-wise) are in bold. Correlations of related intelligence subtests are highlighted in gray. All correlations are significant with $p < .001$. IDS-2 = Intelligence and Development Scales–2.

Table A2. *Intercorrelations of Scaled Subtest Scores for the IDS-2 Domains Intelligence and Scholastic Skills (N = 1,741)*

Subtest	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
Intelligence																	
1. Geometric Shapes	-	.41	.37	.44	.39	.37	.31	.35	.48	.39	.42	.40	.33	.20	**.50**	.32	.30
2. Plates	**.41**	-	.28	.30	.30	.30	.25	.31	.37	.31	.29	.31	.28	.23	.36	.26	.29
3. Two Characteristics	.37	.28	-	**.66**	.37	.35	.27	.32	.40	.32	.34	.34	.35	.27	.45	.40	.32
4. Crossing Out Boxes	.44	.30	**.66**	-	.34	.31	.27	.33	.40	.32	.34	.33	.34	.25	.42	.36	.29
5. Numbers/Letters	.39	.30	.37	.34	-	**.78**	.35	.38	.39	.37	.45	.46	.37	.23	.50	.46	.46
6. Numbers/Letters Mixed	.37	.30	.35	.31	**.78**	-	.34	.40	.37	.33	.43	.46	.39	.24	.48	.45	.46
7. Geometric Figures	.31	.25	.27	.27	.35	.34	-	**.53**	.35	.30	.29	.29	.31	.27	.34	.23	.25
8. Rotated Geometric Figures	.35	.31	.32	.33	.38	.40	**.53**	-	.41	.34	.33	.36	.36	.31	.38	.28	.27
9. Completing Matrices	.48	.37	.40	.40	.39	.37	.35	.41	-	.45	.41	.40	.38	.27	**.49**	.33	.34
10. Excluding Pictures	.39	.31	.32	.32	.37	.33	.30	.34	**.45**	-	.38	.36	.33	.26	.40	.27	.29
11. Categories	.42	.29	.34	.34	.45	.43	.29	.33	.41	.38	-	**.64**	.49	.28	.50	.50	.48
12. Opposites	.40	.31	.34	.33	.46	.46	.29	.36	.40	.36	**.64**	-	.50	.28	.51	.49	.48
13. Retelling a Story	.33	.28	.35	.34	.37	.39	.31	.36	.38	.33	.49	**.50**	-	.43	.43	.45	.41
14. Describing a Picture	.20	.23	.27	.25	.23	.24	.27	.31	.27	.26	.28	.28	**.43**	-	.25	.24	.21
Scholastic skills																	
15. Logical–Mathematical Reasoning	.50	.36	.45	.42	.50	.48	.34	.38	.49	.40	.50	**.51**	.43	.25	-	.50	.48
16. Reading	.32	.26	.40	.36	.46	.45	.23	.28	.33	.27	.50	.49	.45	.24	.50	-	**.66**
17. Writing	.30	.29	.32	.29	.46	.46	.25	.27	.34	.29	.48	.48	.41	.21	.48	**.66**	-
M	10.05	10.03	9.97	9.95	10.37	10.36	10.15	10.09	10.19	10.22	10.33	10.32	10.03	10.29	10.07	9.86	10.18
SD	3.22	3.18	3.13	3.15	3.23	3.13	3.08	3.15	3.22	3.22	3.25	3.09	3.16	3.12	3.22	2.63	3.17
Skewness	-0.07	-0.17	-0.27	-0.10	-0.21	-0.22	0.39	0.16	0.02	-0.09	-0.43	-0.42	-0.40	0.12	-0.32	-0.54	-0.77
Kurtosis	0.34	0.34	0.95	0.85	0.59	0.69	0.49	0.88	-0.15	0.27	0.53	0.63	0.42	0.30	0.38	0.81	0.85
Cronbach's alpha/test–retest reliability [a]	.96	.96	.96	.95	.95	.95	.89	.90	.97	.95	.97	.97	.94	.94	.99	.91 [b]	.76
McDonald's omega	.96	.95	.97	.96	.96	.95	.87	.90	.97	.94	.98	.97	.93	.93	.99	-	-

Note. The highest correlations (row-wise) are in bold. Correlations of related intelligence subtests are highlighted in dark gray, and correlations of subtests within the domains intelligence and scholastic skills are highlighted in light gray. All correlations are significant with $p < .001$. IDS-2 = Intelligence and Development Scales–2.
[a] Reliability coefficients were computed analogous to the IDS-2 manual (Grob & Hagmann-von Arx, 2018b). [b] Based on test–retest reliability (Grob & Hagmann-von Arx, 2018b).

APPENDIX B: Study 2

Grieder, S., Timmerman, M. E., Visser, L., Ruiter, S. A. J., & Grob, A. (2021). *Factor structure of the Intelligence and Development Scales–2: Measurement invariance across the Dutch and German versions, sex, and age.* Manuscript submitted for publication. PsyArXiv Preprint. https://doi.org/10.31234/osf.io/vtw3g

Factor Structure of the Intelligence and Development Scales–2: Measurement Invariance Across the Dutch and German Versions, Sex, and Age

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Abstract

We examined the factor structure of the intelligence and basic skills domains of the German and Dutch versions of an international test battery with 13 representative national standardizations (among others, Italian, Polish, U.K.)—the Intelligence and Development Scales–2 (IDS-2)—with confirmatory factor analyses (CFA) of the standardization samples. This included measurement invariance analyses across the Dutch and German versions and sex using multiple-group CFA, and across age using local structural equation modeling (LSEM). We tested several a priori theoretically (mostly following the Cattell–Horn–Carroll and verbal–perceptual–image rotation models) and empirically (with EFA) determined models and found a second-order model with six first-order factors best represented the Dutch IDS-2 structure. Five IDS-2 factors were confirmed, but Visual Processing and Abstract Reasoning and the intelligence and basic skills domains were not separable. This final model displayed full invariance across the Dutch and German versions and partial scalar invariance across sex, and it was largely invariant across ages 7 to 20 years. Thus, scores derived according to this final model will be comparable across these language versions, sex, and age. The strong general intelligence factor and weak broad ability factors suggest clinical interpretation should mainly be based on the full-scale IQ. We discuss the importance of testing multiple plausible models and adhering to a strict model selection procedure in CFA and implications for intelligence theory and clinical practice.

Keywords: Intelligence and Development Scales–2, intelligence, structural validity, confirmatory factor analysis, measurement invariance, local structural equation modeling, age, sex, language

Introduction

Validation of a test is crucial to determine its usefulness and ability to measure the intended constructs. If the interpretation of the test's scores is based on a hypothesized internal structure, evidence concerning this structure should be provided as part of the validation process, for example, using factor analysis (American Educational Research Association et al., 2014). Especially for new tests, it is useful to consider both data- and theory-driven approaches to determine a test's factor structure. A reasonable strategy for structural validation of a test is first to conduct an EFA and then to test the factor structure found in the EFA against theorized alternative models using CFA on a new sample (e.g., Brown, 2015; Reise, 2012). Once the most useful factor structure has been determined with a large, representative sample, measurement invariance should be tested across subgroups differing in important demographic characteristics such as native language, ethnicity, sex, and age. If this is demonstrated, the factor structure and thus the score interpretation will be consistent across these different groups. In the present study, we adopted the procedure outlined above for the structural validation of a new test—the Intelligence and Development Scales–2 (IDS-2; Grob & Hagmann-von Arx, 2018).

Topical Intelligence Theories

One of the most popular intelligence models to date is the Cattell–Horn–Carroll (CHC) model (McGrew, 1997; Schneider & McGrew, 2018). The CHC model is an integration of Cattell and Horn's fluid–crystallized model (Cattell, 1941; Horn, 1991) and Carroll's (1993) three-stratum model. It postulates a higher-order intelligence structure with over 80 narrow abilities on Stratum I and, in its most recent version (Schneider & McGrew, 2018), at least 14 broad abilities on Stratum II (see Table S1). Although general intelligence is most often included on Stratum III (consistent with Carroll's model), the CHC authors themselves are skeptical about it being a useful construct (Schneider & McGrew, 2018).

The CHC model has been widely used as a comprehensive framework in intelligence research and test construction, but it has also faced criticism. Especially its de-emphasis of general intelligence in favor of broad abilities has been criticized, given extensive evidence for the dominance of general intelligence and often small incremental validity of broad abilities (e.g., Canivez & Youngstrom, 2019; Cucina & Howardson, 2017). Further criticism concerns insufficient tests of plausible alternative models in CHC research with either EFA or CFA (Canivez & Youngstrom, 2019).

The CHC model is not without alternatives. In fact, the verbal–perceptual–image rotation (VPR) model, proposed by Johnson and Bouchard (2005), has been shown to outperform Cattell–Horn's and Carroll's models (Johnson et al., 2007; Johnson & Bouchard, 2005) as well as the CHC model (Major et al., 2012). The VPR model is an adaptation and extension of one of the first hierarchical models introduced by Vernon (1950). It features a four-stratum structure with numerous specific factors on Stratum I, several minor group factors on Stratum II, three major group factors—verbal, perceptual, and image rotation—on Stratum III, and a general intelligence factor on Stratum IV. Johnson and

Bouchard (2005) argued that the categorization of human intelligence into verbal, perceptual, and image rotation is theoretically superior to the categorization into fluid and crystallized, and the research cited above suggests that the VPR model might be empirically superior to other popular intelligence models as well. The present study aimed to test and compare multiple theory-driven intelligence models—in particular ones based on the VPR and CHC models—for the structural validation of the IDS-2.

The IDS-2

The IDS-2 (Grob & Hagmann-von Arx, 2018) assesses cognitive and developmental functions in 5- to 20-year-olds. The original form of the IDS-2 is the German version. Based on this version, international adaptations and standardizations of 12 additional language versions—namely, Brazilian, Danish, Dutch, Finnish, French, Italian, Norwegian, Polish, Swedish, Spanish, U.K., and U.S.—have been or are being developed. One of these versions for which the standardization is completed is the Dutch IDS-2 (Grob et al., 2018). Results of the present study are based on both the German and Dutch IDS-2 versions.

For the intelligence and basic skills domains, the IDS-2 largely complies with the CHC taxonomy (Schneider & McGrew, 2018; see Figure 1 for a representation of the IDS-2 model). The intelligence domain includes 14 subtests that enable the assessment of seven broad abilities largely corresponding to the CHC broad abilities (e.g., Visual Processing [VP] and Processing Speed [PS]; see Table S1). Deviations from current CHC broad abilities include the separation of short-term memory into an auditory and a visual–spatial component (narrow abilities of Working Memory Capacity [Gwm] in the CHC model), and the combination of long-term storage and retrieval (corresponding to the former CHC broad ability of the same name [Glr]). Furthermore, general intelligence is measured with a full-scale IQ consisting of all 14 intelligence subtests. The basic skills subtests Language Skills (LS), Logical–Mathematical Reasoning (LMR), Reading (RE), and Spelling (SPE) can also be integrated in the CHC framework: LS partly (but not exclusively) measures Auditory Processing (Ga), LMR contains aspects of Fluid Reasoning (Gf) and Quantitative Knowledge (Gq), and RE and SPE measure Reading and Writing (Grw). The reason these subtests were not included in the intelligence domain is their dependence on prior knowledge (Bodovski & Farkas, 2007; Tarchi, 2010). In the present study, we focused on all 14 intelligence subtests and the three basic skills subtests LMR, RE, and SPE. We excluded LS, because it predominantly measures language skills and not Ga.

Factor Structure

Structural validation is crucial to determine the appropriateness of test score interpretations that are based on a hypothesized internal structure. The structural validity evidence included in the IDS-2 manuals is based on CFAs with the standardization samples and is restricted to the intelligence domain (Grob et al., 2018; Grob & Hagmann-von Arx, 2018). For both the German and the Dutch IDS-2 version, the theoretical model—with all 14 subtests on Stratum I, seven broad abilities on Stratum II, and general intelligence on Stratum III (see the IDS-2 model in Figure 1, without basic skills)—displayed good fit (comparative fit index [CFI] = .97 and .98, and root mean square error of

approximation [RMSEA] = .04 and .04 for the German and Dutch IDS-2, respectively). Still, further evidence for structural validity is needed for at least two reasons. First, no alternative models (including bifactor models; Reise, 2012) were examined, neither with EFAs nor with CFAs; and second, no structural validity evidence was provided for the IDS-2 domains other than the intelligence domain. To address some of these shortcomings, Grieder and Grob (2020) examined the factor structures for the IDS-2 intelligence domain as well as for the intelligence and basic skills domains combined with EFA, using the German standardization and validation sample. Results supported the theoretical IDS-2 structure for intelligence, with the exceptions of VP and Abstract Reasoning (AR) collapsing to one factor named Abstract Visual Reasoning (AVR), and Long-Term Memory (LM) and Verbal Reasoning (VR) collapsing to one factor named Semantic Long-Term Memory (SLM), resulting in a five-factor model. Adding the three basic skills subtests LMR, RE, and SPE resulted in a six-factor structure with an additional factor Reading and Writing (RW), and with LMR showing a substantial loading on AVR and the highest general intelligence factor loading in a Schmid–Leiman solution (see EFA-Based Model 1 in Figures 1 and S1). A model with separate LM and VR factors, but with a cross-loading of Story Recall (SR) on VR, was also plausible, but it showed Heywood cases and was therefore discarded (see EFA-Based Model 2 in Figures 1 and S1). The explained variances and McDonald's omegas (McDonald, 1985, 1999) based on a Schmid–Leiman solution revealed a strong general intelligence factor and weak broad ability factors. To conclude, the EFA results partly supported the IDS-2 model. A separation of intelligence and basic skills domains did not hold empirically, because there was a strong general intelligence factor encompassing all subtests. In the present study, we tested this EFA-based factor structure, as well as the second plausible model that displayed Heywood cases in Grieder and Grob (2020), against the IDS-2 model, and against plausible alternative models using CFA with a new sample (i.e., the Dutch standardization sample).

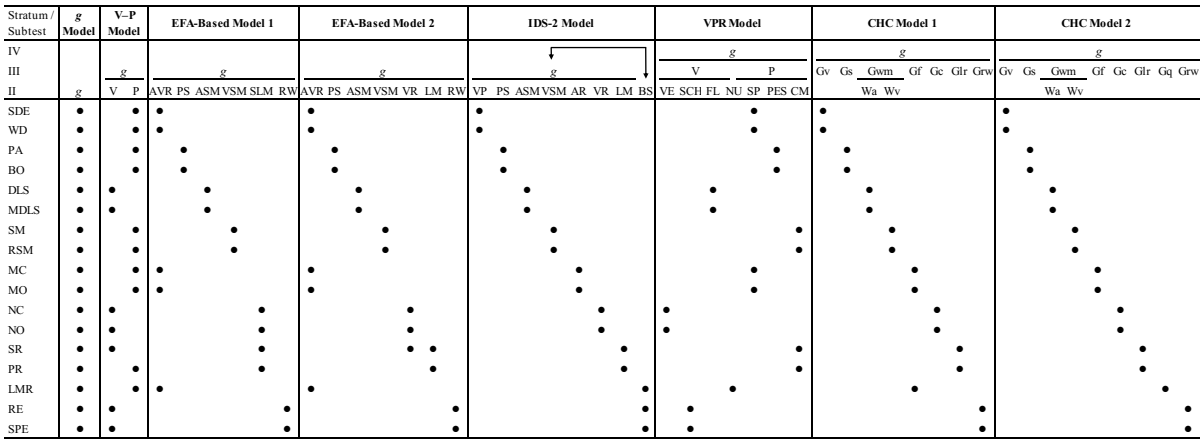
Measurement Invariance

After determining the most plausible factor structure, measurement invariance of this structure across subgroups differing in important demographic characteristics should be investigated to ensure test scores can be interpreted equally across these groups. Invariance analyses are usually performed with multiple-group CFAs (MGCFAs; e.g., Steenkamp & Baumgartner, 1998), where a series of models with parameters increasingly constrained to be equal across groups is fitted. Invariance testing usually starts with configural invariance (i.e., the same factor model is fitted without equality constraints) and continues with metric invariance (i.e., invariance of loadings), scalar invariance (i.e., additional invariance of intercepts), and strict invariance (i.e., additional invariance of residuals).

For the IDS-2, no evidence exists as yet for invariance across the different language versions, and we examined measurement invariance of the intelligence and basic skills domains across the Dutch and German IDS-2 using MGCFAs. In contrast, there is evidence for measurement invariance across sex and age for the IDS-2 intelligence domain. Results from MGCFAs reported in the manuals supported full invariance for the Dutch IDS-2 and partial scalar invariance for the German IDS-2 across

sex, and full invariance across the age groups of 5–8 years, 9–12 years, and 13–20 years for both the German and the Dutch IDS-2 for the theoretical IDS-2 model (Grob et al., 2018; Grob & Hagmann-von Arx, 2018). Nevertheless, it is still useful to test sex and age invariance on the final model in the present study, as this does not necessarily correspond to the theoretical IDS-2 model, and as it also contains the basic skills subtests, for which no sex and age invariance results exist as yet. We therefore investigated measurement invariance of the IDS-2 intelligence and basic skills domains across sex using MGCFAs. Regarding age invariance, most previous studies—as the ones reported in the IDS-2 manuals—used MGCFAs with defined age groups to investigate measurement invariance. Meanwhile, however, more sophisticated methods have become available that take the continuous nature of age explicitly into account. One such method is local structural equation modeling (LSEM; Hildebrandt et al., 2009, 2016), where each value of a predefined moderator variable is evaluated. As in nonparametric regression models, observations near a focal point—at which the model is evaluated—obtain higher weights, and more distant observations obtain lower weights. We therefore investigated age invariance of the IDS-2 intelligence and basic skills domains using LSEM in the present study.

Figure 1. *Higher-Order Models for the Intelligence and Basic Skills Domains*



Note. The double-headed arrow indicates a latent correlation. *g* = general intelligence; V–P = Verbal–Perceptual; EFA = exploratory factor analysis; IDS-2 = Intelligence and Development Scales–2; VPR = Verbal–Perceptual–Image Rotation; CHC = Cattell–Horn–Carroll; V = Verbal; P = Perceptual; Gv = Visual Processing; Gs = Cognitive Processing Speed; Gwm = Working Memory Capacity; Gf = Fluid Reasoning; Gc = Comprehension–Knowledge; Glr = Long-Term Storage and Retrieval; Grw = Reading and Writing; Gq = Quantitative Knowledge; AVR = Abstract Visual Reasoning; PS = Processing Speed; ASM = Auditory Short-Term Memory; VSM = Visual–Spatial Short-Term Memory; (S)LM = (Semantic) Long-Term Memory; RW = Reading and Writing; VR = Verbal Reasoning; VP = Visual Processing; AR = Abstract Reasoning; BS = Basic Skills; VE = Verbal; SCH = Scholastic; FL = Fluency; NU = Number; SP = Spatial; PES = Perceptual Speed; CM = Content Memory; Wa = Auditory Short-Term Storage; Wv = Visual–Spatial Short-Term Storage; SDE = Shape Design; WD = Washer Design; PA = Parrots; BO = Boxes; DLS = Digit and Letter Span; MDLS = Mixed Digit and Letter Span; SM = Shape Memory; RSM = Rotated Shape Memory; MC = Matrices: Completion; MO = Matrices: Odd One Out; NC = Naming Categories; NO = Naming Opposites; SR = Story Recall; PR = Picture Recall; LMR = Logical–Mathematical Reasoning; RE = Reading; SPE = Spelling.

Present Study

The present study had four goals: (1) to examine the factor structure of the intelligence and basic skills domains for the Dutch IDS-2, using its standardization sample; (2) to test measurement invariance of the resulting final model across the Dutch and German IDS-2 versions; (3) to test measurement invariance of this final model (or models, if configural invariance does not hold across language versions) across sex and age for the Dutch and the German IDS-2; and (4) to examine the relative importance of the general intelligence factor versus the broad ability factors across language versions, sex, and age using model-based reliability estimates.

The models we tested are based on topical intelligence theories and on EFA results from the German IDS-2 standardization and validation sample (Grieder & Grob, 2020). For all models, we tested higher-order (see Figure 1) as well as bifactor (see Figure S1) versions (see supplemental material for model descriptions). We examined the following research questions and hypotheses: (1) Which of several theoretically and empirically determined models best represents the structure of the intelligence and basic skills domains for the Dutch IDS-2? Given the results for the German IDS-2 (Grieder & Grob, 2020), we expected EFA-Based Model 1 to fit best for the Dutch IDS-2. (2) Is measurement invariance across the Dutch and German IDS-2 versions supported for the final model for the Dutch IDS-2? (3) Is measurement invariance across (a) sex (females and males[1]) and (b) age (7 to 20 years) supported for the final model(s) for the Dutch and German IDS-2? (4) Given the results from Grieder and Grob (2020), we expected the general intelligence factor to account for the largest share of true score variance for the final model.

For comparison with results reported in the IDS-2 manuals, we also performed all analyses on the intelligence domain alone, excluding the basic skills subtests. The methods, results, and a brief discussion of these analyses are included in the supplemental material. Together with all analysis scripts, this is available at https://osf.io/azep6/.

Method

Participants

Data from the standardization samples of the Dutch and German IDS-2 (N = 1,665 and N = 1,672, respectively) were used. The samples are described below.

Dutch Sample

For the Dutch IDS-2, 204 participants younger than 7 years of age had to be excluded due to the minimum age of 7 years required for conducting the RE and SPE subtests. Another 38 were excluded because more than 50% of the subtest scores were missing, resulting in a final sample of 1,423 participants who were all from the Netherlands. The mean age for this sample was 13.08 years (SD = 3.98, range: 7.00–21.89 years) and 52% were female. The Dutch standardization sample is

[1] We use the terms *females* and *males* to refer to persons self-identifying as girls and/or women and boys and/or men, respectively.

approximately representative for the Dutch population of children and adolescents aged 5 to 20 years in terms of sex, ethnic background of the parents, educational background of the mother, degree of urbanization, region, education type (for secondary school students), and common diagnoses according to information from Statistics Netherlands (2020) for 2015 to 2016.

German Sample

For the German IDS-2, 254 participants younger than 7 years of age had to be excluded. Another 13 participants were excluded because more than 50% of the subtest scores were missing. The final sample therefore consisted of 1,405 participants from Switzerland (n = 812), Germany (n = 517), and Austria (n = 76). The mean age for this sample was 13.12 years (SD = 3.90, range: 7.01–20.98 years) and 51% were female. The subsamples for Switzerland, Germany, and Austria are approximately representative for the Swiss, German, and Austrian population of children and adolescents aged 5 to 20 years in terms of sex, educational background of the mother, and education type according to information from the Swiss Federal Statistical Office (2020), the German Federal Statistical Office (2020), and Statistics Austria (2020) for 2015 to 2017, respectively.

Materials and Procedure

The IDS-2 is an individually administered test for 5- to 20-year-olds that enables the assessment of cognitive (intelligence, executive functions) and developmental (psychomotor skills, socioemotional competence, basic skills, and attitude toward work) functions with 30 subtests. The 14 subtests for intelligence and three subtests for basic skills are described in Table S1 (see Table S2 for descriptive statistics). For the German standardization, participants were recruited via schools and psychosocial institutions for children and adolescents in Switzerland, Germany, and Austria. The Dutch children were recruited via schools, organizations, personal contacts, and social media. Administration time was between 3.5 and 4.5 hr, which, if necessary, could be split into two sessions no more than 1 week apart. Children and adolescents (10- to 20-year-olds) and/or their parents (for 5- to 15-year-olds) provided written consent prior to testing. Parents or adolescents provided demographic information using a personally administered questionnaire at the beginning of the first session. Participants received a gift card of their own choice worth 30 Swiss francs (Switzerland) or 10 euros (The Netherlands), or 25 euros in cash (Germany, Austria) as reimbursement. Ethical approval was obtained from the Ethics Commission Northwest and Central Switzerland and from the responsible local ethics committees in the Netherlands.

Statistical Analyses

All analyses were conducted in R using age-standardized subtest scores (M = 10, SD = 3) as these are the scores used in practice and for the analyses reported in the IDS-2 manuals. CFAs were conducted with the lavaan package, version 0.60–7 (Rosseel, 2012) using the full information maximum likelihood method. McDonald's omegas were calculated with the EFAtools package, version 0.3.0 (Steiner & Grieder, 2020), and LSEMs were conducted with the Sirt package (Robitzsch, 2020).

Analysis Procedure

The analysis was done stepwise, as follows:

1. We tested the competing models listed in Figures 1 and S1 for the Dutch IDS-2, identified the best fitting model, and computed McDonald's omegas for this.
2. Following recommendations from Steenkamp and Baumgartner (1998), we first tested for equality of covariance matrices and mean vectors between the Dutch and German IDS-2 versions (Step 2a) using Box's *M* test (Box, 1949) and the procedure proposed by Nel and Van der Merwe (1986), respectively. If equality was not supported, we continued with an MGCFA, testing measurement invariance across the Dutch and German IDS-2 for the best fitting model identified in Step 1 (Step 2b). To this end, we followed the sequence of increasingly constrained models and model comparisons suggested in F. F. Chen et al. (2005; see supplemental material for details).
3. If configural invariance did hold, we fitted the final model from Step 1 for the German IDS-2 as well and again computed McDonald's omegas. Otherwise, we performed Step 1 also for the German IDS-2 to find the best fitting model for the German IDS-2 and proceeded from there.
4. We then tested sex invariance for the final model(s) separately for the two IDS-2 versions, adopting the same two-step process as for the invariance analyses across language versions. First, we tested for equality of covariance matrices and mean vectors (Step 4a) and then, if these were not equal, we tested for invariance of the model parameters across sex (Step 4b).
5. As a last step, we examined age invariance for the final model(s) separately for the two IDS-2 versions using LSEM (Hildebrandt et al., 2009, 2016; see supplemental materials for details).

Model Identification

The models were standardized by fixing the variances of all latent variables to unity. For factors with only one indicator, we fixed the single indicator's residual variance to $(1 - \alpha) * SD^2$ (e.g., Brown, 2015, p. 139), where α is the indicator's reliability (estimated as Cronbach's alpha) and *SD* is the indicator's standard deviation in the respective sample. For bifactor models, loadings of factors with only two indicators were constrained to be equal to be able to fit the model. Note that for models where all broad ability factors have only two indicators, the bifactor version is mathematically equivalent to the higher-order version.

Fit Indices and Model Selection

We considered CFI ≥ .95, RMSEA ≤ .06, and standardized root mean square residual (SRMR) ≤ .08 as indices of good overall fit (Hu & Bentler, 1999). We deemed fit to be acceptable if the SRMR and at least one of the two other fit indices met the cutoff criteria (Hu & Bentler, 1998, 1999). In addition, a model had to be free of local fit problems, such as nonsignificant or near-unity factor loadings, inflated standard errors, or negative variances.

If more than one model displayed acceptable fit, model comparisons were conducted between the well-fitting models. We first identified the best fitting model with the lowest Akaike information criterion (AIC) and more complex models with higher AICs were discarded from the start. This model was then compared to more parsimonious models using the following delta fit criteria and cutoffs suggested by F. F. Chen (2007): ΔCFI < -.01, ΔRMSEA < .015, and ΔSRMR < .03. If a more parsimonious model displayed no substantial worsening in fit according to all these criteria, the more complex model was discarded. For the MGCFAs, we used the same criteria, except of using ΔSRMR < .01 for invariance of intercepts and residuals (see Chen, 2007). The χ^2 and $\Delta\chi^2$ values were not interpreted because of their sensitivity for large sample sizes (Barrett, 2007).

Model-Based Reliability Estimates

McDonald's omegas (McDonald, 1985, 1999) are model-based reliability estimates that allow partitioning a composite's true score variance into variance explained by the general factor (omega hierarchical, ω_h), and variance explained by the subscale factors (omega subscale, ω_s). The total true score variance can be estimated with omega total (ω_t). There are no universal benchmarks for omega, but a preliminary suggestion has been made for a minimum of .50, with .75 being preferred, for ω_h for the full scale as well as for ω_s for the subscales (Reise et al., 2013). We calculated each of these coefficients (ω_t, ω_h, and ω_s) for the general intelligence composite as well as for all broad ability composites for the final models. We did this either directly, for a bifactor model, or from a Schmid–Leiman transformed higher-order model (Schmid & Leiman, 1957).

Results

Step 1: Model Selection for the Dutch IDS-2

In the first step, the models for the intelligence and basic skills domains introduced in Figures 1 and S1 were fitted for the Dutch IDS-2. The fit for all tested models is displayed in Table 1. Multiple models displayed good fit, and some models were discarded owing to unacceptable local or global fit (see Table S3 for local fit problems). Of the remaining models, we first identified the bifactor version of the EFA-Based Model 2 as the best fitting model according to the AIC. However, the more parsimonious higher-order version—the EFA-Based Model 2—did not show substantially worse fit, and the bifactor version was therefore discarded. The simpler EFA-Based Model 1 showed substantially worse fit than the EFA-Based Model 2 and was therefore discarded as well. The other more parsimonious models all displayed unacceptable global or local fit and were therefore discarded, too. Thus, the EFA-Based Model 2 was selected as the final model for the Dutch IDS-2 (see Figure 2a).

Table 1. *Fit Indices and Model Comparisons for the Intelligence and Basic Skills Domains for the Dutch IDS-2*

Model/Comparison	(Δ)χ^2	(Δ)*df*	(Δ)CFI	(Δ)RMSEA [90% CI]	(Δ)SRMR	(Δ)AIC
1. VPR bifactor [a]	333.02***	99	.970	.041 [.036, .046]	.027	111,364.23
2. V–P bifactor [b]	656.63***	102	.928	.062 [.057, .066]	.038	111,681.85
3. EFA 2 bifactor	401.60***	106	.962	.044 [.040, .049]	.033	111,418.81
4. EFA 1 bifactor	479.99***	106	.952	.050 [.045, .054]	.037	111,497.20
5. CHC 1 bifactor [a]	466.88***	108	.953	.048 [.044, .053]	.035	111,480.09
6. CHC 2	469.12***	110	.953	.048 [.043, .052]	.035	111,478.33
7. CHC 2 bifactor	469.42***	110	.953	.048 [.044, .052]	.035	111,478.63
8. CHC 1	485.26***	110	.951	.049 [.045, .053]	.036	111,494.47
9. VPR	519.43***	110	.947	.051 [.047, .056]	.036	111,528.64
10. EFA 2	**445.63*** **	**111**	**.957**	**.046 [.042, .051]**	**.034**	**111,452.84**
11. IDS-2 / IDS-2 bifactor [a]	496.21***	111	.950	.049 [.045, .054]	.036	111,503.42
12. EFA 1	551.63***	113	.943	.052 [.048, .057]	.038	111,554.84
13. V–P [b]	1429.29***	117	.830	.089 [.085, .093]	.051	112,424.50
14. *g* [b]	1728.78***	119	.791	.098 [.093, .102]	.059	112,719.99
10 versus 3	44.03***	5	-.0051	.0018	.0011	34.03
12 versus 10	106.00 [c]	2	-.0135	.0062	.0033	102.00

Note. N = 1,423. Fit indices and degrees of freedom are displayed for Models 1 to 14, and delta fit indices and change in degrees of freedom for model comparisons (last two rows). The thresholds for good global fit are SRMR ≤ .08, together with CFI ≥ .95 or RMSEA ≤ .06. The cutoffs for the delta fit criteria are ΔCFI < -.01, ΔRMSEA < .015, and ΔSRMR < .03. The final adopted model is in bold; models for which either the global or the local fit was unacceptable are in gray. VPR = Verbal–Perceptual–Image Rotation; EFA = exploratory factor analysis; IDS-2 = Intelligence and Development Scales–2; CHC = Cattell–Horn–Carroll; V–P = Verbal–Perceptual; *g* = general intelligence; CFI = comparative fit index; RMSEA = root mean square error of approximation; CI = confidence interval; SRMR = standardized root mean square residual; AIC = Akaike information criterion.
[a] Local fit not acceptable (see Table S3). [b] Global fit not acceptable. [c] Nonnested models, no χ^2-difference test performed.
*** $p < .001$.

Steps 2 and 3: Invariance Analyses Across the Dutch and German IDS-2 Versions

To test for measurement invariance across the Dutch and German IDS-2 versions, we first tested for the equivalence of covariance matrices and mean vectors. We found that neither the covariance matrices, Box's M test: $\chi^2(153) = 269.94$, $p < .001$, nor the mean vectors, Nel and Van der Merwe's test: $F(17, 2808) = 4.06$, $p < .001$, were equivalent. Therefore, we performed MGCFAs with the final model identified in Step 1. These revealed full invariance of all tested model parameters (i.e., first- and second-order loadings, intercepts, first-order factor means, residuals, and first-order factor disturbances) across the Dutch and German IDS-2 versions (see Table 2). Therefore, we fitted the final model for the Dutch IDS-2 also on the German IDS-2 (see Figure 2b).

McDonald's omegas were highly similar across the two IDS-2 versions. While ω_h was above the threshold of .75 for the general intelligence composite, ω_s was below the threshold of .50 for all broad ability composites (see Figure 2).

Table 2. *Results From Invariance Analyses Across the Dutch and German IDS-2 Versions for the Final Model for the Intelligence and Basic Skills Domains*

Model	χ^2	*df*	CFI	RMSEA [90% CI]	SRMR	Comp.	$\Delta\chi^2$	Δdf	ΔCFI	ΔRMSEA	ΔSRMR
A. Configural	913.16***	222	.957	.047 [.044, .050]	.035						
B1. First-order loadings	940.97***	232	.956	.046 [.043, .050]	.038	vs. A	27.81**	10	-.0011	-.0004	.0024
B2. Second-order loadings	960.07***	239	.955	.046 [.043, .049]	.041	vs. B1	19.09**	7	-.0008	-.0003	.0029
C1. Intercepts	989.80***	249	.954	.046 [.044, .049]	.041	vs. B2	29.73***	10	-.0012	-.0003	.0004
C2. First-order factor means	1023.55***	256	.952	.046 [.043, .049]	.042	vs. C1	33.75***	7	-.0017	.0002	.0007
D1. Residuals	1097.85***	273	.948	.047 [.044, .049]	.043	vs. C2	74.30***	17	-.0036	.0002	.0009
D2. First-order factor disturbances	1106.01***	281	.948	.046 [.043, .048]	.043	vs. D1	8.17	8	-.0000	-.0007	.0004

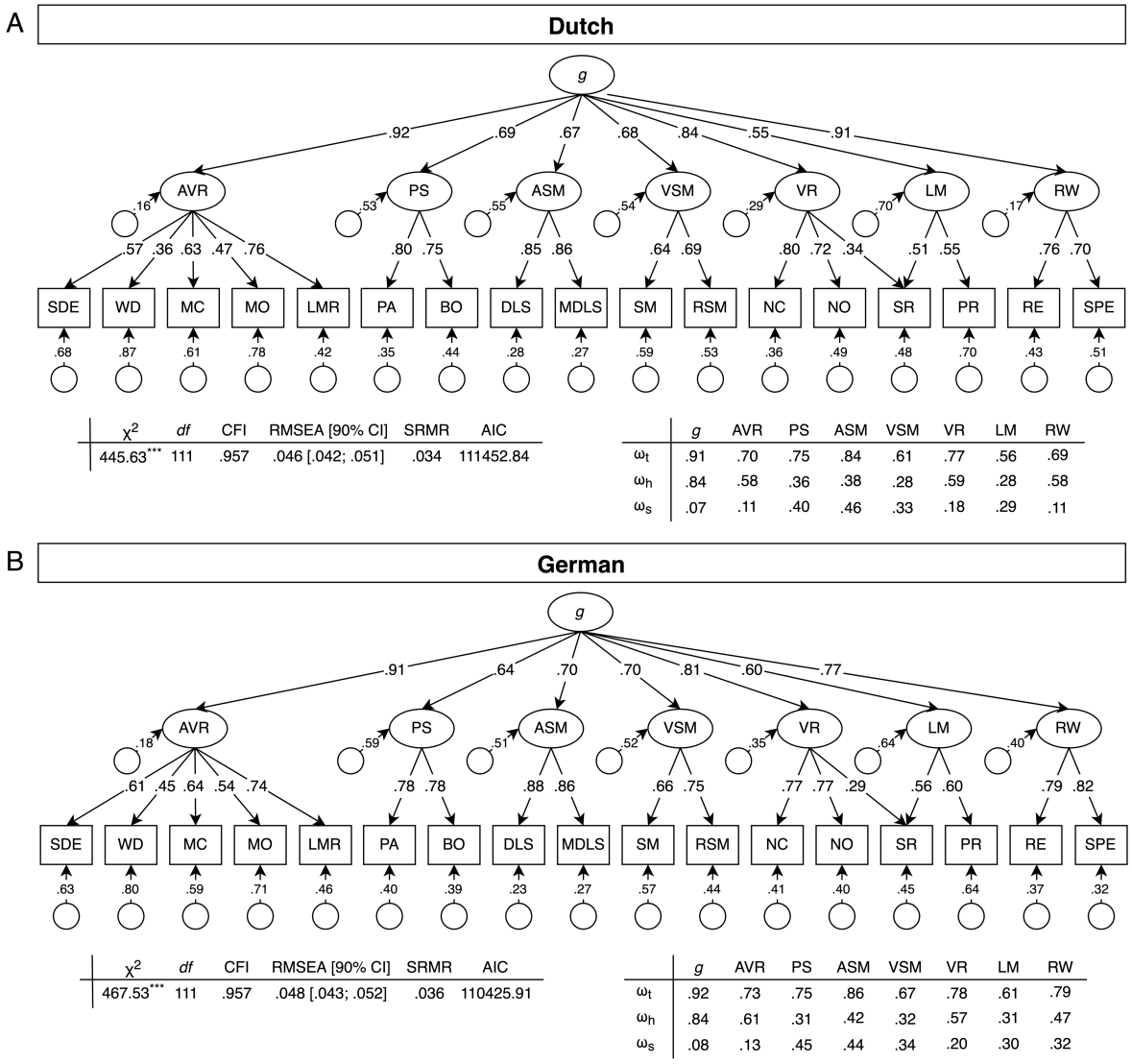
Note. N = 2,828. Results from multiple-group confirmatory factor analyses testing increasingly constrained models are displayed. The models are as follows: (A) Configural invariance; (B1) and (B2) metric invariance of first-order and second-order loadings; (C1) and (C2) scalar invariance at the manifest and first-order factor level; (D1) and (D2) strict invariance at the manifest and first-order factor level. The thresholds for good global fit are SRMR ≤ .08, together with CFI ≥ .95 or RMSEA ≤ .06. The cutoffs for the delta fit criteria are ΔCFI < -.01, ΔRMSEA < .015, and ΔSRMR < .01 for invariance of intercepts and residuals, or ΔSRMR < .03 for all other forms of invariance. CFI = comparative fit index; RMSEA = root mean square error of approximation; CI = confidence interval; SRMR = standardized root mean square residual; Comp. = model comparison; IDS-2 = Intelligence and Development Scales–2.
**p < .01. ***p < .001.

Step 4: Sex Invariance

For both the Dutch and the German IDS-2, we tested for measurement invariance across sex. We found no equivalence of covariance matrices and mean vectors, neither for the Dutch nor for the German IDS-2, Box's M test: $\chi^2(153) = 242.54$, $p < .001$ and $\chi^2(153) = 250.20$, $p < .001$, respectively; Nel and Van der Merwe's test: $F(17, 1389) = 11.29$, $p < .001$ and $F(17, 1364) = 17.62$, $p < .001$, respectively. We therefore conducted MGCFAs with the final model to further investigate sex invariance.

The first- and second-order loadings were invariant across sex for both IDS-2 versions, and thus metric invariance was supported (see Table 3). However, only partial scalar invariance was supported. For the Dutch IDS-2, the intercepts of PR and SDE had to be freed to achieve a nonsubstantial worsening in fit. Females scored higher in PR (estimated intercepts 10.93 vs. 9.77), and males scored higher in SDE (estimated intercepts 10.54 vs. 9.79). For the German IDS-2, the intercepts of PR and SPE had to be freed, with females scoring higher in both subtests (estimated intercepts 11.04 vs. 9.72 and 10.74 vs. 9.50, respectively). The first-order factor means were again invariant for both IDS-2 versions. Finally, based on the partial scalar invariance, strict invariance was supported for both IDS-2 versions, with residuals and first-order factor disturbances being invariant. Parameter estimates and McDonald's omegas for females and males based on the configural invariance model are displayed in Figure S2.

Figure 2. *Standardized Parameter Estimates, Model Fit, and McDonald's Omegas for the Final Model (EFA-Based Model 2) for the Intelligence and Basic Skills Domains of the Dutch (Panel A) and German (Panel B) Intelligence and Development Scales–2*



Note. N_{Dutch} = 1,423; N_{German} = 1,405. All parameters were significant with $p < .001$. *g* = general intelligence; AVR = Abstract Visual Reasoning; PS = Processing Speed; ASM = Auditory Short-Term Memory; VSM = Visual–Spatial Short-Term Memory; VR = Verbal Reasoning; LM = Long-Term Memory; RW = Reading and Writing; SDE = Shape Design; WD = Washer Design; MC = Matrices: Completion; MO = Matrices: Odd One Out; LMR = Logical–Mathematical Reasoning; PA = Parrots; BO = Boxes; DLS = Digit and Letter Span; MDLS = Mixed Digit and Letter Span; SM = Shape Memory; RSM = Rotated Shape Memory; NC = Naming Categories; NO = Naming Opposites; SR = Story Recall; PR = Picture Recall; RE = Reading; SPE = Spelling; CFI = comparative fit index; RMSEA = root mean square error of approximation; CI = confidence interval; SRMR = standardized root mean square residual; AIC = Akaike information criterion; ω_t = omega total; ω_h = omega hierarchical; ω_s = omega subscale.

Table 3. *Results From Sex Invariance Analyses for the Final Model for the Intelligence and Basic Skills Domains*

Model	χ^2	*df*	CFI	RMSEA [90% CI]	SRMR	Comp.	$\Delta\chi^2$	Δdf	ΔCFI	ΔRMSEA	ΔSRMR
Dutch											
A. Configural	559.47***	222	.956	.046 [.042, .051]	.038						
B1. First-order loadings	574.28***	232	.955	.046 [.041, .050]	.041	vs. A	14.80	10	-.0006	-.0007	.0029
B2. Second-order loadings	586.10***	239	.955	.045 [.041, .050]	.044	vs. B1	11.82	7	-.0006	-.0004	.0034
C1. Intercepts	687.69***	249	.943	.050 [.046, .054]	.047	vs. B2	101.59***	10	-.0119	.0046	.0031
C1.1. Partial [a]	659.93***	247	.946	.049 [.044, .053]	.046	vs. B2	73.83***	8	-.0086	.0033	.0021
C2. First-order factor means	682.57***	254	.944	.049 [.045, .053]	.048	vs. C1.1	22.64**	7	-.0020	.0002	.0015
D1. Residuals	727.01***	271	.941	.049 [.045, .053]	.049	vs. C2	44.44***	17	-.0036	-.0001	.0013
D2. First-order factor disturbances	773.76***	279	.935	.050 [.046, .054]	.074	vs. D1	46.75***	8	-.0051	.0013	.0250
German											
A. Configural	591.64***	222	.955	.049 [.044, .053]	.039						
B1. First-order loadings	599.52***	232	.956	.047 [.043, .052]	.040	vs. A	7.88	10	.0003	-.0012	.0009
B2. Second-order loadings	610.30***	239	.955	.047 [.042, .052]	.043	vs. B1	10.78	7	-.0005	-.0005	.0031
C1. Intercepts	764.26***	249	.938	.054 [.050, .059]	.048	vs. B2	153.96***	10	-.0174	.0072	.0045
C1.1. Partial [b]	663.73***	247	.946	.049 [.044, .053]	.047	vs. B2	53.43***	8	-.0095	.0019	.0035
C2. First-order factor means	695.77***	254	.942	.050 [.045, .054]	.048	vs. C1.1	32.04***	7	-.0033	.0007	.0012
D1. Residuals	740.78***	271	.939	.050 [.045, .054]	.049	vs. C2	45.01***	17	-.0037	-.0001	.0014
D2. First-order factor disturbances	787.94***	279	.934	.051 [.047, .055]	.074	vs. D1	47.16***	8	-.0051	.0013	.0250

Note. N_{Dutch} = 1,423; N_{German} = 1,405. Results from multiple-group confirmatory factor analyses testing increasingly constrained models are displayed. The models are as follows: (A) Configural invariance; (B1) and (B2) metric invariance of first-order and second-order loadings; (C1), (C1.1), and (C2) (partial) scalar invariance at the manifest and first-order factor level; (D1) and (D2) strict invariance at the manifest and first-order factor level. The thresholds for good global fit are SRMR ≤ .08, together with CFI ≥ .95 or RMSEA ≤ .06. The cutoffs for the delta fit criteria are ΔCFI < -.01, ΔRMSEA < .015, and ΔSRMR < .01 for invariance of intercepts and residuals or ΔSRMR < .03 for all other forms of invariance. CFI = comparative fit index; RMSEA = root mean square error of approximation; CI = confidence interval; SRMR = standardized root mean square residual; Comp. = model comparison.

[a] Intercepts for Picture Recall and Shape Design freed. [b] intercepts for Picture Recall and Spelling freed.

**$p < .01$. ***$p < .001$.

McDonald's omegas were similar across sex for both IDS-2 versions. We again found that ω_h was always above the threshold of .75 for the general intelligence composite, and ω_s was below the threshold of .50 for all broad ability composites for males for both IDS-2 versions, and for most of the broad ability composites for females as well. However, ω_s for Auditory Short-Term Memory (ASM) exceeded .50 for females for the German IDS-2 (.54), and was near this threshold for females for the Dutch IDS-2 (.49).

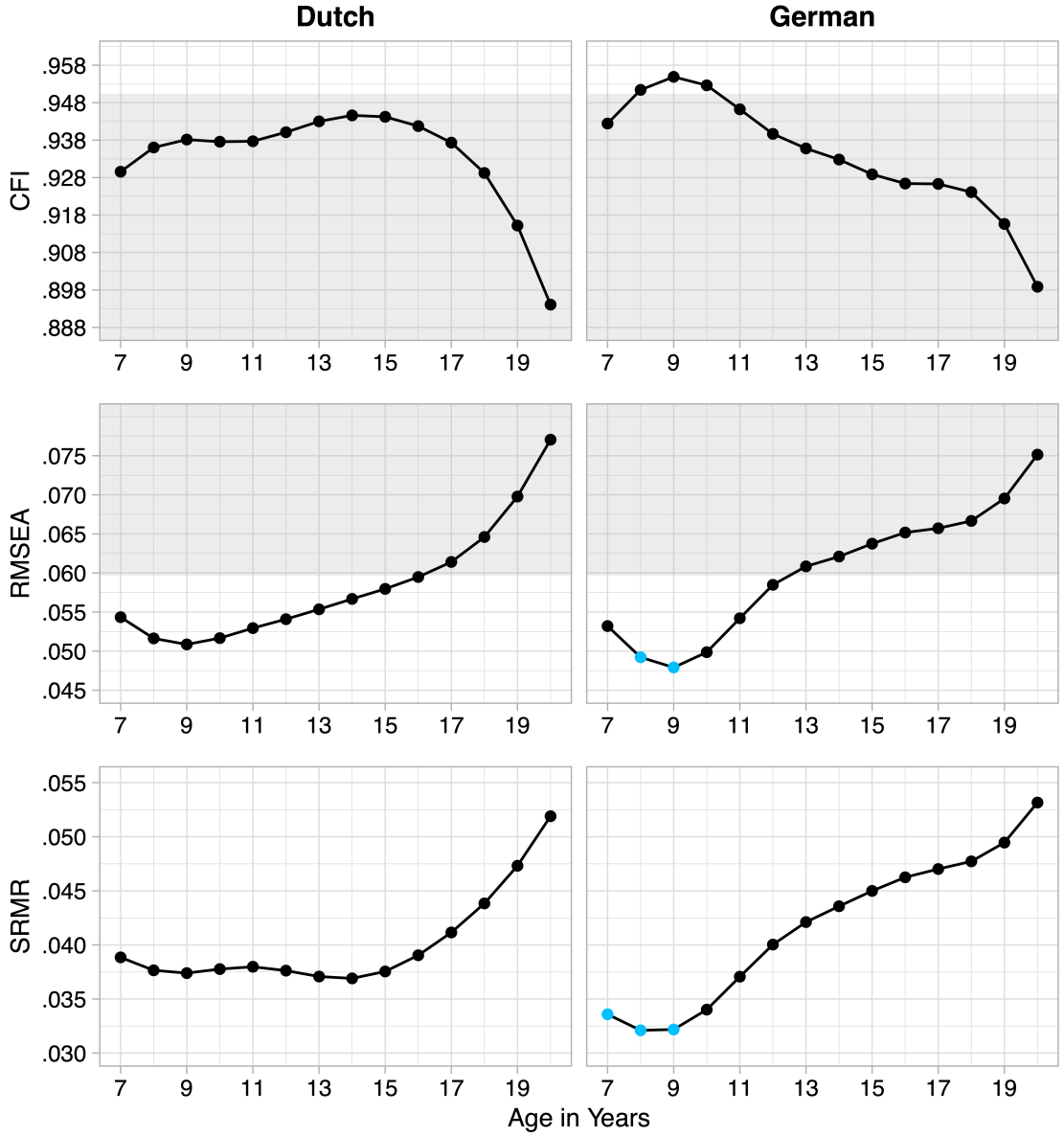
Step 5: Age Invariance

Finally, we investigated age invariance for model fit, first- and second-order loadings, and residuals from the final model for both the Dutch and the German IDS-2. For both IDS-2 versions, model fit in terms of CFI, RMSEA, and SRMR tended to become worse at older ages (see Figure 3). For the Dutch IDS-2, model fit was acceptable for ages 7 to 16 years, but no longer acceptable for ages 17 to 20 years. For the German IDS-2, fit was acceptable for ages 7 to 12 years, but no longer for ages 13 to 20 years.

For both IDS-2 versions, most first-order loadings were invariant, that is, there were no significant pointwise tests (see Figure 4). For the Dutch IDS-2, five of 18 first-order loadings had significant pointwise tests. The loading of Mixed Digit and Letter Span (MDLS) on ASM was significantly lower than the average from the permutated data sets at ages 7 to 10 years, and significantly higher than this average at ages 15 to 19 years. Moreover, there was a drop in the loading of Parrots on PS at ages 12 and 13 years and in the loading of Rotated Shape Memory on Visual–Spatial Short-Term Memory (VSM) at ages 7 and 8 years, while the loadings of Naming Categories (NC) and Naming Opposites on VR were significantly higher than average for ages 20 and 15 to 16 years, respectively. For the German IDS-2, significant pointwise tests occurred for only one of 18 first-order loadings. The loading of NC on VR was lower than average at age 7 years and higher than average at ages 17 and 18 years. The second-order loadings were completely invariant for both IDS-2 versions (see Figure 5). Finally, for residuals, results from pointwise tests displayed the same pattern of significance as described for the first-order loadings above, but with the opposite direction of effects (see Figure S3).

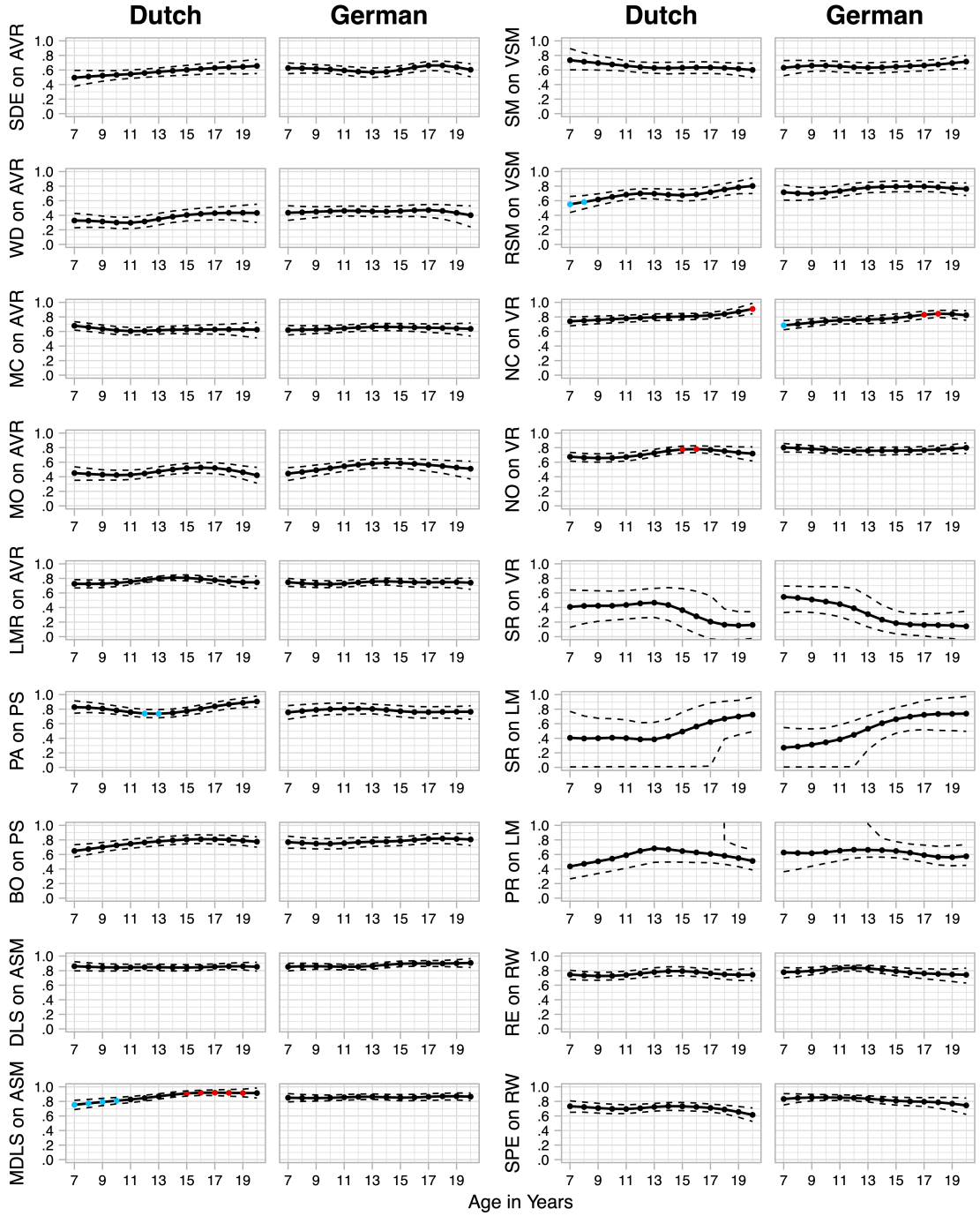
McDonald's omegas varied somewhat across age for both IDS-2 versions (see Figure S4), but ω_h was again above the threshold of .75 for the general intelligence composite, and ω_s was below the threshold of .50 for all broad ability composites for both IDS-2 versions at all ages, although ω_s for ASM was near .50 for ages 19 and 20 years for the Dutch IDS-2.

Figure 3. *Fit Measures for the Final Model for the Dutch and German Intelligence and Development Scales–2 (EFA-Based Model 2) for the Intelligence and Basic Skills Domains Across Age, Determined With Local Structural Equation Modeling*



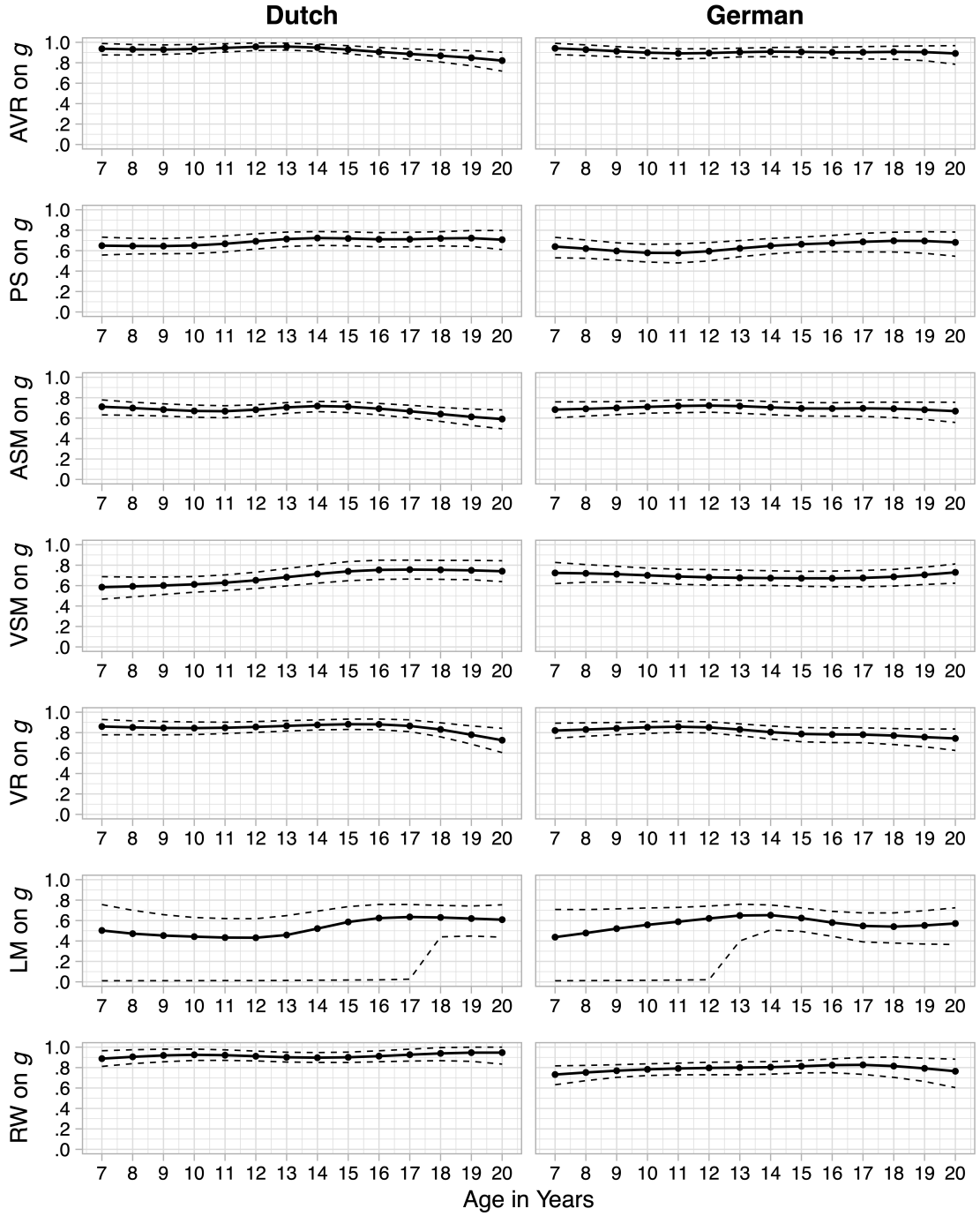
Note. Blue and black dots indicate significantly lower and not significantly different values, respectively, compared to the average from the permutated data sets. The gray-shaded areas indicate values beyond the thresholds of the displayed fit indices: CFI ≥ .95, RMSEA ≤ .06, and SRMR ≤ .08. CFI = comparative fit index; RMSEA = root mean square error of approximation; CI = confidence interval; SRMR = standardized root mean square residual.

Figure 4. *Standardized First-Order Factor Loadings for the Final Model for the Dutch and German Intelligence and Development Scales–2 (EFA-Based Model 2) for the Intelligence and Basic Skills Domains Across Age, Determined With Local Structural Equation Modeling*



Note. Red, blue, and black dots indicate significantly higher, lower, and not significantly different values, respectively, compared to the average from the permutated data sets. The dashed lines indicate the upper and lower levels of bootstrapped confidence intervals with 1,000 samples. SDE = Shape Design; AVR = Abstract Visual Reasoning; WD = Washer Design; MC = Matrices: Completion; MO = Matrices: Odd One Out; LMR = Logical–Mathematical Reasoning; PA = Parrots; PS = Processing Speed; BO = Boxes; DLS = Digit and Letter Span; ASM = Auditory Short-Term Memory; MDLS = Mixed Digit and Letter Span; SM = Shape Memory; VSM = Visual–Spatial Short-Term Memory; RSM = Rotated Shape Memory; NC = Naming Categories; VR = Verbal Reasoning; NO = Naming Opposites; SR = Story Recall; LM = Long-Term Memory; PR = Picture Recall; RE = Reading; RW = Reading and Writing; SPE = Spelling.

Figure 5. *Standardized Second-Order Loadings for the Final Model for the Dutch and German Intelligence and Development Scales–2 (EFA-Based Model 2) for the Intelligence and Basic Skills Domains Across Age, Determined With Local Structural Equation Modeling*



Note. Black dots indicate values that are not significantly different from the average from the permutated data sets. The dashed lines indicate the upper and lower levels of bootstrapped confidence intervals with 1,000 samples. *g* = general intelligence; AVR = Abstract Visual Reasoning; PS = Processing Speed; ASM = Auditory Short-Term Memory; VSM = Visual–Spatial Short-Term Memory; VR = Verbal Reasoning; LM = Long-Term Memory; RW = Reading and Writing.

Discussion

We examined the factor structure of the IDS-2 intelligence and basic skills domains with CFA. Applying a predefined model selection procedure, we found that, of several theoretically and empirically determined models, a model comparable to the one found with EFA for the German IDS-2 (Grieder & Grob, 2020) is the best representation of the structure of these two domains for the Dutch IDS-2. This model includes the five theoretical IDS-2 broad ability factors PS, ASM, VSM, VR, and LM, as well as the factors AVR (including the VP and AR subtests and LMR) and RW, and a second-order general intelligence factor. Full measurement invariance of this final model was established across the Dutch and German IDS-2 versions. For both the Dutch and German IDS-2, full metric invariance, partial scalar invariance, and—based on that—strict invariance was supported across sex. Across an age span of 7 to 20 years, most first-order loadings and residuals, and all second-order loadings, were invariant, but model fit was not acceptable for (older) adolescents. As expected, the general intelligence factor always accounted for the largest share of true score variance across the two IDS-2 versions, sex, and age, while the true score variance explained by the broad ability factors rarely exceeded the threshold of .50. The IDS-2 subtests are thus indicators of a strong general intelligence factor and weak broad ability factors.

Factor Structure

The final model for the IDS-2 intelligence and basic skills domains was the higher-order version of an empirically determined model suggested from a prior EFA study, but it was not, as we had expected, the final model from this study (Grieder & Grob, 2020). Although the bifactor version fitted better than the higher-order version, the fit of the more parsimonious higher-order version was not substantially worse, and the bifactor version was therefore discarded. This is in contrast to many other intelligence tests, where bifactor models clearly outperformed their higher-order counterparts (e.g., Cucina & Byle, 2017). One explanation for the small differences in fit between the bifactor and higher-order versions of most models tested here might be that, due to the availability of only two indicators for many factors, additional constraints were necessary for the bifactor models that rendered them more similar to the higher-order versions.

In line with results from Grieder and Grob (2020), the theoretical IDS-2 factors PS, ASM, VSM, VR, and LM were supported for the Dutch and German IDS-2 versions. The only difference between the final model from the present study and the final model from Grieder and Grob (2020) is that the theoretical factors VR and LM were separable in this study. However, in contrast to the theoretical IDS-2 model, SR showed a cross-loading on VR, suggesting that, besides long-term memory, this subtest also captures verbal abilities through the memorization and recall of exclusively verbal information.

In line with Baddeley's (2003) working memory model and empirical findings (Alloway et al., 2006), but in contrast to the CHC model, our results suggest that ASM and VSM can be separated at the broad ability level and not only at the narrow ability level (e.g., Schneider & McGrew, 2018). This

finding is supportive of the IDS-2 structure and might explain why visual memory tasks often do not load on a common factor with verbal memory tasks (Keith & Reynolds, 2010).

Our results provide further evidence against the separability of visual processing and fluid reasoning abilities, as the theoretical IDS-2 factors VP and AR—corresponding to the CHC broad abilities Visual Processing (Gv) and Gf—collapsed to one factor, AVR, in the present study. This is in line with results from Grieder and Grob (2020) and from previous studies on other intelligence tests (e.g., Canivez et al., 2017, 2021; Keith & Reynolds, 2010), and is in contrast to the CHC model. The collapse of these two factors may be due to the content of Gf subtests often being exclusively visual (e.g., matrices). However, in the rare cases where both Gv and Gf were measured with both verbal and nonverbal/visual content, the factors were also often not separable (e.g., DiStefano & Dombrowski, 2006). Thus, these two presumed abilities are (1) not measured validly in the aforementioned tests, and/or (2) so highly correlated that it is hard to measure them separately, or (3) in fact one ability. If (1) is true, then more valid tests of both abilities are needed, which requires clear, technical definitions of these abilities (Beaujean & Benson, 2019). If (2) or (3) are true, then it is not useful to create separate scores for Gv and Gf.

In line with the CHC model and with results from Grieder and Grob (2020), the basic skills domain (mathematical, reading, and writing skills) was not separable from the intelligence domain. In the theoretical IDS-2 model, the Basic Skills factor correlated to .97 with the general intelligence factor. The final model included an RW factor that had a high loading on the general intelligence factor, and LMR as the highest-loading subtest on AVR. The latter suggests that it is mainly reasoning abilities, as opposed to knowledge, that create between-subjects variance in LMR (cf. Wasserman, 2019). These findings draw into question the separation of intelligence and basic skills in the IDS-2. However, although general intelligence and general academic achievement have been found to be highly correlated also for other tests (e.g., Kaufman et al., 2012), there are still good reasons to create separate scores for the two, as influences of noncognitive variables and the learning history are more likely for academic achievement as opposed to intelligence scores on an individual level (Bodovski & Farkas, 2007; Kaufman et al., 2012; Tarchi, 2010).

Measurement Invariance

Invariance Across Language Versions

Full measurement invariance was supported for the final model across the Dutch and German IDS-2 versions. This means that the standardized scores that are used in practice are comparable between the Dutch and German populations and that the two IDS-2 versions measure the same constructs with comparable reliability for the intelligence and basic skills domains.

Sex Invariance

Full metric invariance was supported; hence, the same constructs are measured across sex. The fact that only partial scalar invariance was supported is in line with previous results on other tests (e.g., Irwing, 2012; Schweizer et al., 2018) and on the German IDS-2 (Grob & Hagmann-von Arx, 2018).

Females scored between 0.39 and 0.44 *SD* higher in a subtest measuring visual long-term memory (i.e., PR) for the Dutch and the German IDS-2, respectively, which is in line with previous evidence (e.g., Schweizer et al., 2018; see Born et al., 1987, for a meta-analysis), while males scored 0.25 *SD* higher in a subtest on visual–spatial abilities (i.e., SDE) for the Dutch IDS-2, also in line with previous evidence (see Born et al., 1987 and Voyer et al., 1995, for meta-analyses). The fact that females scored 0.41 *SD* higher in SPE for the German IDS-2 is also in line with previous findings on a female advantage in reading and writing skills (see Roivainen, 2011, for a review). Based on the partial invariance of intercepts, the invariance of first-order factor means, and strict invariance was also supported. This means that the constructs' reliabilities are also comparable across sex. The demonstration of at least partial measurement invariance for both IDS-2 versions implies that it is not necessary to split the sample into males and females when examining these IDS-2 domains, or to create separate norms for males and females (cf. Steenkamp & Baumgartner, 1998).

Age Invariance

Results from LSEM with the final model demonstrated invariance for most model parameters across age, with more invariant parameters for the German compared to the Dutch IDS-2. The parameters that were most variable across age were the loading of MDLS on ASM and the residual of MDLS for the Dutch IDS-2, with the former tending to increase and the latter tending to decrease up to age 16 years. Thus, MDLS becomes a better indicator of ASM with increasing age, and its unique variance decreases. One reason for this could be the increasing reliability, and thus decreasing measurement error, of MDLS with age.

Moreover, model fit was not acceptable across the whole age range. While the model fitted well for children and younger adolescents, fit was no longer acceptable for older adolescents. Possible explanations for this finding include a change in the relationship between intelligence and basic skills with age, for example, due to a change in tasks or schooling, the decrease in the cross-loading of SR on VR with age, or ceiling effects at older ages. Further analyses ruled out these explanations, though, and modification indices did not provide any further insights into possible sources of misfit. However, a subsequent EFA indicated that the nonsalient loadings (i.e., loadings < .30) were larger at specific ages compared to for the whole sample. For example, 60% of the absolute nonsalient loadings were larger for 17- to 20-year-olds (sum of loadings = 6.54) compared to for the whole sample (sum of loadings = 4.87) for the German IDS-2. Setting these larger nonzero loadings to zero in the CFA might explain why model fit was worse at specific ages compared to for the whole sample.

The large bootstrapped confidence intervals for some parameters show that there may be determination problems at some points of the model, probably due to the few indicators per factor (often only two; Marsh et al., 1998). This is especially pronounced for the loadings of SR, PR, and LM, probably due to the cross-loading of SR. These determination problems are ameliorated if the cross-loading of SR on VR is omitted, but at the cost of worse model fit (see results for intelligence domain alone in the supplemental material).

In general, there was more noninvariance across age than across language versions and sex. However, the criteria for invariance we applied for LSEM results (i.e., the pointwise tests for all focal points for all parameters of a kind need to be nonsignificant) are much stricter than the criteria applied for MGCFA results (i.e., the drop in fit when constraining all parameters of a kind to be equal across groups needs to be nonsubstantial). We further discuss this issue below. Finally, the determination problems that occurred for some parameters and at some ages could probably be avoided if a larger number of strong indicators were included per factor (cf. Fabrigar et al., 1999; Marsh et al., 1998). In particular, this would help better distinguish LM and VR.

Despite these caveats, our results demonstrate that the identified factor structure was largely invariant across an age span of 7 to 20 years, and especially the full invariance of all second-order loadings suggests neither age differentiation nor age dedifferentiation (i.e., a decline or increase in general intelligence factor loadings over age, respectively; see, e.g., Tucker-Drob, 2009).

Relative Importance of General Intelligence Versus Broad Abilities

Model-based reliability analyses with McDonald's omegas revealed a strong general intelligence factor and weak broad ability factors for the IDS-2 intelligence and basic skills domains. This is in line with previous findings from Grieder and Grob (2020) and with findings on other major intelligence tests (e.g., Canivez et al., 2017, 2021; Cucina & Howardson, 2017). Although there was some variation in omegas across the Dutch and German IDS-2 versions, sex, and age, the main conclusions are the same: The true score variance due to general intelligence seems sufficient for interpretation of a full-scale IQ, while the little true score variance due to the broad abilities casts doubt on the justification of broad ability composites, with the possible exception of ASM, whose ω_s sometimes exceeded the threshold of .50.

Implications

Our results have implications for theory, future research, and practice. The fact that many different models displayed good global fit points to the importance of testing and comparing multiple plausible models for the structural validation of a test. In particular, it is useful to test both theoretically driven and empirically derived (e.g., with EFA) models and compare them based on a predefined model selection procedure. Testing a single or a few preferred models, or failing to follow a predetermined model selection strategy, bears the risk of confirmation bias and thus failure to find the most useful representation of a test's structure (e.g., MacCallum & Austin, 2000).

The present study provides further evidence for the difficult separability of Gf and Gv. The accumulating evidence against their separability challenges the CHC model and calls for further research clarifying their status in the structure of intelligence. For the IDS-2, this suggests that the scores for VP and AR should best be combined to one score for interpretation. Furthermore, the relationship between intelligence and academic achievement should be studied further, also with specific samples (e.g., with specific learning disabilities or with irregular learning history), to investigate if and for what subgroups common scores for indicators of the two could be valid.

Results from the invariance analyses suggest that scores derived according to the final model identified here will be comparable across language versions, sex, and age, but more indicators per factor would help get better defined factors at all ages, especially for VR and LM. Finally, model-based reliabilities indicate that carefulness is required when interpreting broad ability composites and intelligence profiles, and that individual interpretation should focus primarily on the full-scale IQ. This finding, which has now been replicated for many intelligence tests, also challenges the usefulness of the CHC model, with its emphasis on broad abilities, for the development of individually administered intelligence tests (cf. Beaujean & Benson, 2019). It needs to be determined if and under what circumstances broad ability composites and, consequently, profile analyses could still be useful.

Limitations

One limitation of our study is that the theory-driven models we tested were not comprehensive operationalizations of the underlying theories. This was mostly due to the limited number of indicators and not all abilities from all theories being represented in the IDS-2. For example, for the CHC models, Learning Efficiency (Gl) and Retrieval Fluency (Gr) were not separable, Gq was represented by only one indicator, and narrow ability factors were only possible for Gwm. Moreover, the inclusion of an image rotation factor for the VPR model was not possible. Our study should not be seen as a test of intelligence theories.

We chose to use age-standardized subtest scores for our analyses because these are the scores used in practice and for comparability to previous factor analyses on the IDS-2 (Grieder & Grob, 2020; Grob et al., 2018; Grob & Hagmann-von Arx, 2018). Hence, analyses of differential item functioning were not possible, nor analyses of mean-level differences across language versions and age. The study of differential item functioning across language versions and the application of LSEM for age using raw subtest scores are interesting prospects for future research. So are invariance analyses on other participant characteristics, such as language skills or intelligence level.

Finally, the criteria used to determine invariance were not the same for LSEM and MGCFA. They were much stricter for the former than for the latter. An alternative to the pointwise tests we used would have been a procedure developed by Hartung et al. (2018) that performs MGCFAs for all possible focal point combinations based on the LSEM results. However, this would have come with the downside of losing the more detailed information for each parameter at each focal point. Moreover, there are no studies yet to determine for how many of the focal point combinations noninvariance was acceptable for invariance across a continuous moderator to hold. The same is true for the number of significant pointwise tests. Simulation studies are needed to develop useful criteria and thresholds for invariance testing with LSEM.

Conclusion

The theoretically proposed structure for the intelligence and basic skills domains of the Dutch and the German IDS-2 was partly supported, as some factors collapsed and the two domains were not separable. The final model selected here was an empirically determined model suggested by prior EFA

of the German IDS-2. This final model was fully invariant across the Dutch and the German IDS-2, and largely invariant across sex and age, suggesting that IDS-2 scores corresponding to the factors in the final model are comparable across these populations. Finally, the IDS-2 subtests were indicators of a strong general intelligence factor and weak broad ability factors, suggesting that clinical interpretation should focus on the full-scale IQ. Our results demonstrate the importance of testing multiple plausible models and following a strict model selection procedure when examining structural validity with CFA, and they illustrate the prospects of up-to-date methods for invariance analyses across continuous variables.

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APPENDIX C: Study 3

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Construct Validity of the German Wechsler Intelligence Scale for Children–Fifth Edition: Exploratory and Confirmatory Factor Analyses of the 15 Primary and Secondary Subtests

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Author Note

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Abstract

The latent factor structure of the German Wechsler Intelligence Scale for Children–Fifth edition (German WISC-V) was examined using complementary hierarchical exploratory factor analyses (EFAs) with Schmid and Leiman transformation and confirmatory factor analyses (CFAs) for all reported models from the German WISC-V *Technical Manual* and rival bifactor models using the standardization sample (N = 1,087) correlation matrix of the 15 primary and secondary subtests. EFA results did not support a fifth factor (Fluid Reasoning). A four-factor model with the dominant general intelligence (g) factor resembling the WISC-IV was supported by EFA. CFA results indicated the best representation was a bifactor model with four group factors, complementing EFA results. Present EFA and CFA results replicated other independent assessments of standardization and clinical samples of the United States and international versions of the WISC-V and indicated primary, if not exclusive, interpretation of the Full Scale IQ as an estimate of g.

Keywords: German WISC-V, exploratory factor analysis, confirmatory factor analysis, bifactor model, hierarchical CFA, intelligence

Introduction

Worldwide popularity of Wechsler scales has resulted in numerous translations, adaptations, and norms for many different countries, languages, and cultures (Georgas et al., 2003; Oakland et al., 2016); and H. Chen et al. (2010) reported latent factor structure invariance of the Wechsler Intelligence Scale for Children–Fourth edition (WISC-IV) across cultures. The Wechsler Intelligence Scale for Children–Fifth edition (WISC-V; Wechsler, 2014a) is the most recent version and purports to measure five first-order factors (Verbal Comprehension [VC], Visual Spatial [VS], Fluid Reasoning [FR], Working Memory [WM], Processing Speed [PS]) and a higher-order general intelligence (*g*) factor. This is consistent with contemporary conceptualizations of intelligence influenced by Carroll, Cattell, and Horn (Carroll, 1993, 2003; Cattell & Horn, 1978; Horn, 1991; Horn & Blankson, 2005; Horn & Cattell, 1966), often referred to as the so-called Cattell–Horn–Carroll (CHC) theory (Schneider & McGrew, 2012, 2018), and was also influenced by neuropsychological constructs (Wechsler, 2014c). A major revision goal in constructing the WISC-V was to separate subtests from the former Perceptual Reasoning (PR) factor into distinct VS and FR factors for better match to CHC. Similar attempts were previously made with the WAIS-IV (Weiss et al., 2013a) *and* WISC-IV (Weiss et al., 2013b), but Canivez and Kush (2013) highlighted numerous psychometric problems with the proposed higher-order models that included five group factors in both the WAIS-IV and the WISC-IV. Among the problems noted by Canivez and Kush (2013) were selective reporting of extant literature, creating intermediary factors that make models appear statistically better, post hoc model modifications, neglecting rival bifactor models, and lack of disclosure of decomposed variance estimates. WISC-V adaptations and norms are available for Canada, Spain, France, the United Kingdom, and Germany; and a version for Japan is forthcoming.

Canivez and Watkins (2016) criticized the publisher's claimed supportive evidence for the preferred higher-order measurement model (Model 5e) presented in the U.S. WISC-V *Technical and Interpretive Manual* (Wechsler, 2014c) that included numerous methodological and statistical problems including failure to report results of exploratory factor analyses (EFAs), use of weighted least squares (WLS) estimation in confirmatory factor analyses (CFA) without explicit justification (Kline, 2016), failure to fully disclose details of CFA, abandoning parsimony of simple structure (Thurstone, 1947) by cross-loading Arithmetic [AR] on *three* group factors, empirical redundancy of FR and *g* due to the standardized path coefficient of 1.0 between *g* and the FR factor, no consideration or testing of rival bifactor models, omission of decomposed variance sources between the higher-order *g* and lower order group factors, and absence of model-based reliability/validity estimates for *g* (omega-hierarchical [ω_H]) and the lower-order group (omega-hierarchical subscale [ω_{HS}]) factors (Watkins, 2017). Furthermore, degrees of freedom often do not add up to what is expected based on freely estimated parameters of stated models that suggests undisclosed fixing parameters to not go beyond permissible bounds. These problems cast substantial doubt for the viability of the publisher preferred "confirmatory" model (Beaujean, 2016; Canivez & Watkins, 2016).

German WISC-V

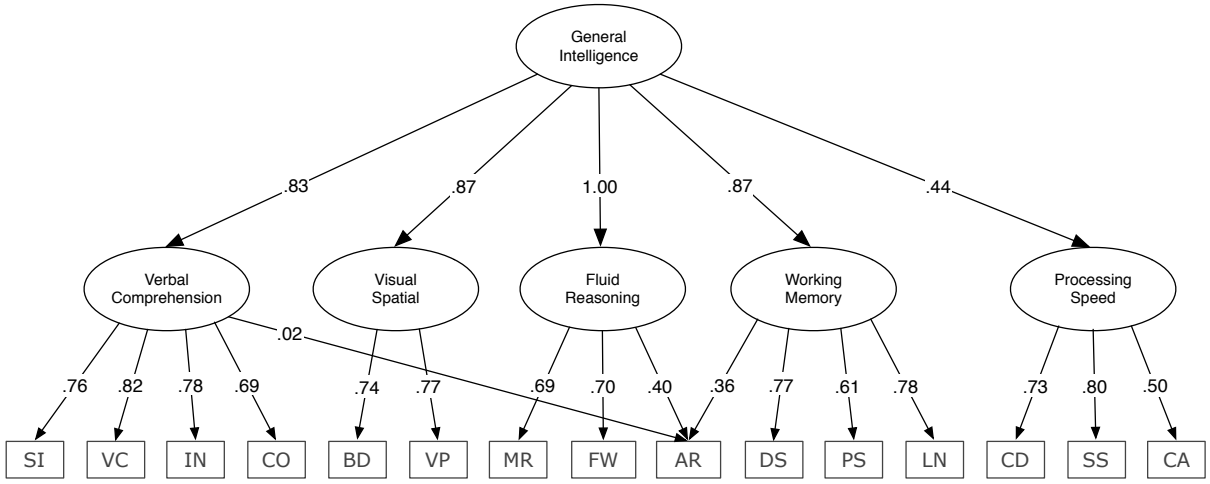
The German adaptation of the U.S. Wechsler Intelligence Scale for Children–Fifth edition (German WISC-V; Wechsler, 2017a), was reported to follow "contemporary intelligence theories, factor analytic studies, and clinical research" (Wechsler, 2017b, p. 15). However, while the U.S. WISC-V explicitly noted CHC theory (Wechsler, 2014c) the German WISC-V does not. Instead, a hierarchical two- or three-stratum intelligence structure (with or without narrow abilities) is assumed without references to specific intelligence theories or models and devoid of bifactor consideration. References to reviews in Flanagan and Harrison (2012) and Sattler (2008a, 2008b) are provided for the German WISC-V for different interpretation models because detailing descriptions of all intelligence theories was reported not to be within the scope of the chapter (Wechsler, 2014c). While a detailed U.S. WISC-V review (Canivez & Watkins, 2016) and several published independent analyses of the U.S. WISC-V were available prior to publication of the German WISC-V (Canivez et al., 2016; Canivez & Watkins, 2016; Dombrowski et al., 2015) none were referenced and it is unknown if they were reviewed or considered by the publisher in developing the German WISC-V. The German WISC-V includes all primary and secondary subtests from the U.S. version, except Picture Concepts, which also was not included in the versions for France and Spain (but *was* included in the Canadian and U.K. versions).

German WISC-V subtests are composed of items retained from the German WISC-IV (Petermann & Petermann, 2011), items adapted and modified from the U.S. WISC-V, and newly developed items. The German WISC-V *Technical Manual* does not provide a rationale for this mixture of kept, adapted, and newly developed items and there is no presentation of equivalence with subtests from the U.S. version. Specific guidelines or standards on which the adaptation and translation process was based were not provided; however, reference to standards applied for the standardization program of the U.S. version, namely the *Standards for Educational and Psychological Testing* (American Educational Research Association [AERA], American Psychological Association [APA], & National Council on Measurement in Education [NCME], 1999; Wechsler, 2017b, p. 57) was noted; although a more recent version of the *Standards* was published in 2014 (AERA, APA, & NCME, 2014). Psychometric properties and understanding of instructions were empirically tested in a pilot study prior to the standardization process, but only for the verbal subtests (Similarities [SI], Vocabulary [VC], Information [IN], Comprehension [CO], and AR). Internal consistency (split-half reliability) coefficients based on the standardization sample were high for all German WISC-V subtests and index scores (ranging from .80 for Cancellation [CA] to .96 for the Full Scale IQ [FSIQ]), and short-term test–retest reliabilities with a mean retest interval of 26 days (SD = 19, range: 7-116 days) for a subsample of 94 individuals ranged from .72 (Picture Span [PS]) to .90 (IN), with a stability coefficient of .89 for the FSIQ. Validity evidence reported in the German WISC-V *Technical Manual* includes factorial validity (described in detail below), convergent and discriminant validity, and distinct group differences validity. The subtests and indexes showed medium to strong relationships with corresponding subtests and indexes from the German adaptations of the WISC-IV, WPPSI-III

(Wechsler Preschool and Primary Scale of Intelligence–Third edition; Petermann et al., 2014), WAIS-IV (Petermann, 2012), and Kaufman Assessment Battery for Children–Second edition (KABC-II; Kaufman & Kaufman, 2015). Furthermore, a subsample of gifted individuals obtained significantly higher scores on all subtests, except for PS and some WM subtests, and had higher mean index scores compared with matched controls; while a subsample of intellectually disabled individuals obtained significantly lower scores on all subtests and had lower mean index scores and FSIQ compared with matched controls.

Figure 1 presents the publisher preferred structural measurement model for the German WISC-V as a basis for creation of standardized factor scores and interpretation. This is the identical model proffered for the United States, Canadian, the United Kingdom, French, and Spanish versions. The publisher claimed the German WISC-V "enables an estimation of general intelligence which is represented by five cognitive domains" (Wechsler, 2017b, p. 102). This five-factor model was preferred over the four-factor model based on reported better global fit, but like the U.S. WISC-V, this preferred model includes problems of the standardized path coefficient of 1.0 from the higher-order *g* factor to FR and includes three cross-loadings for the AR subtest (VC [.02], FR [.40], WM [.36]). Pauls et al. (2020) used multi-group confirmatory factor analysis (MGCFA) to examine the latent factor structure invariance of the publisher preferred German WISC-V measurement model across gender [sic] and reported configural, first-order and second-order metric invariance. Unlike the German WISC-V *Technical Manual*, Pauls et al. explicitly used maximum likelihood estimation with the reported normally distributed subtests scores. Oddly, the reported *df* for baseline models was one *higher* than would be expected based on the tested measurement model. Full scalar invariance was not supported, but partial scalar invariance showed subtest intercepts for IN, Figure Weights (FW), Coding (CD), and CA were not invariant across gender [sic], but invariance was observed for the other German WISC-V subtests. Error variances were also reported to be invariant. In contrast to the German WISC-V *Technical Manual*, Pauls et al. (2020) reported decomposed sources of variance in the German WISC-V according to a Schmid and Leiman (1957) orthogonalized higher-order model and found that while *g* had ample unique variance as reflected by high ω_H (.798) and construct replicability index (H = .896) values, the five group factors did not contain minimally acceptable unique variance. This led Pauls et al. (2020) to conclude primary interpretation of the FSIQ as an estimate of *g* and cautious interpretation of factor index scores, if at all.

Figure 1. *Publisher Preferred Higher-Order Measurement Model 5e With Standardized Coefficients (Adapted From Figure 5.1 [Wechsler, 2017b, p. 107]) for the German WISC-V Standardization Sample (*N *= 1,087)*



Note. SI = Similarities, VC = Vocabulary, IN = Information, CO = Comprehension, BD = Block Design, VP = Visual Puzzles, MR = Matrix Reasoning, FW = Figure Weights, AR = Arithmetic, DS = Digit Span, PS = Picture Span, LN = Letter-Number Sequencing, CD = Coding, SS = Symbol Search, CA = Cancellation.

German WISC-V Concerns

The same major concerns and shortcomings observed in the U.S. WISC-V reported by Canivez and Watkins (2016) were also observed in the German WISC-V *Technical Manual* (Wechsler, 2017b). EFA were not reported, opting for exclusive use of CFA despite the complementary nature of EFA and CFA (Brown, 2015; Gorsuch, 1983; Kline, 2016). Also, given the adaptations and modifications of subtests, creation of new item content, proposed change in factor structure (separation of VS and FR), and a new standardization sample; it cannot be assumed that the factor structure would be unchanged and thus appropriate and necessary to use EFA to inform plausible CFA models to test. The method of estimation was not disclosed nor was the method for setting scales. While maximum likelihood estimation would customarily be used with tests like the German WISC-V given continuous variables and reasonably normally distributed data, WLS estimation was used with other WISC-V versions (Wechsler, 2014b, 2014c, 2015a, 2015b, 2016a, 2016c) but without specific justification (see Canivez & Watkins, 2016). There was no report of the estimator used in German WISC-V CFA and model comparisons relied solely on the χ^2 difference despite reporting Akaike information criterion (AIC) and Bayesian information criterion (BIC) estimates. It was stated in the German WISC-V *Technical Manual* that nested models can be compared using the χ^2 difference test, but "when models are not nested, change in fit is assessed through subjective evaluation rather than statistical comparisons in model fit" (p. 105). More troubling is the continued observation that there are fewer degrees of freedom reported than expected, that is, they are not consistent with the number of freely estimated parameters suggested by specified models. This suggests that parameters may have been fixed (without disclosure) to allow model estimation by not allowing parameters to go beyond permissible bounds as was apparently done

with the U.S. WISC-V (Beaujean, 2016). This calls into question reported global model fit indexes as "supportive." Additionally, bifactor measurement models were apparently disregarded and variance estimates for contributions of first- and second-order factors remain absent. Bifactor models have several advantages: (a) direct influences of the general factor are more easily interpretable, (b) influences of both general *and* specific factors on indicators (subtests) are simultaneously examined, and (c) the psychometric properties necessary for determining scoring and interpretation of subscales (i.e., ω_H and ω_{HS} estimations) are directly examined (Canivez, 2016; Cucina & Byle, 2017; Reise, 2012). Gignac (2006) also noted that the direct hierarchical (i.e., bifactor) model can be considered more parsimonious because it specifies a unidimensional general factor. Furthermore, a major local fit problem—a standardized path coefficient of 1.0 between higher-order *g* and the FR group factor—was dismissed as a common finding in current studies on intelligence tests, without further discussion. Another German WISC-V local fit problem included the standardized path coefficient of .02 from VC to AR, which was not addressed at all in the manual. Finally, no statistical significances are reported in the German WISC-V *Technical Manual* for any of the parameters from the final model, thus hampering the examination of local fit problems.

WISC-V Research

Exploratory Factor Analyses

While EFAs were not reported in the WISC-V *Technical and Interpretive Manual* (Wechsler, 2014c), best practices (Watkins, 2018) applied in independent EFA of the U.S. WISC-V did not support the existence of five group factors in the total WISC-V standardization sample (Canivez et al., 2016) or in four age groups (6-8, 9-11, 12-14, and 15-16 years) within the WISC-V standardization sample (Canivez, Dombrowski, et al., 2018; Dombrowski, Canivez, et al., 2018), as the fifth extracted factor included only one salient subtest loading. Instead, a four-factor solution consistent with the WISC-IV was found to best represent the standardization data. Schmid and Leiman (1957) orthogonalization of the second-order EFA for the total U.S. WISC-V standardization sample and the four age groups found substantial portions of variance apportioned to *g* and substantially smaller portions of variance apportioned to the group factors (VC, PR, WM, and PS). ω_H coefficients for *g* (Reise, 2012; Rodriguez et al., 2016) ranged from .817 (Canivez et al., 2016) to .847 (Dombrowski, Canivez, et al., 2018) and exceeded the preferred level (.75) for clinical interpretation (Reise, 2012; Reise et al., 2013; Rodriguez et al., 2016). ω_{HS} coefficients for the four U.S. WISC-V group factors (Reise, 2012) ranged from .131 to .530, but no ω_{HS} coefficients for VC, PR, or WM approached or exceeded the minimum criterion (.50) for clinical interpretation (Reise, 2012; Reise et al., 2013; Rodriguez et al., 2016). ω_{HS} coefficients for PS, however, approached or exceeded the .50 criterion for possible clinical interpretation. Dombrowski et al. (2015) also failed to find support for five-factors in the total U.S. WISC-V standardization sample using exploratory bifactor analysis through the bifactor rotation criterion (Jennrich & Bentler, 2011). Furthermore, EFA did not support five group factors with a large U.S. clinical sample (Canivez, McGill, et al., 2018). Recent independent research with the French WISC-V

(Wechsler, 2016a) and the WISC-V U.K. edition (WISC-V^{UK}; Wechsler, 2016b) found identical EFA results supporting four first-order factors (not five), dominant general intelligence, and poor unique measurement of the four group factors (Canivez et al., 2019; Lecerf & Canivez, 2018).

Confirmatory Factor Analyses

Independent CFA conducted with the 16 U.S. WISC-V primary and secondary subtests (Canivez, Watkins, & Dombrowski, 2017) found all five of the higher-order models that included five first-order group factors (including the final U.S. WISC-V measurement model presented in the U.S. WISC-V *Technical and Interpretative Manual* as the preferred model) resulted in inadmissible solutions (i.e., negative variance estimates for the FR factor) potentially caused by misspecification of the models. A bifactor model that included five first-order factors produced an admissible solution and fit the standardization data well, but examination of local fit indicated problems where the Matrix Reasoning (MR), FW, and Picture Concepts subtests did not have statistically significant loadings on the FR group factor. The bifactor model with four group factors (VC, PR, WM, and PS) was selected as best based on the combination of statistical fit, local fit, and theory. This was consistent with previous EFA results (Canivez et al., 2016) showing a dominant general intelligence dimension and weak group factors with limited unique measurement beyond *g*. One study, however, (H. Chen et al., 2015) reported factorial invariance of the final publisher preferred WISC-V higher-order model with five group factors across gender [sic], although they did not examine invariance for alternative rival higher-order or bifactor models.

Reynolds and Keith (2017) suggested U.S. WISC-V invariance across age groups, but the model they examined for invariance was an oblique five-factor model, which ignores general intelligence altogether. Then they used CFA to explore numerous post hoc model modifications for first-order models with five-factors and then for both higher-order and bifactor models with five group factors in an attempt to better understand U.S. WISC-V measurement. While such explorations are possible, they may capitalize on chance and sample size. The final best fitting U.S. WISC-V higher-order model produced by Reynolds and Keith was different from the publisher preferred model in that AR was given a direct loading from *g and* a "cross-loading" on WM, and they also added correlated disturbances for the VS and FR group factors (.77) to represent an intermediate nonverbal general reasoning factor between the broad abilities and *g*. Yet the model still produced a standardized path coefficient of .97 from *g* to FR suggesting inadequate discriminant validity. Another concern was reliance on statistically significant χ^2 difference tests for model improvement despite the large sample and multiple comparisons but no meaningful changes in global fit. Researchers preferring higher-order Wechsler scale structures often introduce *post hoc* cross-loadings and correlated disturbance and error terms in altered CFA models; however, such procedures may capitalize on chance and sample size (MacCallum et al., 1992; Schreiber et al., 2006; Ullman, 2001) and it is rare when such parameters are specified *a priori*. Typically, previously unmodelled complexities are *post hoc* model adjustments iteratively added to improve model fit and/or remedy local fit problems, but specification of such

parameters may be problematic due to lack of conceptual grounding in previous theoretical work, may be unlikely to replicate, and increase the dangers of hypothesizing after results are known (HARKing) as noted by Cucina and Byle (2017). Preregistration would help address this potential problem. Furthermore, decomposed variance estimates of the Reynolds and Keith higher-order model showed the U.S. WISC-V subtests primarily reflected general intelligence variance with small portions of unique group factor variance (except for the PS subtests). Their best U.S. WISC-V bifactor model included a covariance estimate between VS and FR (.62), which appears necessary in order to salvage five group factors that EFA (Canivez et al., 2016) failed to locate. Watkins et al. (2018) also tested a similar bifactor model with the Canadian WISC-V (WISC-V^{CDN}), but this bifactor model with five group factors and VS–FR covariance estimate was *not* superior to the bifactor model with four group factors, so the Wechsler-based bifactor model with four group factors (VC, PR, WM, and PS) was preferred and Reynolds and Keith's findings failed replication. Independent research regarding the factor structure of international versions of the WISC-V replicated both EFA and CFA findings yielded by independent assessments of the U.S. WISC-V version (cf. Canivez et al., 2016; Canivez, Watkins, & Dombrowski, 2017), all failing to support five group factors (Canivez et al., 2019; Fenollar-Cortés & Watkins, 2019; Lecerf & Canivez, 2018; Watkins et al., 2018).

Higher-Order Versus Bifactor Models

Publisher references to Carroll's (1993) three stratum theory are provided in WISC-V technical manuals, but repeatedly fail to report EFA findings and decomposed variance estimates using the Schmid and Leiman transformation (SLT; Schmid & Leiman, 1957) which Carroll (1995) insisted on; or use more recently developed exploratory bifactor analysis (Jennrich & Bentler, 2011, 2012). SLT (sometimes referred to as an approximate bifactor solution; Reise, 2012) of EFA loadings apportion subtest variance to the first-order *and* higher-order dimensions because intelligence test subtests are influenced by both first-order factors *and* the higher-order *g* factor in a higher-order model. Interpretation of higher-order models requires this partitioning of variance in EFA, as well as CFA, so the relative influence of the first-order factors in comparison with the higher-order factor(s) may be determined. However, the SLT is just a reparameterization of the higher-order (second-order) model and may not be equivalent to a bifactor model (Beaujean, 2015b).

Higher-order representation of intelligence test structure is an *indirect* hierarchical model (Gignac, 2005, 2006, 2008) where the *g* factor influences subtests indirectly through full mediation through the first-order factors (Yung et al., 1999). The higher-order model conceptualizes *g* as a *superordinate* factor and an abstraction from abstractions (Thompson, 2004). While higher-order models have been commonly applied to assess intelligence test "construct-relevant psychometric multidimensionality" (Morin, Arens, & Marsh, 2016, p. 117), the bifactor model predated widespread use of higher-order models and was originally specified by Holzinger and Swineford (1937) and referred to as a direct hierarchical (Gignac, 2005, 2006, 2008) or nested factors model (Gustafsson & Balke, 1993). In bifactor models, *g* is conceptualized as a *breadth factor* (Gignac, 2008) because both

the *g* and the group factors directly and independently influence the subtest indicators. This means that both *g* and first-order group factors are at the same level of inference constituting a less complicated (more parsimonious) conceptual model (Cucina & Byle, 2017; Gignac, 2008). Carroll (1993) and his three stratum theory appear to reflect bifactor intelligence structure (Beaujean, 2015b) and there are major theoretical differences between higher-order and bifactor models. In the higher-order model, *g* is what the broad first-order factors have in common, whereas in the bifactor model, *g* is what is common among a diverse set of tasks or indicators which is how Spearman and Carroll thought of *g* (Beaujean, 2019). Cucina and Byle (2017) illustrated superiority of bifactor representations among a variety of cognitive tests and, given such results and the advantages of bifactor modeling for understanding test structure (Canivez, 2016; Cucina & Byle, 2017; Gignac, 2008; Reise, 2012), bifactor model comparisons should be routinely examined in addition to higher-order models for structural validation of cognitive tests.

Purpose

Understanding the structural validity of tests is crucial for evaluating interpretability of provided scores (AERA, APA, & NCME, 2014) and the German WISC-V *Technical Manual* lacks sufficient and necessary information regarding evidence of the German WISC-V structure to properly interpret test results. Numerous unanswered questions and incomplete information regarding the German WISC-V structure prohibits users of the German WISC-V to exercise good judgment about which scores have acceptable evidence of construct validity. Beaujean (2015a) indicated that a revised test should be treated like a new test as it cannot be assumed that scores from the revision would be directly comparable to the previous version without supporting evidence. Given the absence of EFA, questionable CFA methods identified in the German WISC-V *Technical Manual* (Wechsler, 2017b), and the lack of details regarding structural validity evidence, the present study (a) used best practices in EFA (Watkins, 2018) to examine the German WISC-V factor structure suggested by the 15 primary and secondary subtest relationships; (b) examined the German WISC-V factor structure using CFA with customary maximum likelihood estimation; (c) compared bifactor models with higher-order models as rival explanations; (d) decomposed factor variance sources in EFA and CFA; and (e) examined model-based reliability/validity (Watkins, 2017). Answers to these questions are essential for users of the German WISC-V to determine the interpretive value of the plethora of scores and score comparisons provided in the German WISC-V and interpretive guidelines promulgated by the publisher.

Methods

Participants

To conduct independent EFA and CFA with the German WISC-V, standardization sample raw data were requested from the publisher (NCS Pearson, Inc.) but access was denied without rationale. Absent raw data, the present analyses required use of summary statistics (correlations, means, and standard deviations) provided in the German WISC-V *Technical Manual* (Wechsler, 2017b, Table 5.1,

pp. 96-97). The published correlation matrix includes correlations rounded to only 2 decimals, but Carroll (1993) stated, "Little precision is lost by using two-decimal values" (p. 82). These correlations were reportedly produced by participants who were members of the full German WISC-V standardization sample (N = 1,087) who ranged in age from 6 to 16 years. The sample was stratified according to the key variables indicated by the Federal Statistical Office of the Federal Republic of Germany (2014): age, sex, migration background, parental education (age groups 6-9 years, four education levels), and children's school level (age groups 10-16 years, five school levels). Institutional review board review and approval of methods were obtained by the first author although no data were directly collected in this study.

Instrument

The German WISC-V (Wechsler, 2017a) is a general intelligence test composed of 15 subtests with scaled scores (M = 10, SD = 3). Like the United States and other versions there are 10 primary subtests (SI, VC, Block Design [BD], Visual Puzzles [VP], MR, FW, Digit Span [DS], PS, CD, Symbol Search [SS]) that are used for the measurement of five factor-based Primary Index scales: Verbal Comprehension Index (VCI), Visual Spatial Index (VSI), Fluid Reasoning Index (FRI), Working Memory Index (WMI), and Processing Speed Index (PSI). Seven of the primary subtests are used to produce the FSIQ. Ancillary index scales (pseudo-composites) are provided and include Quantitative Reasoning Index (QRI), Auditory Working Memory Index (AWMI), Nonverbal Index (NVI), General Ability Index (GAI), and Cognitive Proficiency Index (CPI), but are *not* factorially derived. Index scores and FSIQ are standard score (M = 100, SD = 15) metrics. Secondary subtests (IN, CO, AR, Letter–Number Sequencing [LN], CA) are used for substitution in FSIQ estimation when one subtest is spoiled or for use in estimating newly created (QR, AWM, and NV) or previously existing (General Ability and Cognitive Proficiency) Ancillary index scores. Ancillary index scores are not factorially derived composite scores, but logically or theoretically constructed. Picture Concepts, a subtest present in the U.S. WISC-V (and Canadian and U.K. versions) was not included in the German WISC-V.

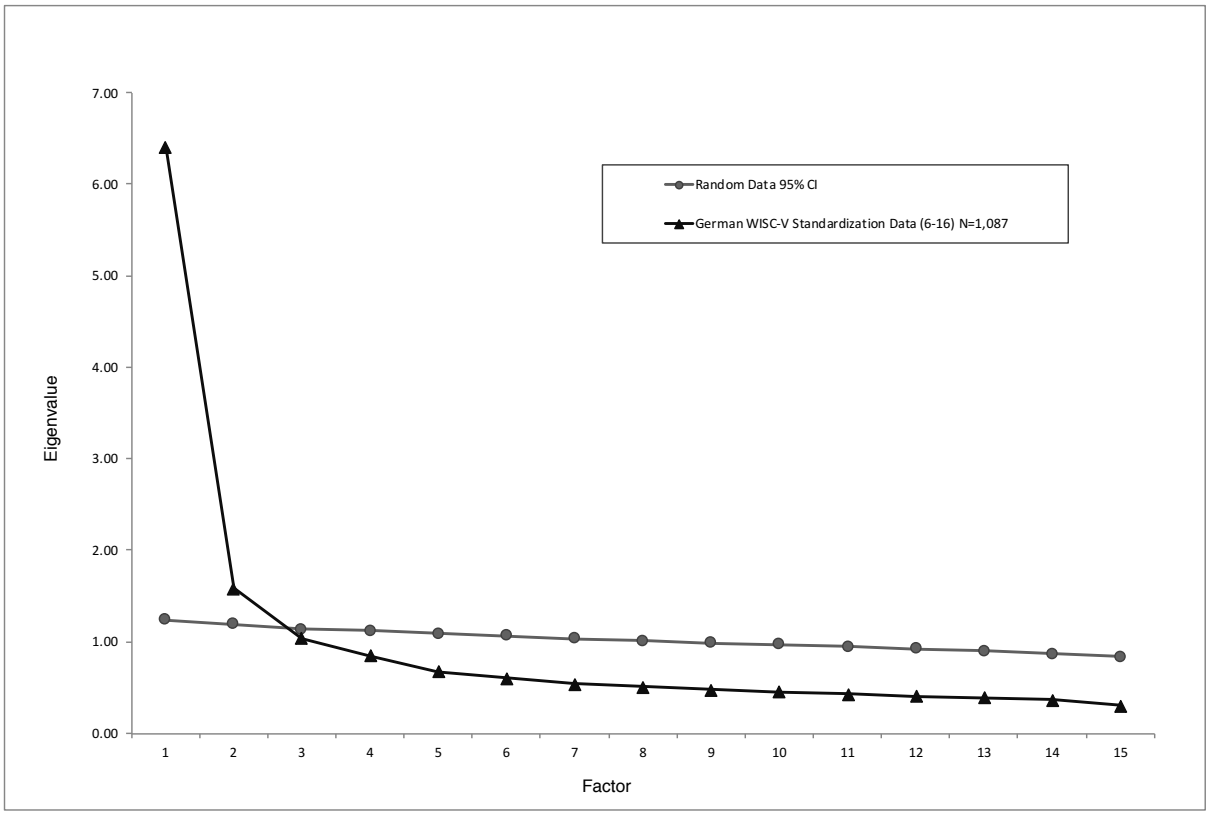
Analyses

Exploratory Factor Analyses

The 15 German WISC-V primary and secondary subtest correlation matrix was used to conduct EFAs. Several criteria were examined and compared for their recommendation of the number of factors that might be extracted and retained (Gorsuch, 1983) and included eigenvalues >1 (Kaiser, 1960), the scree test (Cattell, 1966), standard error of scree (SE_{scree}; Zoski & Jurs, 1996), parallel analysis (PA; Horn, 1965), Glorfeld's (1995) modified PA (see Figure 2), and minimum average partials (MAP; Frazier & Youngstrom, 2007; Velicer, 1976). Statistics were estimated with SPSS 24 for Macintosh or with specific software where noted. The Watkins (2007) SE_{scree} program was used as SE_{scree} reportedly is the most accurate objective scree method (Nasser et al., 2002). Random data and resulting eigenvalues for PA using both mean and 95% confidence intervals (Glorfeld, 1995) were produced using the O'Connor (2000) SPSS syntax with 100 replications to provide stable eigenvalue estimates. PA

frequently suggests retaining too few factors (underextraction) in the presence of a strong general factor (Crawford et al., 2010) so it was not the exclusive criterion. MAP was also conducted using the O'Connor (2000) SPSS syntax.

Figure 2. *Scree Plots for Horn's Parallel Analysis for the German WISC-V Standardization Sample (*N *= 1,087)*



Principal axis EFAs were conducted to analyze the factor structure of the German WISC-V using SPSS 24 for Macintosh. Retained factors were obliquely rotated with promax (k = 4; Gorsuch, 1983) and viable factors required a minimum of two subtests with salient factor pattern coefficients (≥ .30; Child, 2006). Because the German WISC-V explicitly adopted a higher-order structure, the SLT (Schmid & Leiman, 1957) procedure was applied to disentangle the contribution of 1st and 2nd order factors, as advocated by Carroll (1993) and Gignac (2005). The SLT has been used in numerous EFA studies with the WISC-IV (Watkins, 2006; Watkins et al., 2006), WISC-V (Canivez, Dombrowski, et al., 2018; Canivez et al., 2016; Dombrowski, Canivez, et al., 2018), RIAS (Dombrowski et al., 2009; Nelson et al., 2007), Wechsler Abbreviated Scale of Intelligence (WASI) and Wide Range Intelligence Test (WRIT; Canivez et al., 2009), SB5 (Canivez, 2008), WISC-IV Spanish (McGill & Canivez, 2016), French WAIS-III (Golay & Lecerf, 2011), French WISC-IV (Lecerf et al., 2010), French WISC-V (Lecerf & Canivez, 2018), and WISC-V^{UK} (Canivez et al., 2019). The SLT allows for deriving a hierarchical factor model from higher-order models and decomposes the variance of each subtest score

into general factor variance first and then first-order factor variance. The first-order factors are modeled orthogonally to each other and to the general factor (Gignac, 2006; Gorsuch, 1983). The SLT was produced using the *MacOrtho* program (Watkins, 2004). This procedure disentangles the common variance explained by the general factor and the residual common variance explained by the first-order factors.

Confirmatory Factor Analyses

CFA with maximum likelihood estimation was conducted using EQS 6.3 (Bentler & Wu, 2016). Covariance matrices were reproduced for CFA using the correlation matrix, means, and standard deviations obtained from the German WISC-V standardization sample. As with other similar studies (e.g., Canivez, Watkins, & Dombrowski, 2017; Watkins et al., 2018) identification of latent variable scales set a reference indicator to 1.0 in higher-order models and in bifactor models, by setting the variance of latent variables to 1.0 (Brown, 2015; Byrne, 2006). As with other versions of the WISC-V, the VS factor and FR factor are underidentified in some of the five-factor models because they are measured by only two subtests (BD and VP, MR, and FW). Thus, in specifying the VS factor and FR factor (in some five-factor models) in CFA bifactor models, the two subtests' path coefficients on their group factors were constrained to equality prior to estimation to ensure identification (Little et al., 1999).

The structural models specified in Table 5.2 of the German WISC-V *Technical Manual* (Wechsler, 2017b) were also examined in present CFA analyses and are reproduced in Table 1 and Table 2 with the addition of alternative bifactor models that were not included in analyses reported in the German WISC-V *Technical Manual*. Although there are no universally accepted cutoff values for approximate fit indices (McDonald, 2010), overall global model fit was evaluated using the comparative fit index (CFI), Tucker–Lewis index (TLI), standardized root mean squared residual (SRMR), and the root mean square error of approximation (RMSEA). Higher CFI and TLI values indicate better fit whereas lower SRMR and RMSEA values indicate better fit. Hu and Bentler (1999) combinatorial heuristics indicated adequate model fit with CFI and TLI $\geq .90$ along with SRMR $\leq .09$ and RMSEA $\leq .08$. Good model fit required CFI and TLI ≥ 0.95 with SRMR and RMSEA ≤ 0.06 (Hu & Bentler, 1999). Additionally, the AIC was considered. AIC does not have a meaningful scale; the model with the smallest AIC value is most likely to replicate (Kline, 2016) and would be preferred. Superior model fit required adequate to good overall fit *and* display of meaningfully better fit. Meaningful differences between well-fitting models were assessed using ΔCFI $> .01$ and ΔRMSEA $> .015$ (F. F. Chen, 2007; Cheung & Rensvold, 2002) and ΔAIC > 10 (Burnham & Anderson, 2004). In addition to assessing global fit, local fit assessment was conducted as models should never be retained "solely on global fit testing" (Kline, 2016, p. 461).

Table 1. *German WISC-V Primary and Secondary Subtest Configuration for CFA Models With 1–4 Factors*

	Model 1	Model 2		Model 3			Model 4a				Model 4a Bi-factor					Model 4b				Model 4c				Model 4d			
Subtest	*g*	F1	F2	F1	F2	F3	F1	F2	F3	F4	*g*	F1	F2	F3	F4	F1	F2	F3	F4	F1	F2	F3	F4	F1	F2	F3	F4
SI	●	●		●			●				●	●				●				●				●			
VC	●	●		●			●				●	●				●				●				●			
IN	●	●		●			●				●	●				●				●				●			
CO	●	●		●			●				●	●				●				●				●			
BD	●		●		●			●			●		●				●				●				●		
VP	●		●		●			●			●		●				●				●				●		
MR	●		●		●			●			●		●					●			●				●		
FW	●		●		●			●			●		●					●			●				●		
AR	●	●		●					●		●			●				●			●			●	●	●	
DS	●	●		●					●		●			●				●				●				●	
PS	●		●		●				●		●			●				●				●				●	
LN	●	●		●					●		●			●				●				●				●	
CD	●		●			●				●	●				●				●				●				●
SS	●		●			●				●	●				●				●				●				●
CA	●		●			●				●	●				●				●				●				●

Note. All models include a higher-order general factor except for the bifactor model. SI = Similarities; VC = Vocabulary; IN = Information; CO = Comprehension; BD = Block Design; VP = Visual Puzzles; MR = Matrix Reasoning; FW = Figure Weights; AR = Arithmetic; DS = Digit Span; PS = Picture Span; LN = Letter–Number Sequencing; CD = Coding; SS = Symbol Search; CA = Cancellation.

Table 2. *German WISC-V Primary and Secondary Subtest Configurations for CFA Models With 5 Factors*

	Model 5a					Model 5a Bifactor						Model 5b					Model 5c					Model 5d					Model 5e				
Subtest	F1	F2	F3	F4	F5	*g*	F1	F2	F3	F4	F5	F1	F2	F3	F4	F5	F1	F2	F3	F4	F5	F1	F2	F3	F4	F5	F1	F2	F3	F4	F5
SI	●					●	●					●					●					●					●				
VC	●					●	●					●					●					●					●				
IN	●					●	●					●					●					●					●				
CO	●					●	●					●					●					●					●				
BD		●				●		●					●					●					●					●			
VP		●				●		●					●					●					●					●			
MR			●			●			●					●					●					●					●		
FW			●			●			●					●					●					●					●		
AR				●		●				●			●						●	●		●			●		●		●		
DS				●		●				●					●					●					●				●		
PS				●		●				●					●					●					●				●		
LN				●		●				●					●					●					●				●		
CD					●	●					●					●					●					●				●
SS					●	●					●					●					●					●				●
CA					●	●					●					●					●					●				●

Note. All models include a higher-order general factor except for the bifactor model. SI = Similarities; VC = Vocabulary; IN = Information; CO = Comprehension; BD = Block Design; VP = Visual Puzzles; MR = Matrix Reasoning; FW = Figure Weights; AR = Arithmetic; DS = Digit Span; PS = Picture Span; LN = Letter–Number Sequencing; CD = Coding; SS = Symbol Search; CA = Cancellation.

Model-based reliabilities/validities were estimated with coefficients ω_H and ω_{HS}, which estimate reliability of unit-weighted scores produced by the indicators (Reise, 2012; Rodriguez et al., 2016). ω_H is the model-based reliability estimate for the general intelligence factor with variability of group factors removed. ω_{HS} is the model-based reliability estimate of a group factor with all other group and general factors removed (Brunner et al., 2012; Reise, 2012). Omega estimates (ω_H and ω_{HS}) may be obtained from CFA bifactor solutions or decomposed variance estimates from higher-order models and were produced using the *Omega* program (Watkins, 2013), which is based on the tutorial by Brunner et al. (2012) and the work of Zinbarg et al. (2005) and Zinbarg et al. (2006). Omega coefficients should at a minimum exceed .50, but .75 is preferred (Reise, 2012; Reise et al., 2013).

Omega coefficients were supplemented with the *H* coefficient (Hancock & Mueller, 2001), a construct reliability or construct replicability coefficient, and the correlation between a factor and an optimally weighted composite score. *H* represents how well the latent factor is represented by the indicators and a criterion value of .70 (Hancock & Mueller, 2001; Rodriguez et al., 2016) was used. *H* coefficients were produced by the *Omega* program (Watkins, 2013).

Results

Exploratory Factor Analyses

The Kaiser–Meyer–Olkin Measure of Sampling Adequacy of .931 far exceeded the .60 minimum standard (Kaiser, 1974; Tabachnick & Fidell, 2007) and Bartlett's Test of Sphericity (Bartlett, 1954), $\chi^2 = 6{,}987.57$, $p < .0001$; indicated that the German WISC-V correlation matrix was not random. The correlation matrix was thus deemed appropriate for factor analysis. Without standardization sample raw data, it was not possible to estimate univariate subtest skewness or kurtosis or multivariate normality, but principal axis extraction does not require normality. While univariate and multivariate skewness and kurtosis were not reported in the German WISC-V *Technical Manual* (Wechsler, 2017b), Pauls et al. (2020) reported reasonably normally distributed subtest scores for the 15 German WISC-V subtests within the two gender [sic] groups based on univariate estimates (Male sample skewness ranged −.39 to .12, kurtosis ranged −.35 to .60, Female sample skewness ranged −.34 to .09 and kurtosis ranged −.26 to .71); however, multivariate estimates were not provided.

Figure 2 illustrates the scree plots from Horn's parallel analysis for the German WISC-V total standardization sample. Scree, PA, and Glorfeld's modified PA criteria suggested two factors, while eigenvalues > 1 and SE_{scree} criteria suggested 3 factors. The MAP criterion suggested only one factor. In contrast, the German WISC-V publisher desired and claimed five latent factors. EFA began by extracting five factors to examine subtest associations based on the publisher's desired and promoted structure to allow examination of the performance of smaller factors because Wood et al. (1996) noted that it is better to overextract than underextract. Models with four, three, and two factors were subsequently examined for adequacy.

Results of a five-factor extraction with promax rotation presented in Table 3 include a fifth factor with only one salient factor pattern coefficient (SI). This extraction and rotation also produced Factor 1 (WM) that included salient pattern coefficients for theoretically related subtests (AR, DS, PS, LN) but also included salient pattern coefficients for MR and FW. Factor 2 (VC) included salient pattern coefficients for VC, IN, and CO. Factor 3 (VS [formerly PR]) included salient pattern coefficients for BD, VP, and MR. However, MR also cross-loaded on Factor 1 (WM) which indicated a lack of simple structure. Factor 4 (PS) included salient subtest pattern coefficients by the theoretically consistent subtests (CD, SS, and CA). Thus, MR and FW did not share sufficient common variance to constitute a FR dimension as specified by the publisher. This pattern of psychometrically unsatisfactory results is indicative of overextraction (Gorsuch, 1983; Wood et al., 1996) and the five-factor model was judged inadequate.

Table 3. *German Wechsler Intelligence Scale for Children–Fifth Edition (WISC-V) Exploratory Factor Analysis: Five Oblique Factor Solution for the Total Standardization Sample (*N *= 1,087)*

	General	F1: Working Memory		F2: Verbal Comprehension		F3: Visual Spatial		F4: Processing Speed		F5: Inadequate		
Subtest	*S*	*P*	*S*	*P*	*S*	*P*	*S*	*P*	*S*	*P*	*S*	h^2
SI	.749	.020	.611	.173	.708	.013	.633	-.004	.275	**.736**	.877	.787
VC	.741	-.030	.578	**.821**	.856	.026	.581	-.015	.276	.063	.630	.736
IN	.716	.056	.592	**.526**	.739	.186	.622	-.057	.239	.103	.614	.587
CO	.633	.069	.508	**.822**	.752	-.137	.434	.098	.327	-.084	.469	.586
BD	.645	-.009	.543	-.079	.462	**.713**	.732	.073	.325	.076	.514	.544
VP	.668	.018	.569	.022	.502	**.780**	.764	.013	.293	-.072	.481	.586
MR	.640	***.341***	.622	-.006	.471	**.455**	.651	-.036	.247	-.059	.448	.471
FW	.666	**.323**	.631	.158	.559	.277	.619	-.050	.239	.025	.512	.464
AR	.719	**.532**	.724	.065	.559	.157	.625	.045	.341	.022	.522	.546
DS	.688	**.832**	.777	-.067	.489	-.005	.562	-.037	.272	.013	.479	.607
PS	.579	**.507**	.601	.029	.447	.024	.476	.031	.270	.070	.436	.368
LN	.698	**.834**	.779	.073	.536	-.082	.538	-.006	.300	-.065	.458	.614
CD	.400	.123	.338	-.077	.243	-.097	.263	**.727**	.737	.080	.245	.553
SS	.438	-.014	.335	.001	.285	.085	.337	**.772**	.789	-.031	.236	.626
CA	.287	-.141	.183	.143	.235	.082	.221	**.509**	.516	-.054	.155	.280
Eigenvalue		6.41		1.58		1.04		.85		.67		
% Variance		39.91		7.37		4.33		2.71		1.37		

Factor Correlations	F1: WM	F2: VC	F3: PR	F4: PS	F5
Working Memory (WM)	–				
Verbal Comprehension (VC)	.676	–			
Perceptual Reasoning (PR)	.740	.658	–		
Processing Speed (PS)	.396	.336	.366	–	
F5	.633	.698	.670	.283	–

Note. General structure coefficients are based on the first unrotated factor coefficients (*g* loadings). Salient pattern coefficients (≥ .30) presented in bold. German WISC-V subtests: SI = Similarities; VC = Vocabulary; IN = Information; CO = Comprehension; BD = Block Design; VP = Visual Puzzles; MR = Matrix Reasoning; FW = Figure Weights; AR = Arithmetic; DS = Digit Span; PS = Picture Span; LN = Letter–Number Sequencing; CD = Coding; SS = Symbol Search; CA = Cancellation; *S* = structure coefficient; *P* = pattern coefficient; h^2 = communality.

Table 4 presents the results of extracting four factors with promax rotation. The *g* loadings (first unrotated factor structure coefficients) ranged from .287 (CA) to .744 (VC) and—except CD, SS, and CA—were within the fair to good range based on Kaufman's (1994) criteria (≥ .70 = good, .50-.69 = fair, < .50 = poor). Table 4 illustrates robust VC (SI, VC, IN, and CO) and PS (CD, SS, and CA) factors with theoretically consistent subtest associations. The WM factor included the four theoretically related subtests (AR, DS, PS, and LN) but also included salient pattern coefficients of MR and FW. The VS (formerly PR) factor included salient pattern coefficients of BD, VP, and MR, but FW did not have a salient loading on this factor. Overall the four-factor model resembled WISC-IV structure but was not a perfect match. MR had primary loading on the VS factor but cross-loaded on WM. The moderate to high factor correlations presented in Table 4 (.341 to .747) suggested the presence of a general intelligence factor (Gorsuch, 1983) requiring explication.

Table 4. *German Wechsler Intelligence Scale for Children–Fifth Edition (WISC-V) Exploratory Factor Analysis: Four Oblique Factor Solution for the Total Standardization Sample (*N *= 1,087)*

	General	F1: Working Memory		F2: Verbal Comprehension		F3: Visual Spatial		F4: Processing Speed		
Subtest	*S*	*P*	*S*	*P*	*S*	*P*	*S*	*P*	*S*	h^2
Similarities	.723	.090	.609	**.522**	.729	.215	.639	-.018	.273	.568
Vocabulary	.744	-.051	.575	**.919**	.868	-.018	.579	-.011	.276	.756
Information	.718	.041	.590	**.621**	.758	.184	.625	-.057	.238	.596
Comprehension	.630	.065	.506	**.778**	.725	-.196	.433	.108	.327	.547
Block Design	.648	-.029	.542	-.041	.495	**.768**	.742	.068	.322	.556
Visual Puzzles	.668	.017	.570	-.007	.523	**.738**	.752	.017	.290	.566
Matrix Reasoning	.641	***.332***	.622	-.034	.491	**.438**	.650	-.033	.245	.466
Figure Weights	.668	**.314**	.630	.186	.580	.276	.621	-.050	.237	.465
Arithmetic	.720	**.526**	.724	.085	.580	.161	.629	.045	.339	.547
Digit Span	.690	**.832**	.777	-.057	.512	.000	.568	-.038	.269	.607
Picture Span	.580	**.506**	.601	.074	.468	.043	.483	.029	.268	.367
Letter–Number Sequencing	.699	**.826**	.778	.045	.549	-.106	.541	-.002	.299	.609
Coding	.400	.126	.337	-.036	.254	-.060	.270	**.715**	.731	.540
Symbol Search	.439	-.014	.335	-.020	.289	.078	.337	**.776**	.792	.631
Cancellation	.287	-.141	.183	.114	.230	.058	.218	**.511**	.516	.277
Eigenvalue		6.41		1.58		1.04		.85		
% Variance		39.77		7.33		4.23		2.66		

Promax-Based Factor Correlations	F1: WM	F2: VC	F3: PR	F4: PS
F1: Working Memory (WM)	–			
F2: Verbal Comprehension (VC)	.700	–		
F3: Visual Spatial (VS)	.747	.695	–	
F4: Processing Speed (PS)	.392	.341	.364	–

Note. General structure coefficients are based on the first unrotated factor coefficients (*g* loadings). Salient pattern coefficients (≥ .30) presented in bold. Italic type denotes salient cross-loading. *S* = structure coefficient; *P* = pattern coefficient; h^2 = communality.

Table 5 presents results from the three- and two-factor extractions with promax rotation. For the three-factor model, the VS/PR and WM factors merged, leaving fairly distinct VC and PS factors. Oddly, SI cross-loaded on the VS/PR/WM factor. The two-factor model showed merging of VC, VS/PR, and WM factors, leaving only the separate PS factor. No subtest cross-loadings were observed in the two-factor model. The two- and three-factor models clearly displayed fusion of potentially theoretically meaningful constructs that is symptomatic of underextraction, thereby rendering them unsatisfactory (Gorsuch, 1983; Wood et al., 1996).

Table 5. *German Wechsler Intelligence Scale for Children–Fifth Edition (WISC-V) Exploratory Factor Analysis: Two and Three Oblique Factor Solutions for the Total Standardization Sample (*N *= 1,087)*

	Two Oblique Factors				Three Oblique Factors				
Subtest	*g* [a]	F1: *g*	F2: PS	h^2	*g* [a]	F1: PR/WM	F2: VC	F3: PS	h^2
SI	.725	**.751** (.735)	-.036 (.295)	.541	.725	**.304** (.667)	**.509** (.724)	-.018 (.288)	.566
VC	.722	**.748** (.732)	-.035 (.294)	.537	.748	-.035 (.620)	**.904** (.875)	-.009 (.289)	.766
IN	.716	**.765** (.731)	-.077 (.260)	.538	.720	.233 (.650)	**.604** (.755)	-.056 (.252)	.593
CO	.616	**.583** (.613)	.069 (.326)	.380	.629	-.051 (.515)	**.712** (.713)	.108 (.333)	.517
BD	.636	**.595** (.632)	.083 (.346)	.405	.634	**.584** (.644)	.041 (.491)	.071 (.335)	.420
VP	.658	**.643** (.659)	.037 (.320)	.435	.656	**.613** (.669)	.062 (.518)	.025 (.308)	.449
MR	.640	**.653** (.647)	-.012 (.275)	.419	.642	**.720** (.679)	-.036 (.477)	-.035 (.260)	.463
FW	.673	**.706** (.684)	-.049 (.262)	.470	.670	**.577** (.675)	.166 (.568)	-.053 (.251)	.470
AR	.723	**.697** (.722)	.057 (.364)	.524	.722	**.686** (.738)	.047 (.562)	.042 (.351)	.547
DS	.679	**.687** (.685)	-.005 (.298)	.469	.683	**.809** (.731)	-.086 (.492)	-.035 (.281)	.539
PS	.582	**.565** (.582)	.039 (.288)	.340	.580	**.557** (.594)	.035 (.451)	.026 (.277)	.355
LN	.689	**.683** (.693)	.021 (.322)	.480	.690	**.713** (.717)	.006 (.526)	.001 (.308)	.514
CD	.403	.004 (.322)	**.723** (.724)	.525	.400	.062 (.333)	-.047 (.245)	**.713** (.723)	.525
SS	.445	.001 (.355)	**.802** (.803)	.644	.441	.033 (.360)	-.015 (.282)	**.790** (.799)	.639
CA	.289	.010 (.233)	**.505** (.509)	.260	.288	-.098 (.212)	.121 (.229)	**.516** (.517)	.274
Eigenvalue		6.41	1.58			6.41	1.58	1.04	
% Variance		39.20	7.26			39.55	7.31	4.06	
Factor Correlations		F1	F2			F1	F2	F3	
	F1	–			F1	–			
	F2	.441	–		F2	.730	–		
					F3	.427	.346	–	

Note. Factor pattern coefficients (structure coefficients) based on principal factors extraction with promax rotation ($k = 4$). Salient pattern coefficients (≥ .30) presented in bold. German WISC-V subtests: SI = Similarities; VC = Vocabulary; IN = Information; CO = Comprehension; BD = Block Design; VP = Visual Puzzles; MR = Matrix Reasoning; FW = Figure Weights; AR = Arithmetic; DS = Digit Span; PS = Picture Span; LN = Letter–Number Sequencing; CD = Coding; SS = Symbol Search; CA = Cancellation; *g* = general intelligence; PS = Processing Speed; PR = Perceptual Reasoning; WM = Working Memory; VC = Verbal Comprehension; h^2 = communality.
[a] General structure coefficients based on first unrotated factor coefficients (*g* loadings).

Because the four-factor EFA solution appeared to be the most reasonable it was subsequently subjected to second-order EFA and results transformed with the SLT procedure (see Table 6). Following SLT, all German WISC-V subtests were properly associated with their theoretically proposed factors (Wechsler model), except for FW, which had residual variance approximately evenly split between the WM factor and VS factor. The hierarchical *g* factor accounted for 35.1% of the total variance and 65.1% of the common variance. The general factor also accounted for between 6.0% (CA) and 47.1% (AR) of individual subtest variability.

Table 6. *Sources of Variance in the German Wechsler Intelligence Scale for Children–Fifth Edition (WISC-V) for the Total Standardization Sample (*N *= 1,087) According to the Schmid–Leiman Orthogonalized Higher-Order EFA Model With Four First-Order Factors*

	General		F1: Working Memory		F2: Verbal Compre-hension		F3: Visual Spatial		F4: Processing Speed			
Subtest	*b*	S^2	*b*	S^2	*b*	S^2	*b*	S^2	*b*	S^2	h^2	u^2
Similarities	.675	.456	.044	.002	**.310**	**.096**	.111	.012	-.016	.000	.566	.434
Vocabulary	.674	.454	-.025	.001	**.546**	**.298**	-.009	.000	-.010	.000	.753	.247
Information	.667	.445	.020	.000	**.369**	**.136**	.095	.009	-.051	.003	.593	.407
Comprehension	.562	.316	.031	.001	**.463**	**.214**	-.101	.010	.097	.009	.551	.449
Block Design	.629	.396	-.014	.000	-.024	.001	**.397**	**.158**	.061	.004	.558	.442
Visual Puzzles	.649	.421	.008	.000	-.004	.000	**.382**	**.146**	.015	.000	.567	.433
Matrix Reasoning	.623	.388	.161	.026	-.020	.000	**.226**	**.051**	-.030	.001	.466	.534
Figure Weights	.638	.407	*.152*	*.023*	.111	.012	**.143**	**.020**	-.045	.002	.465	.535
Arithmetic	.686	.471	**.255**	**.065**	.051	.003	.083	.007	.040	.002	.547	.453
Digit Span	.665	.442	**.403**	**.162**	-.034	.001	.000	.000	-.034	.001	.607	.393
Picture Span	.552	.305	**.245**	**.060**	.044	.002	.022	.000	.026	.001	.368	.632
Letter–Number Sequencing	.667	.445	**.400**	**.160**	.027	.001	-.055	.003	-.002	.000	.609	.391
Coding	.347	.120	.061	.004	-.021	.000	-.031	.001	**.641**	**.411**	.536	.464
Symbol Search	.382	.146	-.007	.000	-.012	.000	.040	.002	**.696**	**.484**	.632	.368
Cancellation	.244	.060	-.068	.005	.068	.005	.030	.001	**.458**	**.210**	.279	.721
Total Variance		.351		.034		.051		.028		.075	.540	.460
Explained Common Variance		.651		.063		.095		.052		.139		
ω		.923		.815		.857		.794		.727		
ω_H /ω_{HS}		.823		.167		.257		.135		.562		
Relative ω		.892		.204		.300		.170		.773		
H		.907		.408		.507		.367		.749		
PUC		.800										
ω_H /ω_{HS} Figure Weights on WM		.822		.142		.257		.167		.562		

Note. Bold type indicates coefficients and variance estimates consistent with the theoretically proposed factor. Italic type indicates coefficients and variance estimates associated with an alternate factor (where residual cross–-loading *b* was larger than for the theoretically assigned factor). *b* = loading of subtest on factor; S^2 = variance explained; h^2 = communality; u^2 = uniqueness; ω_H = Omega-hierarchical; ω_{HS} = Omega-hierarchical subscale; *H* = construct reliability or replicability index; PUC = percentage of uncontaminated correlations.

At the group factor level, WM accounted for an additional 3.4%, VC for an additional 5.1%, VS for an additional 2.8%, and PS for an additional 7.5% of the total variance. Of the common variance, WM accounted for an additional 6.3%, VC for an additional 9.5%, VS for an additional 5.2%, and PS for an additional 13.9%. The general and group factors combined to measure 54.0% of the total variance in German WISC-V scores, leaving 46.0% unique variance (combination of specific and error variance).

ω_H and ω_{HS} coefficients were estimated based on the SLT results and presented in Table 6, assigning FW to the VS factor. The ω_H coefficient for general intelligence (.823) was high and sufficient for scale interpretation of a unit-weighted composite score based on the indicators; however, the ω_{HS} coefficients for the four German WISC-V group factors (WM, VC, VS, and PS) were considerably lower (.135-.562). Thus, unit-weighted composite scores based on all subtest indicators of the four German WISC-V group factors, likely possess too little true score variance for confident clinical interpretation, with the possible exception of PS (Reise, 2012; Reise et al., 2013). ω_H and ω_{HS} were also estimated with FW assigned to the WM factor (see Table 6) and coefficients showed a slight decrease in ω_{HS} for WM but a slight increase for VS, but still well below the .50 criterion. *H* indexes indicated an optimally weighted composite score for *g* accounted for 90.7% of *g* variance but WM, VC, and VS group factors were not well defined by their optimally weighted indicators (*H*s < .70). The *H* index of .749 for PS indicated that it was well defined by optimal weighting of its three indicators.

Confirmatory Factor Analyses

Global Fit

Results from CFAs for the 15 German WISC-V primary and secondary subtests are presented in Table 7. Combinatorial heuristics of Hu and Bentler (1999) indicated that Models 1 (*g*) and 2 (Verbal and Performance) were inadequate due to too low CFI and TLI and too high SRMR and RMSEA values. Model 3 was adequate but all models (higher-order and bifactor) that included four or five group factors produced global fit statistics that indicated good model fit to these data, better than one-, two-, or three-factor models. Bifactor versions of models with four and five group factors where AR was not cross-loaded were meaningfully better than higher-order versions in CFI and AIC, but meaningful differences in RMSEA were observed only for Model 4a bifactor and the EFA suggested bifactor compared with the higher-order version. All bifactor models were superior to higher-order versions (ΔAIC > 10) and thus more likely to replicate.

Local Fit

While all models with four or five group factors achieved good *global fit*, assessment of local fit identified numerous problems. Table 8 presents each of the models that contained local fit problems (i.e., nonstatistically significant standardized path coefficients, negative standardized path coefficients, and standardized path coefficients of 1.0). Most of these models were thus considered inadequate.

Table 7. *Maximum Likelihood CFA Fit Statistics for the 15 German WISC-V Primary and Secondary Subtests for the Standardization Sample (*N *= 1,087)*

Model [a]	χ^2	*df*	CFI	ΔCFI	TLI	SRMR	RMSEA	ΔRMSEA	RMSEA 90% CI	AIC	ΔAIC
1 General Intelligence	1,242.69	90	.833	-.157	.806	.072	.109	.080	[.103, .114]	76,177.08	1069.43
2 Higher-Order [b]	1,155.06	88	.846	-.144	.816	.071	.106	.077	[.100, .111]	76,093.45	985.80
3 Higher-Order [c]	651.33	87	.918	-.072	.902	.044	.077	.048	[.072, .083]	75,591.72	484.07
4a Higher-Order [d]	276.83	86	.972	-.018	.966	.030	.045	.016	[.039, .051]	75,219.21	111.56
4a Bifactor [e]	**143.26**	**75**	**.990**	**.000**	**.986**	**.023**	**.029**	**.000**	**[.022, .036]**	**75,107.65**	**0.00**
4a Bifactor (no FW–VS path)*	**143.27**	**76**	**.990**	**.000**	**.987**	**.023**	**.029**	**.000**	**[.021, .036]**	**75,105.66**	**-1.99**
4b Higher-Order [f]	279.23	86	.972	-.018	.966	.030	.045	.016	[.040, .051]	75,221.62	113.97
4c Higher-Order [g]	259.36	85	.975	-.015	.969	.030	.043	.014	[.037, .049]	75,203.75	96.10
4d Higher-Order [h]	257.51	84	.975	-.015	.969	.030	.044	.015	[.038, .050]	75,203.90	96.25
EFA Suggested Bifactor [i]	**143.25**	**75**	**.990**	**.000**	**.986**	**.023**	**.029**	**.000**	**[.022, .036]**	**75,107.63**	**-0.02**
5a Higher-Order [j]	237.98	85	.978	-.012	.973	.029	.041	.012	[.035, .047]	75,182.36	74.71
5a Bifactor [k]	153.60	77	.989	-.001	.985	.024	.030	.001	[.023, .037]	75,117.98	10.33
5b Higher-Order [l]	236.19	85	.978	-.012	.973	.029	.040	.011	[.034, .047]	75,180.57	72.92
5c Higher-Order [m]	217.47	84	.981	-.009	.976	.028	.038	.009	[.032, .044]	75,163.86	56.21
5d Higher-Order [n]	228.20	84	.979	-.011	.974	.029	.040	.011	[.034, .046]	75,174.59	66.94
5e Higher-Order [o]	217.25	83	.981	-.009	.975	.028	.039	.010	[.032, .045]	75,165.63	57.98

Note. Bold text illustrates best fitting models. CFI = comparative fit index; TLI = Tucker–Lewis index (nonnormed fit index); SRMR = standardized root mean square (not available in robust estimation); RMSEA = root mean square error of approximation; AIC = Akaike's information criterion; FW = Figure Weights; VS = Visual Spatial.

[a] Model numbers correspond to those reported in the German WISC-V *Technical Manual* Table 5.2 and are higher-order models (unless otherwise specified) when more than one first-order factor was specified. Subtest assignments to latent factors are specified in Tables 1 and 2. [b-o] Models with local fit problems specified in Table 8.

[*] Best model.

Model Selection

According to the ΔAIC > 10 criterion, the models most likely to generalize were Models 4a bifactor and the EFA suggested bifactor. These were also identified best by ΔCFI and ΔRMSEA criteria. However, local fit difficulties with Models 4a bifactor and EFA suggested bifactor (see Table 8) weighed against their selection without modification. Thus, Model 4a bifactor (Figure 3) and EFA suggested bifactor (Figure 4) and their modifications show remarkable similarity. Differences between these models are with which group factor FW is placed, and in both instances, FW had a negative and not-statistically significant standardized path coefficient with the assigned group factor. Figures 3 and 4 also illustrate modification where the FW group factor path was dropped and the model reestimated, which resulted in an identical model with theoretical alignment of all subtests but FW having only a path from *g*.

Table 8. *Local Fit Problems Identified Within Specified Models*

CFA Model [a]	Local Fit Problem
2 Higher-Order [b]	V factor and higher-order *g* factor linearly dependent on other parameters, *g* factor standardized path coefficient with V factor = 1.0
3 Higher-Order [c]	*g* factor standardized path coefficients with V factor (.943) and P factor (.964) were high
4a Higher-Order [d]	*g* factor standardized path coefficients with VS factor (.946) and WM factor (.919) were high
4a Bifactor [e]	FW standardized path coefficient with VS factor (-.005) was not statistically significant; and the MR standardized path coefficient with VS factor (.136), PS standardized path coefficient with WM (.200), and AR standardized path coefficient with WM (.169) were statistically significant but low
4b Higher-Order [f]	*g* factor standardized path coefficients with FR and WM factor (.945) were high
4c Higher-Order [g]	*g* factor standardized path coefficients with VS+AR factor (.947) was high
4d Higher-Order [h]	*g* factor standardized path coefficients with VS factor (.947) was high, AR standardized path coefficient with VC (.069) not statistically significant, AR standardized path coefficient on VS (.262) was low, removing AR path from VC produces Model 4c
EFA Suggested Bifactor [i]	FW standardized path coefficient with WM factor (-.007) was not statistically significant; MR standardized path coefficient with VS (.138), PS standardized path coefficient with WM (.198), and AR standardized path coefficient with WM (.167) were statistically significant but low; removal of WM–FW path produces same model as 4a Bifactor (without VS–FW path)
5a Higher-Order [j]	FR standardized path coefficient with *g* (.995) extremely high
5a Bifactor [k]	MR (.090) and FW (.090) had low standardized path coefficients with FR and not statistically significant, removal of MR and FW group factor paths eliminates the FR factor
5b Higher-Order [l]	FR standardized path coefficient from *g* = 1.0
5c Higher-Order [m]	FR standardized path coefficient from *g* = 1.0
5d Higher-Order [n]	FR standardized path coefficient from *g* = 1.0, AR standardized path coefficient with VC (.151) was low
5e Higher-Order [o]	FR standardized path coefficient from *g* = 1.0, AR standardized path coefficient (.029) with VC not statistically significant, removal of AR loading with VC produces Model 5d

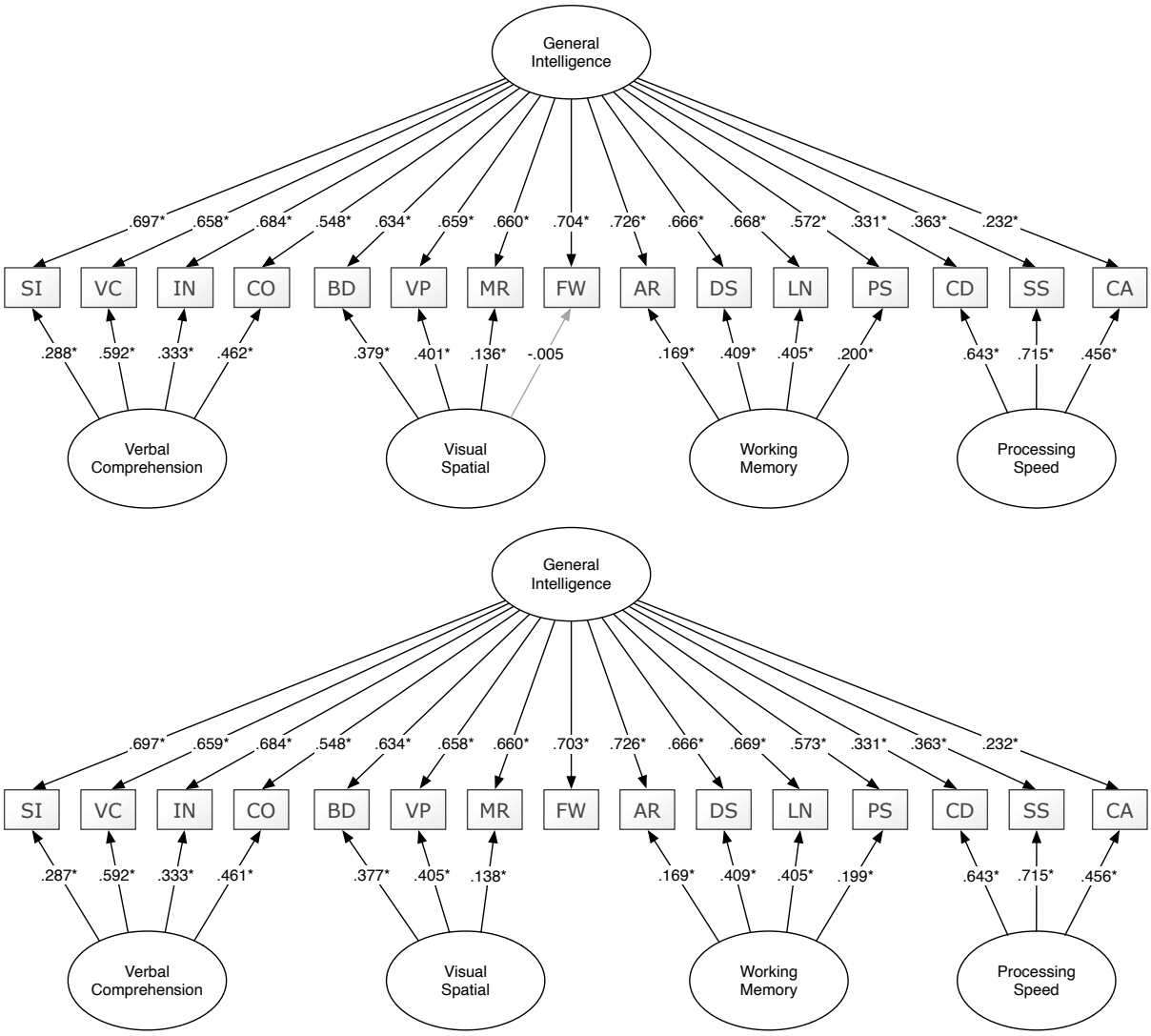
Note. Model number indicates the number of group factors included in the model and model number and letter correspond to those reported in the German WISC-V *Technical Manual*. Bifactor models were added for comparison. Subtest assignments to latent factors are specified in Tables 1 and 2. *g* = general intelligence; V = Verbal; P = Performance; VC = Verbal Comprehension; WM = Working Memory; VS = Visual Spatial; FR = Fluid Reasoning; FW = Figure Weights; MR = Matrix Reasoning; PS = Picture Span; AR = Arithmetic.
[a] CFA model corresponding to Table 7. [b-o] Superscripts correspond model superscript designating local fit problem from Table 7.

Variance and Reliability: Modified Model 4a Bifactor

Table 9 presents sources of variance for the modified Model 4a bifactor (see Figure 3) from the 15 German WISC-V primary and secondary subtests where the group factor path for FW was dropped. This model is identical to the EFA suggested bifactor model with the group factor path for FW dropped (see Figure 4). Most subtest variance was associated with the general intelligence dimension and substantially smaller portions of variance were uniquely associated with the four German WISC-V group factors. ω_H and ω_{HS} coefficients were estimated based on the bifactor results from Table 9 and the ω_H coefficient for general intelligence (.836) was high and sufficient for confident scale interpretation. The ω_{HS} coefficients for the four German WISC-V factors (VC, VS, WM, and PS), however, were considerably lower, ranging from .086 (VS) to .575 (PS). Thus, three German WISC-V

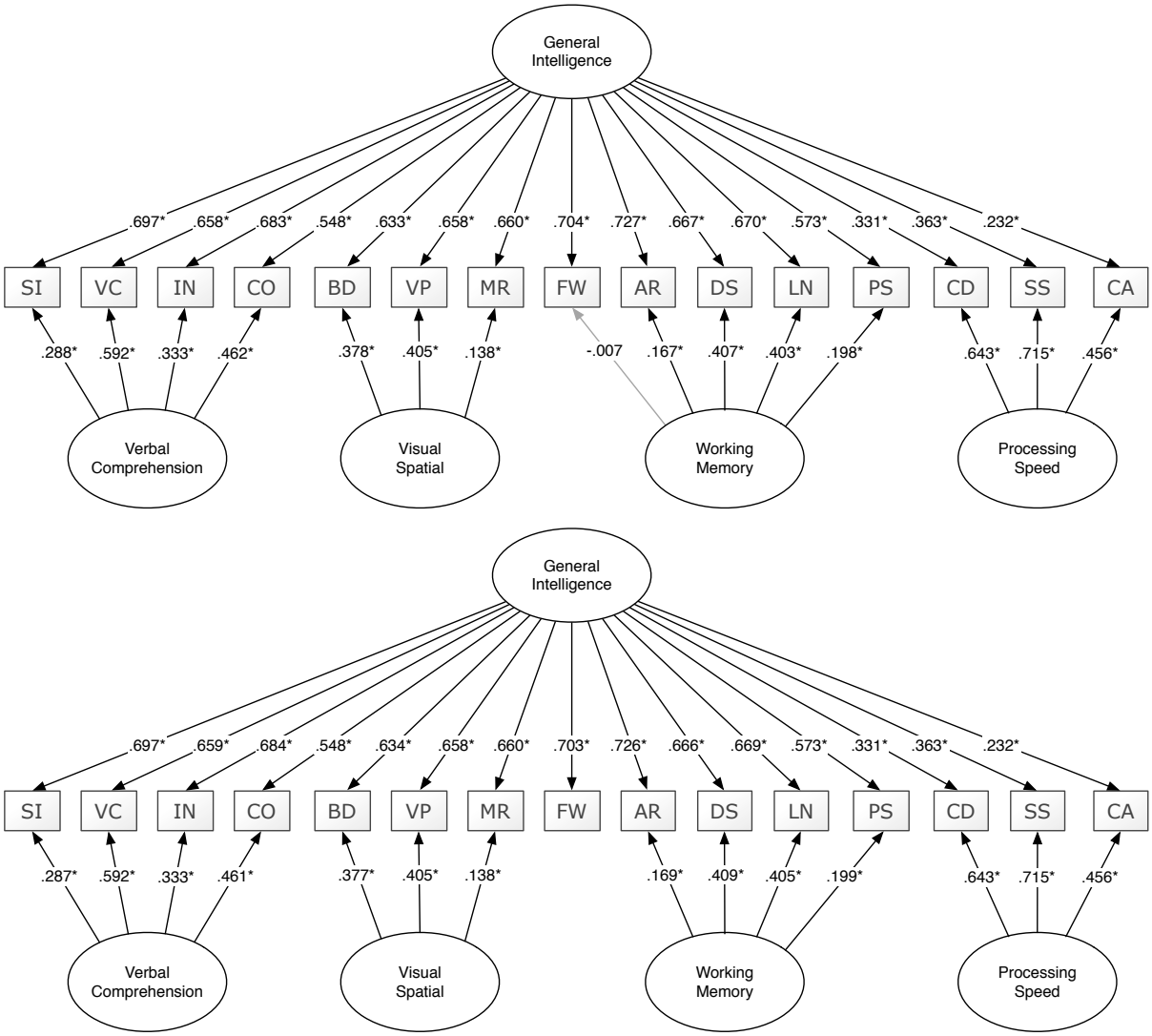
group factors (VC, VS, and WM) likely possess too little unique true score variance in a unit-weighted composite score to support confident clinical interpretation (Reise, 2012; Reise et al., 2013); however, the PS group factor exceeded the minimum criterion for possible interpretation. *H* indexes indicated an optimally weighted composite score for *g* accounted for 90.7% of *g* variance, but the four group factors were not well defined by their optimally weighted indicators (*H*s < .70). For comparison purposes, Table A1 (see online supplement) presents sources of variance for Model 4a bifactor (see Figure 3) from the 15 German WISC-V primary and secondary subtests including FW group factor path and results of explained variances, ω_H and ω_{HS}, and *H* indexes were virtually identical to the modified Model 4a bifactor.

Figure 3. *Bifactor Measurement Model (4a Bifactor), with Standardized Coefficients, for the German WISC-V Standardization Sample (*N *= 1,087) 15 Subtests, With and Without the VS–FW Path*



Note. SI = Similarities, VC = Vocabulary, IN = Information, CO = Comprehension, BD = Block Design, VP = Visual Puzzles, MR = Matrix Reasoning, FW = Figure Weights, AR = Arithmetic, DS = Digit Span, PS = Picture Span, LN = Letter-Number Sequencing, CD = Coding, SS = Symbol Search, CA = Cancellation. $^{*}p < .05$.

Figure 4. *Bifactor Measurement Model (EFA Suggested Bifactor), With Standardized Coefficients, for the German WISC-V Standardization Sample (*N *= 1,087) 15 Subtests, With and Without the VS–FW Path*



Note. SI = Similarities, VC = Vocabulary, IN = Information, CO = Comprehension, BD = Block Design, VP = Visual Puzzles, MR = Matrix Reasoning, FW = Figure Weights, AR = Arithmetic, DS = Digit Span, PS = Picture Span, LN = Letter-Number Sequencing, CD = Coding, SS = Symbol Search, CA = Cancellation. $^{*}p < .05$.

Table 9. *Sources of Variance in the 15 German Wechsler Intelligence Scale for Children-Fifth Edition (WISC-V) Primary and Secondary Subtests for the Standardization Sample (*N *= 1,087) According to a Bifactor Model With Four Group Factors With Visual Spatial to Figure Weights Path Removed*

	General		Verbal Comprehension		Visual Spatial		Working Memory		Processing Speed				
Subtest	*b*	S^2	*b*	S^2	*b*	S^2	*b*	S^2	*b*	S^2	h^2	u^2	ECV
Similarities	.697	.486	.287	.082							.568	.432	.855
Vocabulary	.659	.434	.592	.350							.785	.215	.553
Information	.684	.468	.333	.111							.579	.421	.808
Comprehension	.548	.300	.461	.213							.513	.487	.586
Block Design	.634	.402			.377	.142					.544	.456	.739
Visual Puzzles	.658	.433			.405	.164					.597	.403	.725
Matrix Reasoning	.660	.436			.138	.019					.455	.545	.958
Figure Weights	.703	.494									.494	.506	.999
Arithmetic	.726	.527					.169	.029			.556	.444	.949
Digit Span	.666	.444					.409	.167			.611	.389	.726
Picture Span	.573	.328					.199	.040			.368	.632	.892
Letter–Number Sequencing	.669	.448					.405	.164			.612	.388	.732
Coding	.331	.110							.643	.413	.523	.477	.209
Symbol Search	.363	.132							.715	.511	.643	.357	.205
Cancellation	.232	.054							.456	.208	.262	.738	.206
Total Variance		.366		.050		.022		.027		.076	.541	.459	
ECV		.678		.093		.040		.049		.140			
ω		.926		.859		.805		.818		.725			
ω_H/ω_{HS}		.836		.253		.086		.137		.575			
Relative ω		.903		.295		.107		.168		.793			
Factor Correlation		.914		.503		.294		.370		.758			
H		.907		.506		.276		.319		.668			
PUC		.800											

Note. *b* = loading of subtest on factor; S^2 = variance explained; h^2 = communality; u^2 = uniqueness; ECV = explained common variance; ω_H = Omega-hierarchical (general factor); ω_{HS} = Omega-hierarchical subscale (group factors); *H* = construct reliability or replicability index; PUC = percentage of uncontaminated correlations. Illustrated in Figure 5.

Discussion

Results from the present independent EFA and CFA substantially challenge the German WISC-V structure promoted in the German WISC-V *Technical Manual* on which standard scores and interpretive guidelines are provided. EFA results failed to support a five-factor model as only the SI subtest had a salient loading on the fifth factor which was inadequate and indicative of overfactoring. As with other versions of the WISC-V, there appears to be no separate FR factor and EFA results from both five- and four-factor models show the AR subtest to only saliently load on the WM factor. Given this result, including AR in the pseudocomposite QRI appears misguided. Four first-order factors better represented the German WISC-V structure, but FW saliently loaded on the WM and not VS (formerly PR). The SLT of the four-factor oblique solution showed the primary subtest contribution was related

mostly to *g* rather than to the first-order group factors (except for the PS subtests that poorly measure *g*). The present results replicate the outcomes of two WISC-V EFA studies with international WISC-V versions, the French WISC-V (Lecerf & Canivez, 2018) and the WISC-V^{UK} (Canivez et al., 2019); two EFA studies with the full U.S. WISC-V standardization sample data (Canivez et al., 2016; Dombrowski et al., 2015), and two EFA studies examining the U.S. WISC-V standardization sample normative data partitioned into four age-groups (Canivez, Dombrowski, et al., 2018; Dombrowski, Canivez, et al., 2018). All found a lack of empirical support for five first-order factors and in all these studies the *g* factor accounted for substantially greater common variance and there was strong support for interpretation of composite score estimates of *g*. Also, these studies all showed inadequate portions of unique group factor variance apart from *g* necessary for confident interpretation of factor index scores, except, perhaps, for PS. These results were also observed in a large U.S. clinical sample (Canivez, McGill, et al., 2018).

Present CFA results also failed to support the publisher's preferred measurement model (Model 5e) and instead better supported a bifactor representation of German WISC-V structure with four group factors similar to the present EFA results. When modeling five first-order factors and one higher-order factor with all 15 primary and secondary subtests as promoted by the publisher (including Model 5e), approximate fit statistics *appeared* to support the models, unlike CFA results of five group-factor higher-order models with the U.S. WISC-V that produced model specification errors with negative FR variance (see Canivez, Watkins, & Dombrowski, 2017). However, assessment of local fit identified numerous problems of nonstatistically significant standardized path coefficients, negative standardized path coefficients, and standardized *g* to FR paths of 1.0 or approaching 1.0. The publisher preferred German WISC-V model (Model 5e) included three cross-loadings of AR on VC, FR, and WM identical to the U.S. WISC-V, but present results found the standardized path coefficient of VC to AR (.029) was not statistically significant and the standardized path coefficient from *g* to FR was 1.0 indicating empirical redundancy, thereby indicating Model 5e was not the best model when one considers local fit. A similar result was observed with the French WISC-V where the AR subtest also failed to yield a statistically significant standardized path coefficient from VC, and thus the publisher preferred Model 5e was also not the best model with the French WISC-V (Lecerf & Canivez, 2018). A bifactor representation of the German WISC-V with *g* and *five* group factors (Model 5a bifactor) produced admissible global fit results, but MR and FW did not have statistically significant standardized path coefficients on the FR group factor, thereby challenging FR viability. Removal of nonstatistically significant MR and FW group factor paths eliminated the FR group factor. Thus, in both the higher-order and bifactor representations of the German WISC-V, FR is empirically indistinguishable from psychometric *g*.

These German WISC-V results are not unique and quite similar to EFA and CFA results observed in studies of the WISC-IV (Bodin et al., 2009; Canivez, 2014; Keith, 2005; Styck & Watkins, 2016; Watkins, 2006, 2010; Watkins et al., 2006) and with other Wechsler scale versions (Canivez &

Watkins, 2010a, 2010b; Canivez, Watkins, Good, et al., 2017; Gignac, 2005, 2006; Golay et al., 2013; Golay & Lecerf, 2011; Lecerf & Canivez, 2018; McGill & Canivez, 2016, 2018; Nelson et al., 2013; Watkins & Beaujean, 2014; Watkins et al., 2013). The present results showing dominance of *g* variance and small portions of group factor variance are also not unique to Wechsler scales as similar results have also been observed with the Woodcock–Johnson III (Cucina & Howardson, 2016; Dombrowski, 2013, 2014a, 2014b; Dombrowski & Watkins, 2013; Strickland et al., 2015), the Woodcock–Johnson IV Cognitive and full battery (Dombrowski et al., 2017; Dombrowski, McGill, et al., 2018a, 2018b), the Differential Ability Scale (DAS; Cucina & Howardson, 2016), the DAS–II (Canivez et al., 2020; Canivez & McGill, 2016; Dombrowski et al., 2019), the Kaufman Adolescent and Adult Intelligence Test (Cucina & Howardson, 2016), the KABC (Cucina & Howardson, 2016), the SB5 (Canivez, 2008), the WASI and WRIT (Canivez et al., 2009), and the RIAS (Dombrowski et al., 2009; Nelson & Canivez, 2012, Nelson et al., 2007).

Practical Considerations

The present results have major practical implications in clinical assessment where the FRI is provided yet is not empirically supported by German standardization sample data in either EFA or CFA. This was also observed in the other WISC-V versions (Canivez et al., 2016; Canivez, Watkins, & Dombrowski, 2017; Canivez et al., 2019; Fenollar-Cortés & Watkins, 2019; Lecerf & Canivez, 2018; Watkins et al., 2018). The FR variance is essentially psychometric *g* variance, but this is obfuscated in higher-order models unless variance sources are decomposed (something the publisher has never provided in any WISC-V version) and thus, interpretation of a FR score most likely results in faulty inferences. Furthermore, VCI, VSI, and WMI are scores based on subtests that measure more *g* variance than group factor variance and the unique portions of true score variance provided by VC, VS, and WM are also seemingly inadequate for confident interpretation of scores provided by either unit-weighted or optimally weighted indexes as indicated by low ω_{HS} and *H* coefficients, respectively (Brunner et al., 2012; Reise, 2012; Reise et al., 2013; Rodriguez et al., 2016). Thus, "much of the reliable variance of the subscale scores can be attributable to the general factor, and not what is unique to the group factors" (Rodriguez et al., 2016, p. 225). Factor index scores, as provided by the publisher, conflate *g* variance and group factor variance which cannot be disentangled at the individual level. This too was observed in other WISC-V versions (Canivez et al., 2016; Canivez et al., 2019; Canivez, Watkins, & Dombrowski, 2017; Fenollar-Cortés & Watkins, 2019; Lecerf & Canivez, 2018; Watkins et al., 2018). Users of the German WISC-V can be confident in their individual clinical inferences regarding FSIQ results, but inferences from index scores beyond the FSIQ are likely overinterpretations or misinterpretations as also noted by Pauls et al. (2020). If it is important to generate scores for constructs represented by the group factors, and distinction between VS and FR, then it appears there is much work to be done to create tasks that accomplish this (if that is even possible). As Beaujean and Benson (2019) argue based on the work of Luecht et al. (2006), to achieve this, "publishers should not attempt to create instruments that concurrently measure some unitary attribute (e.g., a general attribute) and

then try to spread out the same information across multiple scores of more specific attributes" (p. 130). Thus, it might be necessary to refrain from creating multidimensional measures of intelligence altogether and instead trying to develop multiple unidimensional tests, each designed to measure a single, theoretically well-defined attribute. Furthermore, because the German WISC-V appears to only measure *g* well and provides group factor scores with inadequate interpretive value beyond *g*, it may be time and cost effective to use a measure like the German version of the Reynolds Intellectual Assessment Scales (RIAS; Hagmann-von Arx & Grob, 2014) as a more efficient assessment of *g*. Because the German RIAS includes only four (two verbal, two nonverbal) intelligence subtests representing verbal and nonverbal group factors there are fewer scores and comparisons that might be misused.

Theoretical Considerations

In addition to practical implications there are also theoretical implications for the present results. The superiority of the bifactor model observed with the German WISC-V which allows the general intelligence dimension to directly influence subtest indicators, while simultaneously allowing group factor influences on subtests, is consistent with Spearman's (1927) conceptualization of intelligence as well as Carroll's (1993; Beaujean, 2015b; Brunner et al., 2012; Frisby & Beaujean, 2015; Gignac, 2006, 2008; Gignac & Watkins, 2013; Gustafsson & Balke, 1993). Beaujean (2015b) noted Spearman's conceptualization of general intelligence was of a factor "that was directly involved in all cognitive performances, not indirectly involved through, or mediated by, other factors" (p. 130) and he also opined that "Carroll was explicit in noting that a bi-factor model best represents his theory" (p. 130). This conceptualizes *g* as a breadth factor that permits multidimensionality by determining how broad abilities perform independent of the *g* factor and was also preferred by Gignac (2008). Bifactor representation of *g* is less complicated and can be considered more parsimonious (Cucina & Byle, 2017; Gignac, 2008) with *g* and group factors at the same level of inference (see also Canivez, 2013b; Thompson, 2004). This is in contrast to the superordinate conceptualization of *g* represented by the publisher preferred higher-order model where the influence of psychometric *g* is fully mediated by the first-order group factors.

The theoretical appropriateness of bifactor models of intelligence was questioned by Reynolds and Keith (2013) who argued "we believe that higher-order models are theoretically more defensible, more consistent with relevant intelligence theory (e.g., Jensen, 1998), than are less constrained hierarchical [bifactor] models" (p. 66). Gignac (2006, 2008) alternatively suggested that because *g* was the most substantial factor it should be directly modeled and that full mediation of *g* in the higher-order model was what required explicit theoretical justification. Carroll (1993, 1995) pointed out that subtest scores reflect variation of both a general and more specific group factor but because they generally contain larger portions of *g* variance the subtest scores reliability is primarily a function of the general factor, not the specific group factor. Other researchers have also argued that Spearman's (1927) and Carroll's (1993) conceptualizations of intelligence are better represented by the bifactor model and not

the higher-order model (Beaujean, 2015b; Brunner et al., 2012; Frisby & Beaujean, 2015; Gignac, 2006, 2008; Gignac & Watkins, 2013; Gustafsson & Balke, 1993).

Murray and Johnson (2013), Gignac (2016), and Mansolf and Reise (2017) determined that bifactor models might be found superior in fit due to unmodeled complexities such as small cross-loadings of indicators on multiple factors, proportionality constraint, or tetrad constraints; so the bifactor model may not be statistically better. Analyses of simulations of bifactor and higher-order models by Morgan et al. (2015) confirmed that regardless of the true structure, both types of models exhibited good model fit. Mansolf and Reise (2017) admitted that presently there is no technical solution to resolve the problem that bifactor and higher-order models cannot be distinguished by fit indices. Given this problem, Watkins et al. (2018) suggested requiring "a parsimonious, substantively meaningful model that fits observed data adequately well" (MacCallum & Austin, 2000, p. 218) and that fulfills the purpose of measurement; while Murray and Johnson (2013) concluded that when estimating or accounting for domain-specific abilities, the "bifactor model factor scores should be preferred" (p. 420). In the case of the German WISC-V, and all Wechsler scales, factor index scores and the numerous factor index score comparisons (ipsative and pairwise) and inferences made from such comparisons beyond the FSIQ is focusing on domain-specific abilities, so a bifactor model is necessary. Researchers and clinicians must know how well German WISC-V group factor scores perform independent of the *g* factor score (F. F. Chen et al., 2006; F. F. Chen et al., 2012).

Reise et al. (2010) also concluded that a bifactor model, which contains a general factor but permits multidimensionality, is better than the higher-order model so that relative contribution of group factors independent of the general factor (i.e., general intelligence) may be determined. This has also been recommended by others (Brunner et al., 2012; DeMars, 2013; Morin et al., 2016; Reise, 2012; Reise et al., 2013; Rodriguez et al., 2016). Given the absence of the FR factor and poor ω_{HS} and *H* coefficients for VC, VS, and WM, interpretation of these German WISC-V index scores "as representing the precise measurement of some latent variable that is unique or different from the general factor, clearly, is misguided" (Rodriguez et al., 2016, p. 225).

A final theoretical implication of present German WISC-V results relates to the so-called CHC theory (McGrew, 2009; Schneider, & McGrew, 2018). While several group factors (broad abilities) could be located, but not FR, the dominance of the *g* factor in explaining common variance in the German WISC-V is consistent with Carroll's three stratum theory and not with the Cattell–Horn extended *Gf-Gc* theory. Cucina and Howardson (2017) offered the same conclusion in their analyses. Given the volume of evidence regarding preeminence of *g* variance in Wechsler scales and other intelligence tests, an annulment of the unhappy arranged marriage of the theories of Cattell–Horn and Carroll appears warranted (Canivez & Youngstrom, 2019; Wasserman, 2019).

Limitations

The present study examined EFA and CFA for the full German WISC-V standardization sample but it is possible that different age groups within the German WISC-V standardization sample might

produce somewhat different results. EFA and CFA with different age subgroups should be conducted to examine structural invariance across age. Other demographic variables where invariance should be examined include sex and socioeconomic status. While Pauls et al. (2020) reported factor structure invariance for the publisher preferred German WISC-V measurement model (Model 5e) across gender [sic] and reported configural, first-order and second-order metric invariance, this only shows that the inadequate measurement model did not vary between groups. Invariance of the better represented bifactor model with four group factors identified in the present study should be examined. Structural invariance across gender [sic] was also reported for the U.S. WISC-V (H. Chen et al., 2015) but bifactor models and models with fewer group factors were also not examined. Because the publisher denied access to the German WISC-V standardization sample raw data, we are unable to independently conduct such analyses.

The present analyses were of the standardization sample and results may not generalize to other populations such as clinical groups or other independent samples of nonclinical groups, participants of different races/ethnicities, immigration status, or language minorities. Finally, the results of the present study only consider the latent factor structure and cannot fully test the construct validity of the German WISC-V. Examinations of German WISC-V relationships with external criteria (e.g., scholastic achievement) are needed. Examinations of incremental predictive validity (Canivez, 2013a; Canivez et al., 2014; Glutting et al., 2006) to determine if reliable achievement variance is incrementally accounted for by the German WISC-V factor index scores beyond that accounted for by the FSIQ (or through use of latent factor scores, see Kranzler et al., 2015) and diagnostic utility (see Canivez, 2013b) studies should also be conducted. However, the small portions of true score variance uniquely contributed by the four group factors identified here with the German WISC-V standardization sample makes it unlikely that German WISC-V factor index scores would provide meaningful additive interpretive value. Finally, while the present findings show dominance of general intelligence this does not mean that psychometric *g* is a single psychological attribute and perhaps, as indicated by Kovacs and Conway (2016) and Kan et al. (2019), the *g* factor may be a formative variable rather than a reflective variable; although Gottfredson (2016) argued the Kovacs and Conway Process Overlap Theory actually can be considered support for *g*. The present results (and Pauls et al., 2020) suggest clinicians should interpret with caution the factor index scores, if at all, due to low amounts of unique contributions of the broad abilities. However, that does not mean that broad abilities do not exist, they just may not be adequately measured by the German WISC-V or other intelligence tests.

Conclusion

Based on the present results, the German WISC-V as presented in the German WISC-V *Technical Manual* appears to be overfactored and the strong replication of previous EFA and CFA findings with the U.S. WISC-V and other international versions further indicates primary, if not exclusive, focus of interpretation on the German WISC-V FSIQ. The attempt to divide the PR factor into separate VS and FR factors was again unsuccessful by not producing a viable FR factor. Therefore,

generating standard scores and comparisons for FR is potentially misleading and users likely misinterpreting scores. If FR cannot be located and does not make a unique contribution then the publisher should provide normative scores for four (VC, VS, WM, and PS) rather than *five* first-order factors, but the small portions of unique variance contribution by VC, VS, and WM likely render them of little utility; and the poor measurement of *g* by PS subtests might call for elimination from a test of general intelligence. The present results will help users of the German WISC-V make informed decisions about whether, when, and how to use the German WISC-V and which scores have adequate psychometric support for confident interpretation. Researchers and clinicians must rely on more than the test technical manuals to appropriately use test scores and their comparisons because test users bear "the ultimate responsibility for appropriate test use and interpretation" (AERA, APA, & NCME, 2014, p. 141). This will also allow professionals ethically using the German WISC-V to "know what their tests can do and act accordingly" (Weiner, 1989, p. 829).

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APPENDIX D: Study 4

Bünger, A., Grieder, S., Schweizer, F., & Grob, A. (2021). *The comparability of intelligence test results: Group- and individual-level comparisons of seven intelligence tests.* Manuscript submitted for publication.

The Comparability of Intelligence Test Results: Group- and Individual-Level Comparisons of Seven Intelligence Tests

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Abstract

A large body of research shows that IQs obtained from different intelligence tests substantially correlate on the group level. Yet, there is little research investigating whether different intelligence tests yield comparable results for individuals. This is, however, paramount to the application of intelligence tests in practice, as high-stakes decisions are based on individual test results. We therefore investigated whether seven current and widely used intelligence tests yield comparable results for individuals aged 4–20 years. We found mostly substantial correlations, though several significant mean differences on the group level. Results on individual-level comparability indicate that the interpretation of exact IQ scores does not hold. Even the 95% confidence intervals could not be reliably replicated with different intelligence tests. Similar patterns appeared for the individual-level comparability of nonverbal and verbal intelligence factor scores as well. Further, the nominal level of intelligence systematically predicted IQ differences between tests, with above- and below-average IQ associated with larger differences compared to average IQ. Analyses based on continuous data confirmed that differences seem to increase toward the above-average IQ range. These findings are critical, as these are the ranges in which diagnostic questions most often arise in practice. Implications for test interpretation and test construction are discussed.

Keywords: intelligence tests, IQ, comparability, individual-level, children and adolescents

Introduction

In the field of applied educational, learning, and school psychology, psychometric test batteries are often applied to assess the developmental status and abilities of a child or adolescent. These test results provide the basis for far-reaching decisions concerning educational support, interventions, and therapy (e.g., curative education, special education programs, support for gifted children, attention training). When making an educational decision, one of the most frequently assessed domains is an individual's cognitive ability, measured by intelligence tests. This might be because many diagnoses that are relevant in the field of school psychology can only be given in relation to a standardized intelligence test, as recommended by the current versions of the *International Statistical Classification of Diseases and Related Health Problems* (ICD; World Health Organization [WHO], 2018) and the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychiatric Association, 2013). In addition, intelligence is one of the most investigated constructs of psychology and numerous tests are available in many languages to provide age-standardized intelligence scores, that is, IQs (Lubinski, 2004). Furthermore, numerous studies have confirmed the association between intelligence and central areas of life, such as educational success (Deary, Strand, Smith, & Fernandes, 2007), professional success and income (Damian, Su, Shanahan, Trautwein, & Roberts, 2015), success in interpersonal relationships (Aspara, Wittkowski, & Luo, 2018), health (Wrulich et al., 2014), and even longevity (Calvin et al., 2011).

Group- and Individual-Level Comparability

Currently, a large number of different intelligence tests are available to diagnosticians. For example, according to a keyword search, there are 39 intelligence tests available for purchase via *Testzentrale* (http://www.testzentrale.ch), an official distributor of German psychometric tests. As German tests are often adaptations of U.S. originals, there are probably even more widely used intelligence tests available in the United States. The opportunity to choose between different intelligence tests has several advantages. For example, learning effects can be minimized by selecting an alternative test battery when testing multiple times. Moreover, additional test batteries can be used to confirm the results of one test. Finally, special characteristics of an individual or of a specific counseling situation can be taken into account by, for instance, providing nonnative speakers with tests that are not specific to any one culture (Hagmann-von Arx, Petermann, & Grob, 2013), or by using a short test (screening) when intelligence is assessed as a control variable (Hagmann-von Arx & Grob, 2014). Apart from these advantages of having several intelligence tests at our disposition, it also raises the central question whether the various tests provide comparable results. This is what is assumed and expected. Interestingly, despite controversial debates and theories about the psychometric structure of intelligence (Beaujean & Benson, 2019a, 2019b; Gignac, 2008; Kovacs & Conway, 2019), most contemporary and well validated intelligence tests relate to the same intelligence theory, the Cattell–Horn–Carroll (CHC) theory of cognitive abilities (McGrew, 1997, 2005, 2009). In CHC theory, intelligence is modeled as a multidimensional construct consisting of a number of different abilities that

are hierarchically structured on three strata: A general intelligence factor *g* is on the top Stratum III, broad abilities are on Stratum II, and more narrow abilities are on Stratum I. Tests relating to CHC theory usually provide not only a Full-Scale IQ (FSIQ) representing *g* but also a varying number of factor index scores (group factors) representing the more specific broad abilities and subtests representing more narrow abilities. Factor index scores are used to identify specific strengths and difficulties (cognitive profile analysis) by about 49% to 55% of school psychologists in the United States (Benson, Floyd, Kranzler, Eckert, & Fefer, 2018), despite growing evidence against their usefulness (McGill, Dombrowski, & Canivez, 2018). The fact that most contemporary intelligence tests relate to CHC theory strengthens the assumption that not only IQs but also CHC factor scores derived from different test batteries are comparable ostensibly. Furthermore, diagnosticians are advised to exclusively apply intelligence tests with strong evidence for reliability and validity (American Educational Research Association, American Psychological Association, & National Council on Measurement in Education, 2014; International Test Commission, 2001) in terms of content, construct (i.e., convergent and divergent), and criterion (i.e., concurrent and predictive power) validity. Tests that meet these criteria are assumed to render reliable and valid results, and IQs obtained in different test batteries are expected to be comparable (Floyd, Clark, & Shadish, 2008) if they are composed of multiple, diverse, and reliable subtests with high loadings on a general intelligence (*g*) factor (Jensen & Weng, 1994). This assumption is supported by the principle of aggregation (Rushton, Brainerd, & Pressley, 1983), which states that the sum of multiple measurements (IQ based on multiple subtests) represents a more stable predictor than a single measurement (IQ based on a single subtest) because measurement error is averaged out. Moreover, a large body of research has confirmed that many intelligence test batteries are highly correlated (Allen, Stolberg, Thaler, Sutton, & Mayfield, 2014; Baum, Shear, Howe, & Bishop, 2015; Hagmann-von Arx, Grob, Petermann, & Daseking, 2012; Hagmann-von Arx, Lemola, & Grob, 2018), which can be seen as an indication of convergent validity, that is, that the test batteries measure the same constructs (Neukrug & Fawcett, 2014). These results are based on group-level comparisons.

Yet, the fact that different intelligence tests correlate highly on the group level is a necessary but not a sufficient criterion for comparability, because diagnoses are given to individuals and because decisions are made based on individual intelligence test results. To ensure that these decisions and diagnoses are valid, the tests must also be comparable on the individual level. The importance of expecting comparable results across intelligence tests on an individual level gets evident when considering that diagnoses are often based on cut-off values. For instance, intellectual disability or giftedness is defined as two or more standard deviations below or above the mean, respectively, obtained on normed and standardized tests. Furthermore, the current valid version of the ICD, the ICD-10 (World Health Organization [WHO], 1990) requires a significant discrepancy (usually 1.2 to 1.5 *SD*s) between below-average school performance (e.g., reading, writing, or mathematical skills) and IQ for the diagnosis of a learning disorder (e.g., dyslexia or dyscalculia). These types of diagnoses often

determine if a child is eligible for educational resource allocation related to special education. It is therefore crucial to have very accurate intelligence measures to ensure that the presence or absence of a diagnosis does not vary with the use of different tests. Currently, it is still a common practice to determine the presence or absence of such diagnoses based on rigid (also called *fixed*) cut-offs (McNicholas et al., 2018). That means, if individual scored only one point above (intellectual disability) or below (intellectual giftedness) the cut-off, the criteria for the specific diagnosis would not be met. Consequently, the presence or absence of a diagnosis would change depending on the chosen test if two tests do not exactly produce the same score for the same individual in the vicinity of the cut-off (e.g., 69 in one and 71 in another test). However, it is widely known that psychometric tests cannot measure intelligence with pinpoint accuracy and always contain measurement error. Instead of using fixed cut-offs, most diagnostic guidelines therefore recommend applying flexible cut-offs by considering a confidence interval around the obtained score (Farmer & Floyd, 2018). In this regard, it is important to investigate whether the confidence intervals of two obtained IQ scores overlap to indicate individual-level comparability. This means that it should not matter which test is used to assess an individual's intelligence, as the IQ should always be equal given a certain error tolerance. Floyd et al. (2008) referred to this type of comparability as the *exchangeability of IQs*. As yet, there is surprisingly little research on this, with only two studies available that investigated the comparability of IQs on the individual level (Floyd et al., 2008; Hagmann-von Arx et al., 2018) and one study that investigated the comparability of CHC broad abilities on the individual level (Floyd, Bergeron, McCormack, Anderson, & Hargrove-Owens, 2005). All three studies used the overlap of the 90% confidence intervals (CIs) of each intelligence test pair as criterion for comparability. The advantage of using CIs is that it takes the unreliability of measurements into account. However, when using CIs, the definition of comparability varies across test comparisons, as reliability and CIs differ between tests. Floyd et al. (2005, 2008) therefore used an absolute difference between two scores less than or equal to 10 IQ points as a second criterion for comparability. This is in line with guidelines that recommend considering 5 IQ points above and below the obtained IQ when diagnosing intellectual disability, consequently considering the presence of this diagnosis up to an IQ of 75 (e.g., American Association on Intellectual and Developmental Disabilities, 2020; American Psychiatric Association, 2017; Bergeron, Floyd, & Shands, 2008). In the first study, Floyd et al. (2008) compared seven English intelligence tests with standardization years between 1987 and 2003 for an age range of 8 to 16 years, with one sample also covering undergraduate students. Findings showed that the IQs differed in about 36% of intelligence test pairs when considering the 90% CI, and in about 28% when considering the 10-point IQ criterion. In the second study, Hagmann-von Arx et al. (2018) compared five German intelligence tests for 6- to 11-year-old children and with standardization years between 2003 and 2012. The authors found that IQs differed in about 24% of intelligence test pairs when considering the 90% CI. The two studies indicate that for 64% to 76% of schoolchildren, the IQs obtained from different tests can be considered comparable on the individual level. In the third study, Floyd et al. (2005) compared four CHC broad

abilities scores (crystallized intelligence, fluid reasoning, visual processing and processing speed) obtained in seven English intelligence tests with standardization years between 1989 and 2004 for an age range of 3 to 16 years, with one sample also covering undergraduate students. Findings showed that the CHC broad abilities scores differed in 14%–22% when considering the 90% CI and in 29–38% when considering the 10-point IQ criterion.

Overall, these findings indicate that decisions, diagnoses, and cognitive profile interpretation based on a single test are of questionable validity for a fourth to a third of children.

Reasons for (In-)Comparability

To better understand this relatively high percentage of incomparability, it is important to consider potential sources of variability in intelligence test scores that do not stem from true ability of the examinees. In addition to unsystematic random errors, these sources include specific test characteristics, characteristics of the test situation (environmental influences), personal characteristics of the examinees, and examiner's influence (National Research Council, 2002).

Test batteries that are applied for important decisions must provide reliable test results, that is, have reliability coefficients of at least .90 (Aiken & Groth-Marnat, 2005; Evers, 2001; Nunnally & Bernstein, 1994). This means that even if the composite score of a particular test fulfills this criterion, it always contains an unsystematic random error component. This is usually taken into account with the CI plotted around the obtained IQ. Furthermore, it is possible that test batteries lead to inconsistent test results because of differences in standardization. For example, the historical time of standardization may be an influential aspect, as demonstrated by Flynn (1987, 2009). The Flynn effect refers to the fact that the average intelligence of a population increases between 3 and 5 IQ points per decade. In contrast, since the end of the 1990s, there have also been studies showing a negative or so- called anti-Flynn effect. Authors of these studies postulate that average IQs have been declining again since the 1990s (Dutton & Lynn, 2014; Lynn & Harvey, 2008; Sundet, Barlaug, & Torjussen, 2004). Regardless of whether there is an increase or decrease in average intelligence, performance on intelligence tests appears to be influenced by culture and cohort effects. This issue can play an important role in the comparability of test results, especially if the standardization years of two tests are far apart. Moreover, IQs are not a pure representation of an individual's true cognitive ability (intelligence construct or true *g*). Therefore, it is important to consider test characteristics that affect measurement accuracy and that vary across intelligence tests (Farmer, Floyd, Reynolds, & Berlin, 2020). For instance, although most contemporary intelligences tests relate to the CHC theory, the composite score of an intelligence test, which is considered an indicator of *g*, may be calculated based on different sets of subtests that vary in number as well as heterogeneity of content. Especially for IQs that are based on only a few subtests, inadequate content sampling poses a risk for inflation or deflation of IQs and may therefore reduce their comparability (Farmer et al., 2020). In addition, systematic differences in task format may occur (e.g., verbal or nonverbal, paper-and-pencil or computer based; Floyd et al., 2008). This may interact with conditions and characteristics of individual examinees such as limited language skills, for example, due

to migration background (Daseking, Lipsius, Petermann, & Waldmann, 2008), or any type of speech or language disorder (Gallinat & Spaulding, 2014; Miller & Gilbert, 2008), as well as impaired fine motor skills (Yin Foo, Guppy, & Johnston, 2013). Regarding characteristics of a specific test situation, the order of test administration with practice or fatigue effects (Hausknecht, Halpert, Di Paolo, & Moriarty Gerrard, 2007), the time interval between two or more tests, or the time of day (Gupta, 1991) in combination with "morningness" and "eveningness" of examinees (Goldstein, Hahn, Hasher, Wiprzycka, & Zelazo, 2007) as well as general motivation in the specific test situation (Duckworth, Quinn, Lynam, Loeber, & Stouthamer-Loeber, 2011) may cause variance in intelligence test scores that does not reflect an examinee's true ability. To investigate to what extent intelligence test scores are influenced by individuals and test procedures, Hagmann-von Arx et al. (2018) and Floyd et al. (2008) applied generalizability theory analyses (Briesch, Swaminathan, Welsh, & Chafouleas, 2014; Cronbach, Gleser, Nanda, & Rajaratnam, 1972). Both studies showed that no more than 4% of the variance in IQs could be attributed to specific test characteristics. The largest part of the variance (7%–27%, Floyd et al., 2008; 29%–42%, Hagmann-von Arx et al., 2018) was attributable to interactions between the examinees and the test situation. However, only Hagmann-von Arx et al. (2018) explored the impact of specific variables, such as time interval between two tests, the order of test administration, or qualitative nominal intelligence levels, on intelligence test differences and they could not detect any significant systematic effect.

Limitations of Previous Studies

The three previous studies that focused on individual-level comparability of intelligence tests results show several limitations. First, they did not cover the whole age span in which questions relevant to school psychology tend to arise. Furthermore, age was not investigated as a possible predictor of test score differences, and although Hagmann-von Arx et al. (2018) discussed language skills and migration background as a possible influence on test comparability, they did not include any language or cultural background variable in their analyses. Moreover, to investigate the possible impact of intelligence level on test comparability, Hagmann-von Arx et al. (2018) calculated a mean IQ across all intelligence tests that they carried out. This practice is problematic as it obfuscates the influence of systematic errors on the specific test results (Schneider & McGrew, 2011). Another limitation is that both studies investigated the overlap of the 90% CIs, whereas in practice it is most often the 95% CI that is used for interpretation.

The Present Study

Despite previous findings that question the exchangeability of IQs and broad ability scores on an individual level, diagnostic guidelines in the context of school psychology are often still based on cut-off scores what implies the assumption of score exchangeability. This shows that more research investigating individual-level comparability of intelligence test scores is necessary. The aim of our study was therefore to gain further insight into the comparability of intelligence test scores with a main focus on individual-level comparability, addressing the aforementioned limitations from previous

studies. In our study, we addressed the following two directional hypotheses and four open research questions:

Hypothesis 1: We expected substantial correlations between IQs obtained in seven different intelligence tests.

Hypothesis 2: We expected comparable mean scores on the group level.

Research Question 1: We explored the comparability of IQs on an individual level using several criteria for comparability including criteria that consider reliability (i.e., overlap of the 90% and 95% CIs) as well as criteria that are independent of reliability and therefore invariant across test batteries (i.e., maximum absolute difference of 10 IQ points and nominal intelligence level).

Research Question 2: We explored whether the year of standardization (with respect to a possible Flynn or anti-Flynn effect) influenced comparability such that tests with earlier standardization years revealed higher IQs than tests with a more recent standardization or vice versa.

Research Question 3: We investigated the individual-level comparability for nonverbal (nonverbal intelligence; NI) and verbal (verbal intelligence; VI) factor indexes of each intelligence test and examined whether individual-level comparability was significantly higher or lower for NI and VI versus IQ.

Research Question 4: Last, we investigated whether age, bilingualism, time interval between two tests, order of test administration, and nominal intelligence level were significantly related to IQ differences as an indicator of comparability. We think providing precise evidence on these questions will have vast consequences for theory and practice in intelligence testing.

To investigate these hypotheses and research questions, we assessed seven current and widely used intelligence tests across childhood and adolescence in German-speaking countries. The wide age range of participants in our sample (4–20 years) covers the preschool, primary, and secondary school years as well as the years after high school graduation, representing the entire age range of development in which school-psychology issues arise.

Method

Participants

The sample was a subsample of the standardization and validation study of the Intelligence and Development Scales–2 (IDS-2; Grob & Hagmann-von Arx, 2018) as well as of the German adaptation of the Stanford–Binet Intelligence Scales–Fifth Edition (SB5; Grob, Gygi, & Hagmann-von Arx, 2019). We included 383 children and adolescents (M_{age} = 10.31 years, SD_{age} = 3.96; age range: 4 to 20 years; 47% boys). The percentage of bilingual and nonnative speakers was 32%. All participants spoke German well enough to understand instructions and to give oral answers. None of the participants had a speech or language disorder. According to parent and self-reports, 10 of the included participants were diagnosed with a learning disorder (e.g., dyslexia or dyscalculia), one was diagnosed with Asperger's syndrome, and 15 were diagnosed with attention-deficit/hyperactivity disorder. The pattern of test

comparability did not change with the inclusion of these participants; hence they were not excluded from the study. Forty-eight percent of the participants' mothers held a university degree.

Measures

For the intelligence test comparisons, we applied the IDS-2 (Grob & Hagmann-von Arx, 2018), the German adaptations of the SB5 (Grob et al., 2019), the Reynolds Intellectual Assessment Scales (RIAS; Hagmann-von Arx & Grob, 2014), the Snijders Oomen Nonverbal Intelligence Test 6-40 (SON-R 6-40; Tellegen, Laros, & Petermann, 2012), the Wechsler Adult Intelligence Scale–Third Edition (WAIS-III; von Aster, Neubauer, & Horn, 2006), the Wechsler Intelligence Scales for Children–Fourth Edition (WISC-IV; Petermann & Petermann, 2011), and the Wechsler Preschool and Primary Scale of Intelligence–Third Edition (WPPSI-III; Petermann, 2009). Details on the intelligence tests such as age range, latent factors measured, year of standardization, and reliability (internal consistencies), as well as indications of which latent factors were classified as IQ, NI, and VI, can be found in Table 1.

Table 1. *Test Characteristics*

Test	Age range (Years; months)	Factors and indices	Standardization years	Internal consistency [a]		
				FSIQ	NI	VI
IDS-2	5;0 to 20;11	Visual processing, processing speed, auditory short-term memory, visuospatial short-term memory, long-term memory, abstract reasoning [b], verbal reasoning [c], general intelligence [d]	2015–2017	.98	.94	.96
SB5	4;0 to 80;11	Fluid reasoning, knowledge, quantitative reasoning, visuospatial processing, working memory, verbal intelligence[c], nonverbal intelligence [b], general intelligence [d]	2015–2017	.99	.98	.99
RIAS	3;0 to 99;11	Verbal intelligence [c], nonverbal intelligence [b], general intelligence [d]	2011–2012	.95	.93	.94
SON-R 6-40	6;0 to 40;11	Nonverbal intelligence [b,d]	2009–2011	.95	.95	N/A
WAIS-III	16;0 to 89;11	Verbal intelligence [c] (verbal comprehension, working memory), performance intelligence [b] (perceptual organization, processing speed), general intelligence [d]	1999–2005	.97	.94	.96
WISC-IV	6;0 to 16;11	Verbal comprehension [c], perceptual reasoning [b], working memory, processing speed, general intelligence [d]	2005–2006	.97	.93	.94
WPPSI-III	3;0 to 7;2	Verbal intelligence [c], performance intelligence [b], processing speed, general language, general intelligence [d]	2001–2002	.95	.91	.92

Note. FSIQ = Full-scale IQ; NI = Nonverbal Intelligence Index; VI = Verbal Intelligence Index; IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation.

[a] Reliability is reported as internal consistency coefficients. According to the corresponding test manuals, coefficients of the IDS-2, RIAS, and WISC-IV are Cronbach's α and coefficients of the SB5, WAIS-III, and WPPSI-III are split-half reliabilities. [b] Index used for nonverbal intelligence analyses. [c] Index used for verbal intelligence analyses. [d] Index used for FSIQ analyses.

Procedure

Recruitment was carried out for preschoolers via day-care centers, nursery schools, and kindergartens and for school-aged children and adolescents via schools, institutions, and private contacts. All participants were assessed with the IDS-2 and/or the SB5 and with one or more of the above-mentioned intelligence tests that were suitable for their age (as described in Table 1). Due to restrictions in testing time, some participants could not be assessed with all measures that were suitable for their age. Of the 383 participants, 50 (13.1%) were assessed with two, 114 (29.8%) were assessed with three, 202 (52.7%) were assessed with four, and 17 (4.4%) were assessed with five intelligence tests. The exact numbers of participants per test and test combination are listed in Tables 2 and 3. The assessments were conducted between June 2015 and February 2018 in individual settings by trained psychology major students. The tests took place at participants' homes or at the Department of Psychology of the University of Basel. The participants (16- to 20-year-olds) or their parents (4- to 15-year-olds) received written feedback on the test results, where standard scores were available (i.e., RIAS, SON-R 6-40, WAIS-III, WISC-IV, WPPSI-III). For the standardization study, participants received a voucher worth CHF 30 (for a 3 to 3.5-h test; i.e., IDS-2) or CHF 20 (for a 2-h test; i.e., SB5) instead of written feedback, as standard scores were not available for these tests at that time. The Ethikkommission Nordwest- und Zentralschweiz [Ethics Commission Northwest and Central Switzerland] approved the study protocol. Parents gave written informed consent for their child to participate and participants gave oral (children) or written (adolescents) informed assent.

Statistical Analyses

In a preliminary step, the raw test scores of the intelligence tests were transformed into the respective IQs ($M = 100$, $SD = 15$) using the test manuals. The assumption of normal distribution was tested using the Shapiro–Wilk test. The IQs of the IDS-2, SB5, RIAS, SON- R 6-40, WAIS-III, and WPPSI-III were normally distributed ($W = .959–.994$, $p > .05$). The distribution of the WISC-IV scores deviated significantly from a normal distribution ($W = .967$, $p < .001$), and therefore nonparametric methods were used for the intelligence test comparisons involving the WISC-IV. We tested whether the mean IQs in our sample differed from the expected mean of 100 using single-sample *t* tests and a single-sample Wilcoxon signed-rank test for the WISC-IV. The analyses described in the following refer to the comparison of all intelligence test pairs, except the Wechsler scales (i.e., WAIS-III, WISC-IV, and WPPSI-III). They were not compared among each other, since these each apply to different age ranges. Pearson's product-moment correlations and Spearman's rank-order correlations were calculated for all intelligence test pairs (Hypothesis 1). Due to the restricted standard deviations, correlation coefficients were corrected not only for attenuation but also for range restriction, according to the formula proposed by Alexander, Carson, Alliger, and Carr (1987, p. 312).

To test for group-level comparability of the intelligence test scores, *t* tests for dependent samples or Wilcoxon signed-rank tests were calculated (Hypothesis 2). As effect sizes, Cohen's *d* was calculated for *t* tests and *r* for Wilcoxon signed-rank tests. Group-level analyses were two-tailed and

group-level differences were interpreted as substantial if statistical significance was given at $p < .05$ and an effect of $d \geq 0.20$ or $r \geq .10$ was reached.

Following Research Question 1, we explored how comparable IQs obtained with different test batteries are at the individual level. As individual-level score comparability is not a technical concept, there are many ways that scores can be considered comparable. Therefore, we applied five different criteria to define comparability. First, and in line with previous research (Floyd et al., 2005, 2008; Hagmann-von Arx et al., 2018), we considered two IQs of the same individual comparable if the 90% CIs of the two test scores overlapped (Criterion CI90). The maximum difference two IQs could have to be considered comparable on criterion CI90 is indicated as *critical difference*. This critical difference was defined as the sum of half of each test's 90% confidence interval in intelligence test scores.

As a second criterion, we considered two IQs of the same individual comparable if the 95% CIs of the two test scores overlapped (Criterion CI95). This criterion is of high practical relevance, since in practice it is often the 95% CI and not the 90% CI that is interpreted. To be able to compare our results to findings from previous studies (Floyd et al., 2008; Hagmann- von Arx et al., 2018), we plotted the 90% CIs around the obtained test scores using the standard error of the mean. However, following recommendations of Atkinson (1989), most intelligence tests use the estimated true score instead of the obtained test score as well as the standard error of estimation to plot the CI, as is the case for the seven intelligence tests we investigated in this study. This procedure produces asymmetrical CIs that are larger toward the mean, thus largely accounting for the regression toward the mean. Therefore, we adhered to this procedure for the calculation of 95% CIs. Both, the 90% and the 95% CIs were calculated based on statistics reported in the technical test manuals.

As a third criterion, following Floyd et al. (2005, 2008), we assumed comparability if the absolute difference between two IQs of an individual was less than or equal to 10 points (Criterion IQ10). This approach is useful as it uses the same standard to compare all IQ pairs (the critical difference of 10 IQ points is invariant across all test batteries), yet it does not consider the actual reliability of each test score (Floyd et al., 2005).

As fourth and fifth criterion, we calculated the correspondence of the nominal intelligence level (lower extreme IQ: < 70; below average IQ: 70–84; average IQ: 85–115; above average IQ: 116–130; upper extreme IQ: 130; Criterion Nominal IQ) as well as the correspondence of the nominal intelligence levels for the 95% CIs (e.g., average to above average; Criterion Nominal CI95). These nominal criteria are again of special practical relevance as the categorization of scores into average, below or above average are often chosen by practitioners to communicate and explain intelligence test results to laypeople.

Following Research Question 2, we investigated if there was an effect of standardization time (with respect to a possible Flynn or anti-Flynn effect). We therefore calculated the percentage of participants who scored higher on the more recently standardized intelligence test and the percentage

of participants who scored lower on the more recently standardized intelligence test at Criterion CI90 for each pair of intelligence tests.

Following Research Question 3, we examined the individual-level comparability of NI and VI analogous to Research Question 1. Yet we focused on just two of the five comparability criteria, namely the CI95 that considers test reliability as well as the IQ10 that is independent of test reliability and therefore invariant across all test batteries. We then conducted χ^2 tests of proportions to examine whether individual-level comparability was significantly higher or lower in NI and VI versus IQ.

Finally, following Research Question 4, we analyzed whether age, bilingualism, time interval between two tests, order of test administration, and nominal intelligence level significantly predicted the comparability of test results. We therefore ran simple linear regressions with the described variables as independent variables and absolute IQ differences between each test pair as separate dependent variables. A power analysis revealed that to detect large effects ($\beta = .50$) with a power of .80 and an alpha level of .05, a sample size of at least 26 was necessary. We therefore excluded comparisons that were available for less than 26 individuals from the regression analyses, that is, the IDS-2 and WPPSI-III, SB5 and WAIS-III, SON-R 6-40 and WAIS-III, and SON-R 6-40 and WPPSI-III comparisons. Age and time interval between two tests were included as continuous variables and bilingualism and order of test administration as binary variables. In contrast to the Criterion Nominal IQ (as described above), nominal intelligence level was defined here as a three-level categorical variable (i.e., below average IQ: < 85; average IQ: 85–115; above average IQ: > 115) due to small sample sizes at the tails. For each test comparison, a participant was classified as below average or above average if at least one of the two intelligence test scores fell within the defined range. Regression analyses were two-tailed and statistical significance was defined at the .05 alpha level. To counter possible alpha inflation due to multiple testing, all p-values were corrected using the Hommel (1988) correction.

Results

Preliminary Analyses

Table 2 shows the distribution of the intelligence test scores collected in our study. Single-sample t tests revealed that the means of all IQs (M = 102.46–115.29, p = .007 to $p < .001$) were significantly higher than the expected mean (M = 100). Further, the standard deviations of all intelligence tests (SD = 9.64–12.80) except for the SON-R 6-40 (SD = 15.17) were smaller compared to the expected standard deviation (SD = 15).

Table 2. *Descriptive Statistics and One-Sample* t *Tests*

Test	*N*	Minimum	Maximum	*M*	*SD*	Skewness	Kurtosis	*t*/*Z* [a]	*d*/*r* [b]
IDS-2	231	69	133	102.46	12.05	-0.129	-0.001	3.11**	0.20
SB5	202	55	134	102.47	12.80	-0.397	0.721	2.74**	0.19
RIAS	327	76	132	105.03	9.64	-0.057	0.305	9.44***	0.52
SON-R 6-40	269	60	145	113.91	15.17	-0.120	-0.023	15.03***	0.92
WAIS-III	34	77	136	115.29	12.57	-0.685	1.351	7.09***	1.22
WISC-IV	226	60	142	112.49	11.59	-0.711	2.255	11.37***	1.46
WPPSI-III	46	78	134	105.70	12.13	0.028	-0.141	3.19**	0.47

Note. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation.

[a] Comparisons including the WISC-IV were analyzed with the Wilcoxon sign-ranked test (*Z*). [b] Effect sizes including the WISC-IV were indicated as $r = Z/\sqrt{N}$. All other effect sizes are indicated as Cohen's *d*.

**$p < .01$; ***$p < .001$.

Group-Level Comparisons

Table 3 shows the results of all 18 intelligence test comparisons on the group level (Hypotheses 1 and 2). The first of each pair of intelligence tests is the one with the more recent standardization year, except for the IDS-2 and the SB5, which were standardized within the same time period. Sixteen of 18 correlations were significant and ranged from moderate to strong. Only the correlations between the SB5 and the WAIS-III ($r = .38$, $p = .25$) and between the SON-R 6-40 and the WAIS-III ($r = .37$, $p = .08$) were rather weak and did not reach statistical significance. However, as for these pairs of tests, samples sizes were small, the lack of significance could be due to insufficient power. A power analysis with an alpha of .05 and a power of .80 revealed that with a sample size of 11 (as for the SB5 and WAIS-III pair) only correlations larger than or equal to .52 could have been detected.

Further, *t* tests for dependent samples and Wilcoxon tests showed that the means at the group level were substantially lower for the first-listed test with more recent standardization years in 13 of the 18 comparisons (M_{diff} = 2.81 to -13.43, d = -0.27 to -1.29, r = -.23 to -.81). Only regarding the comparisons between the SON-R 6-40 and the WPPSI-III (M_{diff} = 9.28, d = 0.73) was the mean score higher for the SON-R 6-40 with more recent standardization years. The comparisons between the IDS-2 and the SB5, between the SB5 and the RIAS, between the SB5 and the WAIS-III, and between the RIAS and the WPPSI-III did not reveal substantial differences.

Table 3. *Group-Level Comparison of IQs*

Test comparison	Difference in standardization years	*N*	r/ρ [a]	r_{VarRel} [b]	Mean differences (*SD*)	*t*/*Z* [c]	*d*/*r* [d]
IDS-2 and SB5	0	55	.73***	.82***	0.62 (9.90)	0.46	0.05
IDS-2 and RIAS	5	196	.65***	.81***	-3.04 (9.58)	-4.44***	-0.27
IDS-2 and SON-R 6-40	6	154	.67***	.74***	-10.44 (11.02)	-11.75***	-0.77
IDS-2 and WAIS-III	12	30	.46*	.59***	-15.33 (12.39)	-6.78***	-1.29
IDS-2 and WISC-IV	11	113	.65***	.78***	-8.66 (9.31)	-7.52***	-0.71
IDS-2 and WPPSI-III	15	24	.87***	.94***	-4.54 (5.96)	-3.74**	-0.39
SB5 and RIAS	5	167	.60***	.76***	-2.00 (10.42)	-2.48*	-0.17
SB5 and SON-R 6-40	6	139	.71***	.76***	-12.63 (11.09)	-13.43***	-0.87
SB5 and WAIS-III	12	11	.28	.38	-8.55 (12.91)	-2.20	-0.80
SB5 and WISC-IV	11	138	.75***	.84***	-10.32 (7.90)	-9.49***	-0.81
SB5 and WPPSI-III	15	29	.68***	.78***	-4.34 (10.10)	-2.32*	-0.35
RIAS and SON-R 6-40	1	248	.48***	.63***	-8.59 (13.16)	-10.28***	-0.67
RIAS and WAIS-III	7	28	.56**	.74***	-10.43 (11.24)	-4.91***	-0.87
RIAS and WISC-IV	6	199	.66***	.83***	-7.34 (8.59)	-9.32***	-0.66
RIAS and WPPSI-III	10	30	.51**	.71***	1.23 (11.76)	0.57	0.10
SON-R 6-40 and WAIS-III	6	24	.31	.37	-7.00 (15.57)	-2.20*	-0.53
SON-R 6-40 and WISC-IV	5	171	.61***	.70***	2.81 (10.86)	-3.05**	-0.23
SON-R 6-40 and WPPSI-III	9	18	.41	.49*	9.28 (13.79)	2.85*	0.73

Note. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation.

[a] Comparisons including the WISC-IV were analyzed using the nonparametric Spearman rank-order correlation (ρ). All other comparisons were analyzed using the Pearson product-moment correlation (r). [b] Correlations corrected for range restriction and attenuation. [c] Comparisons including the WISC-IV were analyzed with the Wilcoxon test (Z). All other comparisons were analyzed with the dependent t test (t). [d] Effect sizes including the WISC-IV were indicated as $r = Z/\sqrt{N}$. All other effect sizes are indicated as Cohen's d.

*$p < .05$. **$p < .01$. ***$p < .001$.

Individual-Level Comparisons

Table 4 shows the results of the intelligence test comparisons on the individual level (Research Question 1). A total of 1,774 test comparisons were analyzed across all seven intelligence tests and 383 subjects. Since the number of possible comparisons between the different test pairs varied considerably, the total percentage of comparability was calculated across all 1,774 test comparisons. The total comparability regarding the Criteria CI90, IQ10, CI95, Nominal IQ, and Nominal CI95 was 49.8%, 58.1%, 61.9%, 61.9%, and 89.4%, respectively. Regarding Research Question 2, in 39.9% of all cases a lower IQ was achieved in the more recent (first-mentioned) test and in only 9.6% of all cases was a higher IQ achieved in the more recent test. The comparison between the IDS-2 and the SB5 was excluded from this analysis as these tests were standardized within the same time period.

Table 4. *Individual-Level Comparison of IQs*

Test comparison	*N*	M_{diff}	Critical difference [a]	CI90 [b] (%)	Higher intelligence [c] (%)	Lower intelligence [d] (%)	IQ10 [e] (%)	CI95 [f] (%)	Nominal IQ [g] (%)	Nominal CI95 [h] (%)
IDS-2 and SB5	55	7.31	5.94	52.7	30.9	16.4	78.2	61.8	69.1	89.1
IDS-2 and RIAS	196	7.99	8.98	63.8	9.2	27.0	72.4	74.5	76.0	96.9
IDS-2 and SON-R 6-40	154	12.36	8.98	40.3	4.5	55.2	48.7	53.2	55.8	85.1
IDS-2 and WAIS-III	30	15.60	8.40	30.0	0.0	70.0	33.3	33.3	53.3	90.0
IDS-2 and WISC-IV	113	10.26	7.74	43.4	3.5	53.1	59.3	56.6	63.7	92.0
IDS-2 and WPPSI-III	24	6.13	8.98	79.2	4.2	16.6	87.5	91.7	75.0	100.0
SB5 and RIAS	167	8.17	7.96	55.7	16.8	27.5	70.7	67.7	77.2	92.2
SB5 and SON-R 6-40	139	13.78	7.96	33.1	2.9	64.0	41.7	41.7	53.2	79.9
SB5 and WAIS-III	11	11.09	7.38	36.4	9.1	54.5	63.6	45.5	63.6	81.8
SB5 and WISC-IV	138	11.07	6.72	29.0	2.2	68.8	53.6	43.5	55.8	89.9
SB5 and WPPSI-III	29	9.59	7.96	31.0	20.7	48.3	58.6	62.1	65.5	96.6
RIAS and SON-R 6-40	248	12.38	11.00	54.0	7.3	38.7	50.0	64.1	56.5	85.1
RIAS and WAIS-III	28	12.79	10.42	39.3	7.1	53.6	39.3	67.9	46.4	89.3
RIAS and WISC-IV	199	9.44	9.76	55.3	4.0	40.7	59.8	68.8	57.8	93.5
RIAS and WPPSI-III	30	9.03	11.00	76.7	16.7	6.6	70.0	80.0	66.7	93.3
SON-R 6-40 and WAIS-III	24	12.67	10.42	58.3	12.5	29.2	58.3	62.5	50.0	83.3
SON-R 6-40 and WISC-IV	171	9.10	9.76	57.3	28.7	14.0	60.2	71.3	60.8	92.4
SON-R 6-40 and WPPSI-III	18	13.61	11.00	44.4	44.4	11.2	38.9	61.1	55.6	88.9
Total sample	1,774	10.45	8.96	49.8 [i]	9.6 [i]	39.9	58.1	61.9	61.9	89.9

Note. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation; CI = confidence interval.

[a] The sum of half of each test's 90% confidence interval in intelligence test scores. [b] The percentage of participants who reached a difference between each pair of intelligence test scores of less than or equal to the critical difference. [c] The percentage of participants who scored higher on the first-listed and more recently standardized intelligence test of each pair of intelligence tests. [d] The percentage of participants who scored lower on the first-listed and more recently standardized intelligence test of each pair of intelligence tests. [e] The percentage of participants who reached a difference between each pair of intelligence test scores of less than or equal to 10 IQ points. [f] The percentage of participants with overlapping 95% CIs. [g] The percentage of participants who scored on the same qualitative nominal intelligence level in both tests (< 70: lower extreme; 70–84: below average; 85–115: average; 116–130: above average; > 130: upper extreme). [h] The percentage of participants who scored on the same qualitative nominal intelligence level in both tests when considering both qualitative nominal intelligence levels if the 95% CI spanned two levels (e.g., above average to average). [i] This mean percentage was calculated without the comparison of the IDS-2 and SB5, as they were standardized within the same period.

Detailed results on the individual-level comparability of NI and VI (Research Question 3) are reported in Tables A1 and A2. For NI, a total of 1,780 test comparisons were analyzed across all seven test batteries, including the IQ of the SON-R 6- 40, as it is officially classified as NI. For VI, a total of 1,020 test comparisons were analyzed across all tests except the SON-R 6-40, as this test does not provide any verbal index score. Regarding Criterion IQ10, χ^2 tests revealed that with 49.3%, the total comparability of NI was significantly lower than the total comparability of IQ, with 58.1% ($\chi^2 = 27.62$, $p < .001$) and the total comparability of VI with 62.2% was significantly higher than the total comparability of IQ ($\chi^2 = 6.02$, $p < .05$). When considering Criterion CI95, the total comparability of

NI increased to 62.2% and did not differ significantly from the total comparability of IQ, with 61.9% ($\chi^2 = 0.03$, $p > .05$). The total comparability of VI also increased to 68.6% and was significantly higher than the total comparability of IQ ($\chi^2 = 12.60$, $p < .001$).

Predictors of Incomparability

Results of linear regressions with age, bilingualism, time interval between two tests, order of test administration, and nominal intelligence level as predictors of IQ differences (Research Question 4) are reported in Table 5. Age was a significant positive predictor (higher IQ differences for older participants) for only 1 of 14 IQ differences, and a significant negative predictor (lower IQ differences for older participants) for 2 of 14 test comparisons. Bilingualism, time interval between tests, and order of test administration did not significantly predict IQ differences for any of the test comparisons.

Regarding nominal intelligence level, having an IQ in the below-average range in at least one intelligence test was significantly associated with higher IQ differences in six test comparisons. Having an IQ in the above-average range in at least one intelligence test was significantly associated with higher IQ differences in 13 test comparisons (β = .18 to .70).

Table 5. *Linear Regressions With Age, Bilingualism, Time Interval Between Tests, Order of Test Administration, and Nominal Intelligence Level as Predictors of IQ Differences Between Two Intelligence Tests*

	IDS-2 with					SB5 with				RIAS with				SON-R 6-40 with
Test comparison	SB5	RIAS	SON-R 6-40	WAIS-III	WISC-IV	RIAS	SON-R 6-40	WISC-IV	WPPSI-III	SON-R 6-40	WAIS-III	WISC-IV	WPPSI-III	WISC-IV
Age	.20	.01	-.08	.25	.14	-.05	-.16	.26*	-.55*	-.20*	.07	-.10	-.35	-.16
Bilingualism	-.19	-.06	.02	-.29	.01	.16	.16	-.07	.45	.03	-.19	-.04	.04	.18
Time interval between tests	-.06	.07	.07	.20	.19	.13	.14	-.08	.14	-.03	-.02	.13	.09	.05
Order of test administration	.09	.09	.17	.12	.19	.11	.03	-.03	.15	.02	.32	-.02	.13	.05
Nominal intelligence level														
Below average	.45**	.29***	.14	.18	.14	.39***	.26**	.37***	.57**	.02	.39	.06	.06	-.01
Above average	.24	.18*	.50***	.70***	.25*	.31***	.54***	.22*	.46*	.56***	.53*	.41***	.45*	.32***

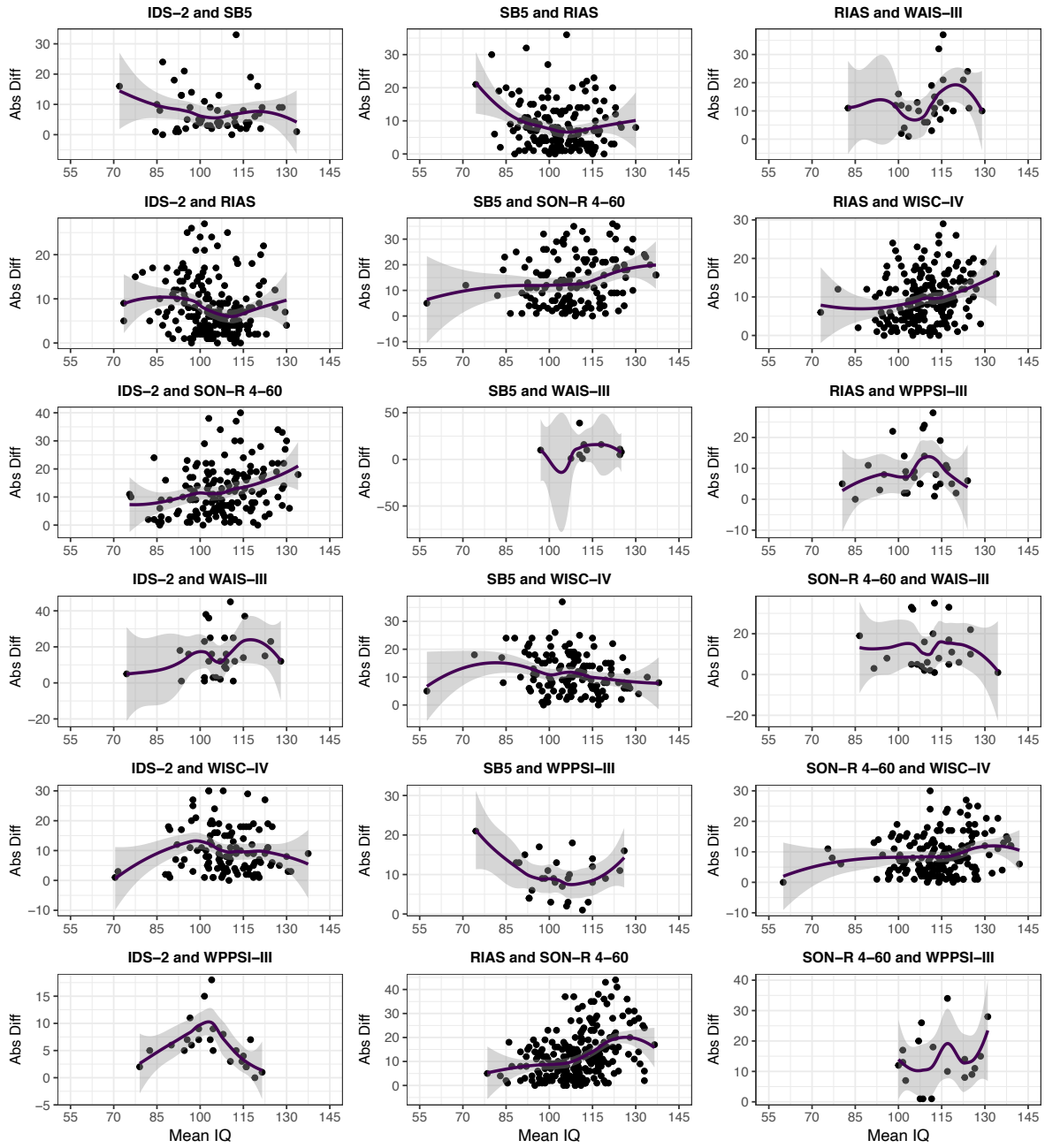
Note. The significance levels reported are corrected for alpha inflation using the Hommel correction. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation. Coefficients are standardized beta regression coefficients. Bilingualism was coded as follows: 1 = female; 0 = male. Order of test administration was coded as follows: 1 = the first listed test was conducted earlier; 0 = the first listed test was conducted later. Nominal level: below average = IQ < 85; above average = IQ > 115; average (IQ 85–115) was the reference category.
*$p < .05$. **$p < .01$. ***$p < .001$.

Post Hoc Analyses

The systematic effect of the nominal intelligence level on IQ differences revealed in the regression analyses leads to the hypothesis that comparability decreases toward the ends of the IQ distribution, the specific ranges in which diagnostic questions most often arise in practice. Yet, due to the particular trichotomization procedure used for the independent variable (nominal IQ levels), results of the regression analyses may potentially be biased towards finding higher IQ differences in the above- and below-average ranges.[1] Therefore, to further scrutinize this hypothesis, we additionally ran post hoc analyses based on continuous data. More specifically, we plotted the mean IQs of two tests (x-axis) for each individual against the absolute difference of the two tests (y-axis) which resulted in a total of 18 plots for all possible test comparisons (single plots, Figure 1). We then fitted local polynomial (loess) regressions to determine the relationships between mean IQ and IQ differences. We thereby assumed that the regression curve increased towards the extreme ends of the IQ distribution if differences are higher in these ranges. Whereas some of the curves indeed indicated greater differences toward the end of the IQ distribution, some curves also showed different patterns. We then additionally combined all the data of the 18 single plots in a combined plot in order to generate a more powerful representation of the effect in question (Figure 2). Whereas in this combined curve greater differences toward the upper bound of the IQ distribution were clearly visible, the curve flattened toward the average and below-average range, indicating that IQ differences in these ranges were of comparable size.

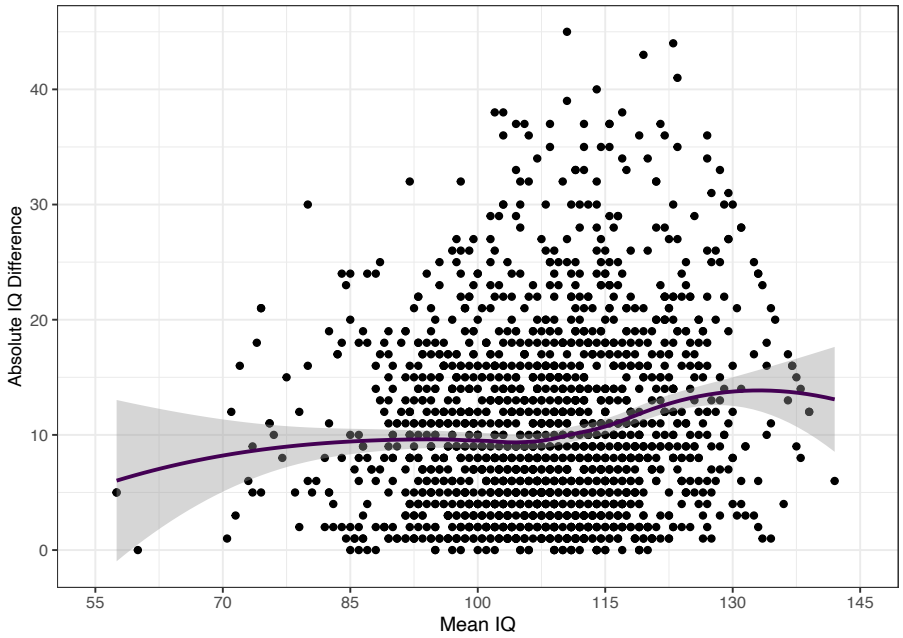
[1] We thank two anonymous reviewers for the fruitful discussions of this issue and possible solutions to it.

Figure 1. *Mean of the Two IQ Scores for Each Individual Plotted Against the Absolute Difference Between the Two IQ Scores*



Note. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III = Wechsler Adult Intelligence Scale–Third Edition, German adaptation; WISC-IV = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; WPPSI-III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation; Abs Diff = absolute difference between the two IQ scores.

Figure 2. *Mean of the Two IQ Scores for Each Individual Plotted Against the Absolute Difference Between the Two IQ Scores: Data From All Test Comparisons Combined*



Note. Each individual is represented here with one data point for each test comparison that was available for this individual (i.e., between one and five data points per individual). Tests included are: Intelligence and Development Scales–2; Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; Reynolds Intellectual Assessment Scales, German adaptation; Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; Wechsler Adult Intelligence Scale–Third Edition, German adaptation; Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation; Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation.

Discussion

The main objective of the present study was to investigate comparability between IQs from seven widely used intelligence tests on the group level and in particular on the individual level. In line with Hypothesis 1, we found mainly substantial correlations between intelligence tests on the group level. Only 2 of 18 correlations were not significant, and these should be interpreted with caution, as sample sizes in these two cases were low. However, contrary to our Hypothesis 2 and to Hagmann-von Arx et al. (2018), we found that in 14 of 18 comparisons, the mean IQ differed significantly between two test scores. Investigating whether IQs obtained from different tests could be classified as comparable (exchangeable) on an individual level, we found that this was true for a relatively low percentage that varied across the different criteria from 49.8% (criterion CI90) to 89.9% (criterion Nominal CI95). Within the percentage of incomparability, tests with earlier standardization years yielded higher IQs than tests with more recent standardization years, which we interpret as an indicator of the Flynn effect (Research Question 2). The pattern of individual-level comparability of nonverbal and verbal factor scores was similar to that found for the IQs (Research Question 3). Beyond that, the nominal intelligence level (below- or above-average vs. average range) was the only systematic predictor of intelligence score differences between tests (Research Question 4). More specifically,

having an IQ in the above-average range was related to greater IQ differences in almost all test comparisons and having an IQ in the below average range was related to greater IQ differences in almost half of all test comparisons. Additional post-hoc analyses based on continuous data confirmed the previous finding of greater IQ differences toward the upper, but not toward the lower bound of the IQ distribution. This finding is in line with Spearman's law of diminishing returns (SLODR; Spearman, 1927) or ability differentiation, which states that the influence of general intelligence on test results decreases with increasing ability level. Consequently, differences in content sampling are expected to have a large influence in these ranges, providing a possible explanation for larger differences for individuals with higher IQs. Yet, few individuals scored in the below-average range in our sample and further research with more children scoring in this range is needed to scrutinize whether comparability is or is not reduced in the below average IQ range, as suggested by our regression analysis results.

Individual-Level Comparisons

Before taking a closer look at the various criteria of comparability on the individual level, we would like to point out that we neither interpret nor compare comparability of specific test combinations (e.g., whether the comparability between the IDS-2 and the RIAS is higher than the comparability between the IDS-2 and the WISC-IV) because sample sizes varied considerably between test comparisons. Instead, we focus on the overall comparability in order to compare and discuss the advantages and disadvantages of the different criteria.

Our results show that comparability was lowest for the criterion CI90, as this interval was narrowest. With a between-test comparability about 50%, using the 90% CI for test interpretation does not seem to be useful. With the criterion proposed, for example, by Bergeron et al. (2008) to consider 5 IQ points above and below the exact point score when diagnosing intellectual ability (criterion IQ10), test comparability was higher, although still quite low with about 58%. Yet, this criterion seems somewhat arbitrary as the considered range of 10 IQ points around the obtained IQ does not consider test-specific unreliability nor the regression toward the mean. CI95 and nominal IQ have revealed the same percentage of comparability with slightly over 60%. The nominal IQ has practical relevance, as it allows for clear classification and may thus be used for diagnosis and classification (e.g., of intellectual disability or intellectual giftedness). However, like the criterion IQ10, the Nominal IQ also disregards the fact that tests might differ with regard to their measurement accuracy. Given that comparability was equally low as in criterion CI95, the CI95 should hence be preferred to the Nominal IQ. Last, the comparability between tests is maximized and approaches 90% when interpreted on the Nominal CI95 criterion. However, one must bear in mind that the IQs of two tests could differ by up to 45 points and still be classified as "comparable" according to this criterion. Given such a wide IQ range, hardly any useful decisions or conclusions can be made. For this reason, and even though this criterion may best represent the extent to which intelligence tests are incomparable, the Nominal CI95 does not provide a diagnostically useful interpretation of test results. Consequently, it seems very difficult to find a criterion that allows for both a reasonably narrow IQ range and a high probability that a

comparable result would be achieved with another intelligence test. Overall, we conclude that for the IQ, the CI95 criterion has the best trade-off between comparability and accuracy. Still, the percentage of comparability is disconcertingly low on this criterion, too.

For the investigation of individual-level comparability of first-order factor scores, our results show that the comparability of NIs is lower compared to the comparability of IQs when reliability is *not* considered (criterion IQ10) and that the comparability of NIs does not differ from the comparability of IQs when reliability is considered (criterion CI95). However, the comparability of VIs was—although still quite low—significantly higher than that of IQs on both criteria, IQ10 and CI95. If the comparability of VI was only higher on criterion CI95, one might assume that this is explained by the fact that VI has generally slightly lower reliability coefficients, what widens their CIs resulting in a higher chance for overlap. Yet, this assumption is rejected by the finding that comparability is also higher on criterion IQ10 which is independent of reliability and therefore invariant across all comparisons. A possible explanation for the higher comparability of VIs could therefore be that there is greater content overlap between the subtests of the different test batteries that constitute the VI compared to the NI and IQ. Although content overlap should be neglectable for the measurement of *g* due to the principle of aggregation (Jensen & Weng, 1994; Rushton et al., 1983), it does play an important role when the number of subtests is lower (Farmer et al., 2020), as it is in VI and NI. This could now lead to the conclusion that the VI is to be preferred to the IQ as an intelligence estimate. However, there is a large body of research with at least two widely supported counterarguments: First, IQ is a more accurate representation of *g* and first-order factor scores are usually less reliable than the IQ (Jensen, 1981, 1998; Kranzler & Floyd, 2013). In this sense, the finding that comparability is higher for VI than for IQ does not make VI a better representation of *g*. Second, factor scores contain only a little unique variance compared to the IQ (Canivez & Youngstrom, 2019; McGill et al., 2018). Consequently, we do not know to what extent this comparability is caused by shared variance of *g* and to what extent it is caused by more specific verbal reasoning. Furthermore, individual-level comparability is only slightly higher for VI compared to IQ and our results indicate that cognitive profile scores cannot be reliably replicated across test batteries. Therefore, we recommend interpreting NI as well as VI only with extreme caution, if at all, although it is a common practice among school psychologists (e.g., McGill et al., 2018). This goes in line with previous literature that heavily questions the usefulness of any kind of cognitive profile interpretation (e.g., to identify patterns of specific learning disorders) for school psychology practice (e.g., McGill et al., 2018).

Given the high relevance of having comparable IQs across intelligence tests, it is important to discuss potential factors that might have reduced comparability. Some of these, that were also included in the regression analyses, are characteristics of the testee, such as age or bilingualism, which were both not systematically associated with IQ differences. Other possible explanations concern characteristics of the tests themselves, such as their overlap in content (see above) or the presence of naming fallacies (i.e., subtests from different tests purported to measure the same constructs, when they in fact measure

different constructs, or vice versa; Thorndike, 1904). Moreover, consequences of multiple testing, such as the time interval between tests as well as the order of test administration might also systematically influence IQ differences due to potential effects of temporal stability (see discussion on transient effects below) and practice effects, respectively. However, we found no support for these influences in our regression analyses. As stated above, it is likely that other variables not considered here influenced the comparability, for example, differences in the standardization samples between the test batteries (e.g., over- or undersampling of individuals with particularly high or low ability). Either way, it needs to be discussed what practitioners as well as test developers can do to deal with potential sources of incomparability.

Implications for the Application and Construction of Intelligence Tests

A common recommendation to deal with this comparability issue is to apply more than one intelligence test, especially for high-stakes decisions. From a pure statistical perspective, this recommendation is reasonable as the reliability of a score increases when enhancing the number of items and subtests that measure the same construct (i.e., intelligence). In line with this, previous results from generalizability theory analyses showed that up to five intelligence tests need to be applied to achieve a reliability of at least .90 (Hagmann-von Arx et al., 2018). As this is obviously not feasible in practice, the authors suggested using at least two tests for high-stakes decisions. Yet, whereas the interpretation of a test result and subsequent diagnoses and decisions are strengthened if IQs obtained in two different tests are comparable, the question of what to do if they lie far apart remains unanswered. Whereas it seems to be common practice to average composite scores from different tests to gain a more reliable and valid estimation of intelligence, we do not support this practice as it may cause additional measurement error and biases the averaged score toward the mean (Schneider & McGrew, 2011). Moreover, even if this bias toward the mean were considered, there are numerous reasons for possible underachievement (e.g., fatigue, sleep quality, motivational factors, test anxiety, environmental distractions, etc.) but rather few reasons for possible overachievement (e.g., guessing, learning effects, inappropriate help from the examiner) on intelligence tests. Measurement error can therefore not be averaged arithmetically. Furthermore, as stated above, differences in content between the tests might also cause systematic differences in global IQ scores in interaction with characteristics of the testee (e.g., Irby & Floyd, 2017). Instead of averaging IQs across tests, it may be tempting for practitioners to consider the more extreme of two scores to set a diagnosis (if only one of the two reaches the cut-off), especially when the presence of a diagnosis is associated with financial and educational resource allocation. Yet, especially in the below-average range, we do not recommend this practice either and certainly not without checking for a potential underachievement, as the drawbacks of a false diagnosis of intellectual disability (e.g., stigmatization, Golem effect) may exceed the positive effect of educational support. Overall, the practical benefit of applying two or more intelligence tests seems to be somewhat limited as long as there is no coherent evidence-based strategy of interpreting scores that diverge widely. What we do recommend is, as already proposed in diagnostic guidelines and previous

studies, to only interpret scores with adequate reliability and with evidence-based support of validity and utility (e.g., American Educational Research Association et al., 2014; McGill et al., 2018). Furthermore, we recommend to consider anamnestic information such as language and cultural background and personal characteristic (e.g., interaction difficulties) as well as environmental conditions to determine if a child's and adolescent's abilities are accurately measured (American Psychological Association, 2017; International Test Commission, 2001; National Research Council, 2002). This might be a necessary step both prior to test administration for an adaptive test selection as well as after test administration for a careful test interpretation. Investigating some of these possible influences (i.e., bilingualism, age, and test interval) on test comparability, we could not detect any systematic effect. However, it is possible that additional factors that have not been examined in this study may have impacted test comparability on the individual level. Further, it is also possible that some potential factors influenced test comparability for certain individuals under certain circumstances, but not for others. If this was the case, we must assume that incomparability and measurement inaccuracy is increased by random variations (e.g., a temporary state of an individual or random circumstances) in addition to systematic effects. This type of error variance is often called transient error (Schmidt, Le, & Ilies, 2003). The National Research Council (2002) provided a comprehensive list of possible systematic and transient error sources (threats) including characteristics of the examinee, examiner's influence, and environmental influences as well as psychometric test characteristics and stated that "all of them can be controlled to some considerable degree" (p. 100). Nevertheless, we note that this is a considerable challenge for practitioners, not only because of the multitude of potential threats but also because of time pressure, that often plays a crucial role in the diagnostic process. Our results show the extent of incomparability across test scores when potential transient error sources are not considered. With this in mind, our results indicate how prone intelligence test scores are to interference, and how high the risk of misdiagnosis may be if the diagnostic process is not carried out with the utmost thoroughness. It thus seems important to explore what not only test administrators but also test developers can do to reduce potential sources of incomparability.

In this context, we consider it important to discuss the use of internal consistency coefficients for the calculation of CIs. This practice should be reconsidered for mainly two reasons: First, the specific internal consistency coefficient used for the calculation of CIs differs among tests (i.e., Cronbach's α or split-half reliability), which influences the width of the CI and with that also the comparability between tests. Second, internal consistency coefficients do not take into account the previously described transient errors. Yet, ignoring such transient errors leads to an overestimation of the reliability (Schmidt et al., 2003). Thus, it is important to investigate whether other reliability coefficients—such as the test–retest reliability that does consider transient errors—would provide an alternative and more accurate estimate and basis for the calculation of CIs. Nevertheless, this does not solve the problem of comparability seeming to be especially low in the above-average intelligence range (and potentially also in the below-average range). If this finding is replicated in future research, it will

be of interest to explore how and to what extent the IQ level should be considered within the calculation of reliability coefficients and CIs. Our suggestion is that technical test manuals should provide conditional (e.g., IQ-level specific) reliability coefficients and standard errors. For if measurement error is generally higher in the vicinity of cut-off scores, the risk for error of classification and misdiagnosis increases, too (American Educational Research Association et al., 2014). This potential risk and its implications must be made visible, not only in research articles but also test manuals that provide recommendations on test interpretation. This will allow practitioners to engage in an informed discussion about the utility of rigid versus flexible cut-off scores when making high-stakes decisions.

Generally, it is essential to ensure that comparability of intelligence test results is not reduced by sampling error, and that test developers proceed according to the current state of research. This includes making sure that the IQ is based on an adequate number of subtests (see Farmer et al., 2019), that the IQ consists of a heterogeneous set of subtests in order to average out measurement error, and that only subtests with a high *g* loading are included (Farmer et al., 2020; Jensen & Weng, 1994). Further research is needed to investigate whether and how individual-level comparability might be enhanced with an optimal balance of these three aspects.

Finally, practitioners as well as test developers should remember that simply putting the results from two tests purported to measure the same construct on the same scale by standardization does not necessarily mean that their scores will be interchangeable. Test developers should consider applying and communicating explicit score equating practices (see, for example, Dorans & Holland, 2000; Holland, 2007; Livingston, 2014), and assure that the prerequisites for score equating are met (see Dorans & Holland, 2000). Only if the prerequisites of score equation are met and if scores are equated explicitly (using a specific linking function), then the expectation of interchangeable scores many practitioners and researchers already hold may be really justified.

Limitations

Our study has several limitations. First, mean IQs of our sample were higher than the standardization means. This bias may have been caused by participants being allowed to decide whether they wanted to do more tests after having taken either the IDS-2 or the SB5. It is likely that participants with lower results on one of the first tests had less joy and were therefore less motivated to participate in further tests. If it is true that comparability decreases toward the upper end of the IQ distribution, this circumstance may limit the generalizability of our findings in a way that overall comparability might be higher in a sample with mean IQs closer to the standardization mean. Moreover, the relatively low number of individuals with below-average intelligence also prevented us from drawing reliable conclusions regarding comparability in this particular intelligence range. Second, our sample sizes varied considerably between test comparisons. We therefore did not explicitly compare or interpret comparability of specific test combinations. Third, we had relatively small sample sizes for some comparisons, and especially the youngest (4–6 years) and oldest (16–20 years) participants. This has reduced the power to detect small and medium effects for the group- level correlation analyses as well

as for the regression analyses, especially those with age as a possible predictor of IQ differences between tests. It also led to the exclusion of half of the comparisons involving the WPPSI-III and WAIS-III. For the regression analyses in particular, all comparisons with sample sizes smaller than 82 (that is, 5 of the 14 comparisons for which regression analyses were performed) would have required at least medium effect sizes ($\beta = .30$) to be detected with an alpha of .05 and a power of .80, and were therefore likely underpowered for some predictors. Further research including larger samples and more individuals in all age ranges might shed further light on the possible impact of age and other predictors on test score comparability. Fourth, the combination of all test comparisons in one single plot and loess regression analysis for the investigation of the relationship between individual mean IQ and IQ differences (Figure 2) might be biased due to an enmeshment of within- and between-individual data (each individual contributed one to five data points) and of different test comparisons that might differ systematically (e.g., two tests with lower content overlap might result in higher differences in general). Finally, information about mono- or bilingualism was collected via self- or parent report. An examination of actual language skills by using performance-based tests could provide valuable information on whether and to what extent they influence test comparability.

Conclusion

We conclude that the interpretation of an exact IQ from a single intelligence test does not hold empirically, which strongly questions the use of rigid cut-off values for diagnosis and related educational resource allocation and favors the use of flexible cut-offs that consider the test-specific measurement error. At least the 95% CI must be considered when making high- stakes decisions. Even then, and especially in the upper (and probably also in the lower) bounds of the IQ scores, there is a considerable risk that diagnosis and resource allocation for special education will be based largely on test-specific characteristics and measurement error and less on the individual's true ability. To reduce this risk and to enhance between-test comparability, diagnosticians are encouraged to consider possible factors that interfere with an individual's ability. Hence, test administrators need to be well trained and informed about the limitations of conventional intelligence tests. Further, our results point to the fact that current intelligence tests may have relied too much on results derived from group-level analyses while excluding individual-level analyses to determine psychometric test quality. Future research is needed to generate effective approaches to enhancing accuracy and with this also individual-level comparability between tests.

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Appendix

Table A1. *Individual-Level Comparison of Nonverbal Factor Scores*

Test comparison	N	M_{diff}	SD_{diff}	$Diff_{min}$	$Diff_{max}$	IQ10 [a] (%)	CI95 [b] (%)
IDS-2_{AR} and $SB5_{NI}$	55	11.9	9.5	0	38	52.7	58.2
IDS-2_{AR} and $RIAS_{NI}$	196	11.0	8.1	0	46	53.1	72.4
IDS-2_{AR} and SON-R 6-40	154	13.0	10.3	0	46	50.0	59.7
IDS-2_{AR} and WAIS-III_{PI}	30	19.7	12.1	2	44	26.7	33.3
IDS-2_{AR} and WISC-IV_{PR}	114	14.3	10.2	0	46	43.9	58.8
IDS-2_{AR} and WPPSI-III_{PI}	24	14.3	9.4	1	34	41.7	41.7
$SB5_{NI}$ and $RIAS_{NI}$	168	11.4	9.1	0	51	56.5	63.1
$SB5_{NI}$ and SON-R 6-40	140	14.6	9.8	0	47	37.9	40.0
$SB5_{NI}$ and WAIS-III_{PI}	11	12.5	14.6	1	46	63.6	63.6
$SB5_{NI}$ and WISC-IV_{PR}	139	15.4	10.1	0	43	36.7	42.4
$SB5_{NI}$ and WPPSI-III_{PI}	29	11.5	10.1	0	39	55.2	58.6
$RIAS_{NI}$ and SON-R 6-40	248	13.1	10.0	0	53	49.6	64.1
$RIAS_{NI}$ and WAIS-III_{PI}	28	15.4	10.9	0	47	39.3	60.7
$RIAS_{NI}$ and WISC-IV_{PR}	200	12.2	8.8	0	42	48.0	71.5
$RIAS_{NI}$ and WPPSI-III_{PI}	30	11.6	8.9	1	33	60.0	66.7
SON-R 6-40 and WAIS-III_{PI}	24	11.9	12.0	1	43	66.7	66.7
SON-R 6-40 and WISC-IV_{PR}	172	9.0	6.3	0	33	61.0	83.1
SON-R 6-40 and WPPSI-III_{PI}	18	11.9	8.5	1	28	44.4	66.7
Total sample	1,780	12.61	9.29	0	53	49.3	62.2

Note. IDS-2_{AR} = Intelligence and Development Scales–2, abstract reasoning; $SB5_{NI}$ = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation, nonverbal intelligence; $RIAS_{NI}$ = Reynolds Intellectual Assessment Scales, German adaptation, nonverbal intelligence; SON-R 6-40 = Snijders–Oomen Nonverbal Intelligence Test 6-40, German adaptation; WAIS-III_{PI} = Wechsler Adult Intelligence Scale, German adaptation, performance intelligence; WISC-IV_{PR} = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation, perceptual reasoning; WPPSI-III_{PI} = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation, performance intelligence; CI = confidence interval.

[a] The percentage of sample participants who reached a difference between each pair of intelligence test scores of less than or equal to 10 IQ points. [b] The percentage of sample participants with overlapping 95% confidence intervals.

Table A2. *Individual-Level Comparison of Verbal Factor Scores*

Test comparison	*N*	M_{diff}	SD_{diff}	$Diff_{min}$	$Diff_{max}$	IQ10 [a] (%)	CI95 [b] (%)
IDS-2_{VR} and $SB5_{VI}$	55	8.42	7.09	0	32	70.9	61.8
IDS-2_{VR} and $RIAS_{VI}$	196	8.41	6.63	0	30	69.4	79.1
IDS-2_{VR} and WAIS-III_{VI}	30	13.17	7.41	0	29	43.3	53.3
IDS-2_{VR} and WISC-IV_{VC}	113	10.60	8.41	0	45	56.6	64.6
IDS-2_{VR} and WPPSI-III_{VI}	24	7.50	7.33	1	27	83.3	83.3
$SB5_{VI}$ and $RIAS_{VI}$	167	8.72	6.21	0	27	66.5	66.5
$SB5_{VI}$ and WAIS-III_{VI}	11	11.64	7.24	0	25	45.5	45.5
$SB5_{VI}$ and WISC-IV_{VC}	138	11.37	8.25	0	42	53.6	53.6
$SB5_{VI}$ and WPPSI-III_{VI}	29	9.07	6.09	0	22	58.6	62.1
$RIAS_{VI}$ and WAIS-III_{VI}	28	9.86	6.46	1	25	60.7	71.4
$RIAS_{VI}$ and WISC-IV_{VC}	199	9.82	8.12	0	46	62.3	73.9
$RIAS_{VI}$ and WPPSI-III_{VI}	30	8.10	5.67	0	24	70.0	90.0
Total sample	1,020	9.58	7.29	0	46	62.8	68.6

Note. IDS-2_{VR} = Intelligence and Development Scales–2, verbal reasoning; $SB5_{VI}$ = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation, verbal intelligence; $RIAS_{VI}$ = Reynolds Intellectual Assessment Scales, German adaptation, verbal intelligence; WAIS-III_{VI} = Wechsler Adult Intelligence Scale, German adaptation, verbal intelligence; WISC-IV_{VC} = Wechsler Intelligence Scales for Children–Fourth Edition, German adaptation, verbal comprehension; WPPSI-III_{VI} = Wechsler Preschool and Primary Scale of Intelligence–Third Edition, German adaptation, verbal intelligence; CI = confidence interval.

[a] The percentage of sample participants who reached a difference between each pair of intelligence test scores of less than or equal to 10 IQ points. [b] The percentage of sample participants with overlapping 95% confidence intervals.

APPENDIX E: Study 5

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Limited Internal Comparability of General Intelligence Composites: Impact on External Validity, Possible Predictors, and Practical Remedies

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Silvia Grieder provided conceptualization, methodology, formal analysis, writing–original draft, writing–review and editing, and visualization; Anette Bünger provided conceptualization and writing–review and editing; Salome D. Odermatt provided writing–review and editing; Florine Schweizer provided writing–review and editing; Alexander Grob provided conceptualization, resources, writing–review and editing, and supervision. We have no conflicts of interest to declare. We thank Anita Todd for copy editing.

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Abstract

Research on comparability of general intelligence composites (GICs) is scarce and has focused exclusively on comparing GICs from different test batteries, revealing limited individual-level comparability. We add to these findings, investigating the group- and individual-level comparability of different GICs within test batteries (i.e., internal score comparability), thereby minimizing transient error and ruling out between-battery variance completely. We (a) determined the magnitude of intraindividual IQ differences, (b) investigated their impact on external validity, (c) explored possible predictors for these differences, and (d) examined ways to deal with incomparability. Results are based on the standardization samples of three intelligence test batteries, spanning from early childhood to late adulthood. Despite high group-level comparability, individual-level comparability was often unsatisfactory, especially towards the tails of the IQ distribution. This limited comparability has consequences for external validity, as GICs were differentially related to and often less predictive for school grades for individuals with high IQ differences. Of several predictors, only IQ level and age were systematically related to comparability. Consequently, findings challenge the use of overall internal consistencies for CIs and suggest using CIs based on test–retest reliabilities or age- and IQ-specific internal consistencies for clinical interpretation. Implications for test construction and application are discussed.

Keywords: general intelligence, IQ, screening, individual level, reliability, validity

Introduction

General intelligence is defined as the broad mental capacity to reason, solve problems, comprehend complex ideas, and learn quickly (Gottfredson, 1997). It predicts numerous important life outcomes, including academic achievement (Lubinski, 2004; Roth et al., 2015), occupational success, socioeconomic status, income (Batty et al., 2009; Gottfredson, 2004; Lubinski, 2004), health, and longevity (Batty et al., 2009; Gottfredson & Deary, 2004).

The concept of general intelligence was first introduced by Charles Spearman as a common factor explaining the positive manifold of cognitive test outcomes—psychometric *g* (Spearman, 1904). Since Spearman, research on intelligence structure has moved to hierarchical models, but the majority of these models still includes a general intelligence factor. The currently perhaps most influential intelligence model, the Cattell–Horn–Carroll (CHC) model (McGrew, 1997, 2009; Schneider & McGrew, 2018), assumes a three-stratum structure with narrow abilities at the bottom that are indicators of broad abilities (such as fluid reasoning, comprehension–knowledge, perceptual speed), which are in turn influenced by a general factor. Although the existence of a general factor is open to debate in the CHC taxonomy (e.g., McGrew, 2009), virtually all intelligence tests whose development was based on the CHC model—and almost all intelligence tests in general—include a Full-Scale IQ (FSIQ) as an indicator of general intelligence that typically is a composite score of many diverse or of all subtests from a test battery. To avoid an intertwining of the theoretical construct of general intelligence and its measurement, we refer to the theoretical construct as *general intelligence*, to the latent measure of general intelligence as *general factor*, and to a (unit-weighted) subtest composite intended to measure general intelligence as *general intelligence composite (GIC)*.

As most intelligence tests include a GIC, a major question in test construction concerns the determinants of a reliable and valid measurement of general intelligence. A recent study by Farmer et al. (2020) investigated such determinants by comparing the reliability and accuracy of different intelligence composites from two test batteries. They systematically varied the heterogeneity, general factor loadings (both separately and in combination), and number of subtests and found that, as a single criterion, high general factor loadings were more important than heterogeneity for an accurate composite. The most accurate composites were those derived from numerous (12 to 13) diverse subtests with high general factor loadings, but the gains in reliability and accuracy began to flatten out from about four subtests on. Yet, as the authors pointed out, small gains in reliability are of practical relevance, as they can have substantial effects on confidence intervals (CIs) and hence on comparability on an individual level (i.e., overlap of CIs). It is therefore important to investigate individual-level in addition to group-level comparability to learn more about the accuracy of different composites.

There are different kinds of score comparability or score linking, all of which technically require the application of a specific linking function (Dorans & Holland, 2000; Holland, 2007). However, the composite scores from intelligence tests are seldomly linked directly using an explicit linking function. Rather, the different composites are standardized separately and it is presumed that

different composites intended to measure the same construct, for example, general intelligence, will be *equal* or *exchangeable*. At least this is how these scores are applied in practice, where often one test is selected from a variety of different tests purporting to measure the same construct(s) and the resulting test score is interpreted as if it would have been the same (or at least very similar, considering measurement error) on any of the other tests. However, for scores to be regarded equal, at least five requirements have to be fulfilled that not necessarily hold for different GICs: (a) the same construct requirement, (b) the equal reliability requirement, (c) the symmetry requirement, (d) the equity requirement, and (e) the population invariance requirement (Dorans & Holland, 2000).

The same construct requirement holds that two tests need to measure the same theoretical construct, which requires this to be carefully defined based on a sound theory providing clear guidance for test development at the item level (Beaujean & Benson, 2019; Maul et al., 2019). We would assume that most concurrent intelligence tests will fail this requirement, at least for GICs. The equal reliability requirement is also often violated, especially when comparing scales of different length, but violations of this requirement are less important if reliabilities are high (Dorans & Holland, 2000). For intelligence tests, internal consistencies are usually very high and unreliability is often addressed using CIs. However, the question remains whether internal consistencies sufficiently capture the tests' measurement error (see below). The symmetry requirement is usually met by definition (Dorans & Holland, 2000) and therefore not of interest for our study. The equity requirement concerns the exchangeability of test results and holds that an individual test outcome should be the same no matter which of the compared tests is used. It is this requirement that prior studies on individual-level comparability of GICs were most concerned with and that we mostly focus on in our study as well. Finally, the population invariance requirement holds that the compared scores should be equally comparable across all different (sub-)populations the tests are intended for use. Violations of this requirement can be indicative of violations of the same construct and/or the equal reliability requirements (Dorans & Holland, 2000). It is tested by comparing the comparability of test results across specific subgroups of the whole population, which is what we did in the present study. To clarify whether it is justified to regard different GICs to be equal in the sense of Dorans and Holland (2000)—henceforth called *comparable*—it is thus important to investigate the degree to which the aforementioned requirements are fulfilled for these GICs.

The few studies we know of that dealt with the comparability of GICs in this sense performed individual-level comparisons between GICs derived from different test batteries (Bünger et al., 2021; Floyd et al., 2008; Hagmann-von Arx et al., 2018). These revealed substantial intraindividual absolute differences in GICs on an IQ scale—henceforth called IQ differences—and limited comparability of CIs and IQs in nominal categories. All three of the aforementioned studies concluded that any two intelligence tests do not necessarily render comparable FSIQs on the individual level, even if they show high correlations and no mean differences on the group level. Results from all three studies thus indicate violations of the equity requirement.

We add to these previous findings with the present study, in which we investigated the individual-level internal comparability of different GICs, that is, of different composites derived from the *same* test battery that are all intended to measure general intelligence. Proceeding this way, transient error (i.e., error due to variations in mood, information-processing efficiency, etc. over time; see Schmidt et al., 2003) as well as differences in examiner influences are kept to a minimum as all scores stem from a single test session, and between-battery variance (i.e., the standardization sample and differences in global test characteristic, such as general instructions, type of presentation) is held constant. Internal comparability analyses also have the advantage that they practically eliminate carryover effects, that is, the influence of practice effects on scores on a second test if this includes very similar tasks to the first test. For the purpose of internal comparability analyses, the comparison between the FSIQ and an Abbreviated Battery IQ (ABIQ) from the same test battery is well suited. While the FSIQ is typically based on many or all subtests, an ABIQ is based on a subset of subtests and is thus typically less reliable than the FSIQ and intended as a screening. After practitioners have administered an ABIQ, their decision as to whether the rest of the test battery will also be administered is often based on the screening results (Thompson et al., 2004). It is therefore important to investigate the individual-level comparability of FSIQ and ABIQ.

In the present study, we examined this internal comparability of GICs—mainly FSIQ and ABIQ—in four steps. First, we determined the magnitude of intraindividual IQ differences between the GICs; second, we investigated the impact of these differences by comparing the GICs' external validity; third, we examined possible predictors of these differences; and fourth, we sought ways to deal with incomparability.

Given imperfect reliability and results from Floyd et al. (2008), Hagmann-von Arx et al. (2018), and Bünger et al. (2021), we expected to find at least some IQ differences. To examine the possible impact of such differences on external validity, we determined the GICs' differential relationships with school grades. As general intelligence measures are strong predictors of scholastic achievement and academic success (Deary et al., 2007; Gygi et al., 2017; Roth et al., 2015; Watkins et al., 2007), these criteria are typically used for external validation of intelligence tests. In our study, we focused not on the absolute magnitude of relationships between GICs and school grades but rather on possible differences in magnitude of relationships between the FSIQ and school grades and the ABIQ and school grades. While the former has been studied extensively (see above), to our knowledge, effect sizes of the FSIQ and the ABIQ have never been compared explicitly, which is what we did in the present study.

After having determined the magnitude and impact of IQ differences, we were interested in possible predictors of these. Results from previous studies suggest that most of the error variance in IQs is systematic (Irby & Floyd, 2016, 2017). To learn more about the sources of systematic variation in IQ differences, we explored several possible predictors of IQ differences. These include variables already considered in previous studies (Bünger et al., 2021; Hagmann-von Arx et al., 2018), such as IQ level and age, as well as other, not yet examined characteristics of the testee and their behavior in the test

situation. If characteristics of the testee should explain some variation in IQ differences, this would indicate a failure of the population invariance requirement. Characteristics of the composite, such as the number, general factor loadings, and content of subtests involved, might also predict IQ differences, as these characteristics influence the accuracy of composites (see Farmer et al., 2020). Because these characteristics are invariant between individuals, inclusion in quantitative analyses was not possible here. Hence, we address them in a descriptive manner only.

As a last step, we explored possible ways to deal with incomparability. To this end, we examined alternative ways of describing intelligence test results other than exact IQ scores, aiming to achieve more reliable and stable estimates that may meet the equality requirements to a greater extent. Obvious candidates that were also examined in previous studies (Bünger et al., 2021; Floyd et al., 2008; Hagmann-von Arx et al., 2018) are CIs and nominal categories (e.g., "average" for an IQ between 85 and 115). In all three studies mentioned above, however, CIs were computed solely on the basis of an overall internal consistency, which reflects the most common use in practice. Results from these studies suggest that using such CIs still does not lead to satisfactory comparability. As an extension to these previous studies, we therefore varied the reliability coefficients used for the calculation of CIs. It is known that test–retest reliability tends to be lower at younger ages (Watkins & Smith, 2013) and toward the tails of the IQ distribution (due to regression to the mean; Campbell & Kenny, 1999). Considering floor and ceiling effects, the same might also be true for internal consistency. If this was the case, using CIs based on separate internal consistency coefficients for age and IQ groups—henceforth called age- and IQ-specific internal consistencies—should lead to higher rates of comparability between IQs compared to using CIs based on the same overall internal consistency for all participants. This assumption is supported by results from Bünger et al. (2021), who found that IQ level was a significant predictor of IQ differences. A possible influence of age on comparability was not investigated by Floyd et al. (2008) and Hagmann-von Arx et al. (2018), and age was no systematic predictor for IQ differences in regression analyses reported in Bünger et al. (2021). However, as Bünger et al. (2021) concluded, further analyses with larger age groups are warranted to learn more about IQ comparability across age, which was possible in the present study. Hence, we investigated comparability across IQ level and age for all criteria, and we examined comparability for CIs based on age- and IQ-specific internal consistencies. Moreover, using internal consistency as a reliability estimate bears the danger of overestimating reliability, as it misses transient error (see above; Schmidt et al., 2003). This source of error is, however, assessed in test–retest reliability, which is why we also considered CIs based on test–retest reliability coefficients in our study.

Present Study

The primary objective of this study was to investigate the individual-level internal comparability of different GICs. For this purpose, we compared GICs (mostly FSIQ vs. ABIQ) derived from the same test battery for participants from the standardization samples of three test batteries, spanning from early childhood to late adulthood: The *Intelligence and Development Scales–2* (IDS-2;

Grob & Hagmann-von Arx, 2018a), and the German adaptations of the *Stanford–Binet Intelligence Scales–Fifth Edition* (SB5; Grob et al., 2019b) and the *Reynolds Intellectual Assessment Scales* (RIAS; Hagmann-von Arx & Grob, 2014a). Since comparisons of GICs from different intelligence tests were not an aim of this study, we exclusively compared GICs *within* these test batteries. To learn more about the impact of, possible predictors for, and ways to deal with incomparability, secondary objectives of this study were to examine the differential external validity of GICs, to identify predictors of IQ differences, and to see if individual-level comparability could be enhanced by varying reliability coefficients for the calculation of 95% CIs.

We addressed the following hypotheses and research questions: First, we expected that (a) the GICs for each test battery would be highly intercorrelated, and (b) there would be no significant mean differences between GICs. Second, we examined the magnitude of intraindividual differences in IQ points (both overall and across IQ level and age). Third, we hypothesized that relationships of school grades with the ABIQ would be smaller compared to those with the FSIQ. Fourth, we examined whether certain characteristics of the testee (e.g., age) or their behavior in the test situation (e.g., understanding of instructions) were associated with IQ differences. Finally, we examined how many participants would achieve comparable intelligence estimates (again both overall and across IQ level and age) determined with different criteria (i.e., different 95% CIs and nominal categories). We expected higher comparability for CIs based on age- and IQ-specific internal consistencies and test–retest reliabilities compared to CIs based on one overall internal consistency coefficient. Supplementary material to this study, including analysis scripts, is available at https://osf.io/hfqe5/.

Method

Participants

The IDS-2 standardization sample consists of 1,672 participants from Switzerland, Germany, and Austria. Complete data on all GICs were available for 1,622 participants (50.9% girls and women; age in years: M = 12.06, SD = 4.40, range: 5.02–20.97). About one third (31.4%) of participants' mothers had a university degree, 16.5% of participants were bilingual (German and at least one other native language), 7.6% were nonnative speakers (German not their native language), and 3.4% reported having an attention-deficit/hyperactivity disorder (ADHD) or attention-deficit disorder (ADD) diagnosis (hereafter called AD[H]D). For a subsample of 414 individuals (50.7% girls and women; age in years: M = 12.07, SD = 2.59, range: 5.42–19.37), there were additional cross-sectional data on school grades.

The SB5 standardization sample consists of 1,829 participants from Switzerland, Germany, Austria, and Liechtenstein. Complete data on all GICs were available for all 1,829 participants (51.4% girls and women; age in years: M = 23.46, SD = 20.02, range: 4.00–83.96). Around one third (29.4%) of participants—or for children and adolescents, their mothers—had a university degree, 8.8% of participants were bilingual (German and at least one other native language), 7.8% were nonnative

speakers (German not their native language), and 2.9% reported having an AD(H)D diagnosis. For a subsample of 249 individuals (47.4% girls; age in years: *M* = 11.31, *SD* = 2.38, range: 5.79–17.68), there were additional cross-sectional data on school grades.

The RIAS standardization sample consists of 2,145 participants from Switzerland and Germany. Complete data on all GICs were available for 2,109 participants (49.5% girls and women; age in years: *M* = 19.84, *SD* = 20.28, range: 3.00–99.96). About one fifth (20.7%) of participants—or for children and adolescents, their mothers—had a university degree, and 17.9% of participants were nonnative speakers (German not their native language). For a subsample of 64 individuals, there were additional data on school grades collected 2 to 4 years after the intelligence assessment (51.6% girls; age in years at T1: *M* = 9.02, *SD* = 1.02, range: 6.07–11.22, and at T2: *M* = 11.41, *SD* = 0.99, range: 9.00–14.00).

Materials

Intelligence Test Batteries

The IDS-2 assess cognitive (intelligence, executive functions) as well as developmental (psychomotor skills, socioemotional skills, basic skills, and motivation and attitude) in 5- to 20-year-olds with a total of 30 subtests (Grob & Hagmann-von Arx, 2018a; see Table S1 for descriptions). The IDS-2 allow for the estimation of three different GICs. The Profile IQ (an Extended Battery IQ, henceforth called IDS-$2_{EBIQ(14)}$) is based on all 14 subtests that also constitute a profile of the following seven broad abilities, each estimated by two subtests: Visual Processing, Processing Speed, Auditory Short-Term Memory, Visual-Spatial Short-Term Memory, Long-Term Memory, Abstract Reasoning, and Verbal Reasoning. The first seven subtests (one per broad ability) constitute a GIC without a factor profile–the FSIQ (IDS-$2_{FSIQ(7)}$). Additionally, the two subtests with the highest general factor loadings in a confirmatory factor analysis of the first seven subtests—Completing Matrices and Naming Categories—constitute the ABIQ (IDS-$2_{ABIQ(2)}$).[1] Finally, the IDS-2 include a rating of the participation of the testee during testing with 12 questions answered by the test administrator at the end of the intelligence, executive functions, and developmental functions assessments. Here, we used the answers on the intelligence assessment only.

The SB5 are an intelligence test battery for 4- to 83-year-olds that include a total of 10 subtests (Grob et al., 2019b; see Table S1 for descriptions). The following five broad abilities can be estimated based on one verbal and one nonverbal subtest each: Fluid Reasoning, Knowledge, Quantitative Reasoning, Visual-Spatial Processing, and Working Memory. Additionally, the five verbal and five

[1] This distinction of an extended battery (in the IDS-2: 14 subtests), a standard battery (in the IDS-2: seven subtests), and an abbreviated battery (in the IDS-2: two subtests) is also made in other test batteries, for example the Woodcock–Johnson IV (WJIV, Schrank et al., 2014) and the Universal Nonverbal Intelligence Test 2 (UNIT 2; Bracken & McCallum, 2016). The abbreviated battery is typically intended for screening purposes, the standard battery as an accurate and yet time efficient measure for diagnostic purposes, and the extended battery as a comprehensive measure that typically enables more or better-defined subscale scores (e.g., Schrank et al., 2014) and/or a more reliable and valid measure of general intelligence to be used for high-stakes decisions (e.g., Bracken & McCallum, 2016).

nonverbal subtests are used for a Verbal and a Nonverbal IQ. All 10 subtests are used for an FSIQ ($SB5_{FSIQ(10)}$) and the two routing subtests—Nonverbal Fluid Reasoning and Verbal Knowledge—constitute the ABIQ (hereafter called the $SB5_{ABIQ(2)}$). Finally, the SB5 include a rating of the participant's understanding of instructions and cooperation in the test situation with one question each answered by the test administrator at the end of the test session.

The RIAS measure verbal and nonverbal intellectual abilities as well as memory with two subtests each (six in total) in 3- to 99-year-olds (Hagmann-von Arx & Grob, 2014a; see Table S1 for descriptions). The two corresponding subtests are used to form a Verbal Intelligence Index, a Nonverbal Intelligence Index, and a Memory Index. All four intelligence subtests are used for an FSIQ ($RIAS_{FSIQ(4)}$). Additionally, one verbal and one nonverbal intelligence subtest—Guess What and Odd-Item Out—constitute the Reynolds Intellectual Screening Test (RIST; hereafter called the $RIAS_{ABIQ(2)}$).

Having seven available GICs thus enabled five comparisons: three for the IDS-2, one for the SB5, and one for the RIAS. The FSIQs from all three test batteries differ from each other in terms of number and content of subtests, whereas all ABIQs consist of one subtest each measuring fluid reasoning and comprehension knowledge (see Table 1). For the $IDS\text{-}2_{EBIQ(14)}$ and $IDS\text{-}2_{FSIQ(7)}$, as well as for the $RIAS_{FSIQ(4)}$ and $RIAS_{ABIQ(2)}$, only the number of subtests, and not the content, differs, as the corresponding GICs tap the same broad abilities in equal shares. In contrast, for the $IDS\text{-}2_{EBIQ(14)}$/$IDS\text{-}2_{FSIQ(7)}$ and $IDS\text{-}2_{ABIQ(2)}$, as well as for the $SB5_{FSIQ(10)}$ and $SB5_{ABIQ(2)}$, content and number of subtests differ.

Participant and Parent Questionnaires

Adolescent and adult participants and/or—for children and adolescents—their parents reported on demographic variables, including age, sex, education (additionally for children and adolescents: education of the parents), native language, and psychological and physical abnormalities (including AD[H]D). In an additional questionnaire, some parents reported their child's school grades in German (instructional language), mathematics, social studies, geography and history (combined), and science from the last two school semesters.

Procedure

Participants were recruited through schools and psychosocial institutions for children and adolescents in Switzerland, Germany, and Austria. For the IDS-2, administration of the whole test battery took between 3 and 4 h and, if necessary, could be split into two sessions no more than 1 week apart. Administration of the intelligence part alone took approximately 1.5 h and was completed within one test session. For the SB5, administration took 1.5 to 2 h and for the RIAS it took around 30 to 40 min. Written consent was obtained from children and adolescents (10 years and older) and/or from their parents (5- to 15-year-olds). The demographic questionnaire was administered at the beginning of the first session. The parental report of school grades was completed at home either within weeks after the session (IDS-2 and SB5) or as part of a follow-up study 2 to 4 years after the intelligence assessment (RIAS). Participants from Switzerland received a gift card of their own choice worth 30 (IDS-2) or 20

(SB5 and RIAS) Swiss francs and participants from Germany and Austria received 25 (IDS-2) or 12 (SB5 and RIAS) euros in cash for participation.

Table 1. *Number, Position, and Content of Subtests, and Reliabilities and Widths of 95% CIs for Each GIC*

					Internal consistency				Width of 95% CI			
GIC	# of Subt.	Pos. in Test Seq.	Content Overlap (%)	CHC Broad Abilities Tapped	Overall [a]	Age-specific [a]	Age- and IQ-level-specific [b]	r_{tt} [a]	95CI	$95CI_{age}$	$95CI_{ageIQ}$	$95CI_{rtt}$
$IDS\text{-}2_{EBIQ(14)}$	14	1–14	100	Gf, Gc, Gsm, Gv, Glr, Gs	.98	.95–.97	.67–.98	.85	8	10–13	8–28	21
$IDS\text{-}2_{FSIQ(7)}$	7	1–7	44	Gf, Gc, Gsm, Gv, Glr, Gs	.97	.92–.95	.54–.97	.89	10	12–16	10–30	19
$IDS\text{-}2_{ABIQ(2)}$	2	6, 7	38	Gf, Gc	.95	.83–.90	.53–.92	.86	13	17–23	15–30	21
$SB5_{FSIQ(10)}$	10	1–10	50	Gf, Gc, Gsm, Gv, Gq	.99	.93–.98	.45–.96	.94	6	8–16	10–30	14
$SB5_{ABIQ(2)}$	2	1, 2		Gf, Gc	.97	.76–.93	.30–.93	.86	10	15–26	15–30	21
$RIAS_{FSIQ(4)}$	4	1–4	100	Gf, Gc	.95	.93–.97	.55–.96	.88	13	10–16	11–30	19
$RIAS_{ABIQ(2)}$	2	1, 2		Gf, Gc	.93	.90–.94	.51–.96	.87	15	13–18	12–30	20

Note. Content overlap was calculated by dividing the number of subtests tapping the same broad abilities for both GICs by the total number of subtests over both GICs and multiplying this decimal by 100. Each content overlap percentage concerns the respective GIC and the one in the row below (for the $IDS\text{-}2_{ABIQ(2)}$: the $IDS\text{-}2_{EBIQ(14)}$). CHC broad ability assignments are based on information in the test manuals and descriptions in McGrew (2009, Table 1). Mean test–retest intervals were 24 days (IDS-2), 22 days (SB5), and 19 days (RIAS). Age- and IQ-level-specific internal consistencies: IQ groups: <85, 85–115, >115; age groups: IDS-2: 5–6, 7–8, 9–12, 13–15, 16–20 years, SB5: <7, 7–8, 9–12, 13–15, 16–20, 21–29, 30–59, ≥ 60 years, RIAS: 3–4, 5–6, 7–8, 9–12, 13–15, 16–20, 21–59, ≥ 60 years. GIC = general intelligence composite; CI = confidence interval; # of Subt. = number of subtests; Pos. in Test Seq. = position in test sequence; CHC = Cattell–Horn–Carroll; r_{tt} = test–retest reliability; 95CI = 95% CI with overall internal consistencies; $95CI_{age}$ = 95% CI with age-specific internal consistencies; $95CI_{ageIQ}$ = 95% CI with age- and IQ-level-specific internal consistencies; $95CI_{rtt}$ = 95% CI with test–retest reliability; EBIQ = Extended Battery IQ; FSIQ = Full-Scale IQ; ABIQ = Abbreviated Battery IQ; IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; Gf = fluid reasoning; Gc = comprehension knowledge; Gsm = short-term memory; Gv = visual processing; Glr = long-term memory and retrieval; Gs = cognitive processing speed; Gq = quantitative knowledge.

[a] Derived from manuals. Based on Cronbach's alphas (IDS-2 and RIAS) or split-half reliabilities (SB5).

[b] Computed according to the manuals with a formula provided by Lienert and Raatz (1998, p. 330) based on Cronbach's alphas (IDS-2 and RIAS) or split-half reliabilities (SB5).

Statistical Analyses

All analyses were conducted in R (R Core Team, 2020). The complete analysis code is available at https://osf.io/hfqe5/. Within each test battery, we first inspected group-level comparability of GICs with Pearson correlations (both uncorrected and corrected for unreliability of both GICs) and paired samples *t* tests. For individual-level comparability, we then calculated intraindividual absolute differences in IQ points. To compare the GICs' external validity, we performed linear regressions of school grades on GICs. All grades were transformed into Swiss school grades, ranging from 1 (lowest) to 6 (highest). In our study, we focused on grades in German and mathematics as well as on the grade point average (GPA). The GPA was computed as the average of all reported grades for each participant. As the GICs were expected to be highly correlated, we included them in separate models and compared the resulting R^2s and 95% CIs for standardized regression coefficients (betas). Because the models were

not nested, we could not determine the significance of the change in R^2. Instead, following Cumming (2009), we regarded two betas as significantly different from one another if their 95% CIs overlapped to a degree of 50% or less.

We explored several possible predictors of IQ differences, specifically, age, sex, AD(H)D (yes vs. no), native language (monolingual German [reference] vs. bilingual and vs. other native language), IQ level (average [85 ≤ IQ ≤ 115, reference] vs. below average [IQ < 85] and vs. above average [IQ > 115]), education of the participant or—for children and adolescents—of their mother (university degree vs. no university degree), participation in the test situation (for IDS-2; age-standardized scores with *M* = 10 and *SD* = 3), cooperation in the test situation (for SB5; yes vs. no/partly), understanding of instructions (for SB5; yes vs. no/partly), and the interaction between IQ level and age. We used gamma generalized linear models with a log link function to model IQ differences. In contrast to a classic linear regression, with a normally distributed dependent variable (Gaussian family) and an identity link function ($g(u) = u$), the generalized linear models we used model a gamma-distributed dependent variable (Gamma family) with a log link function ($g(u) = \log(u)$; see, for example, McElreath, 2015 for more information on generalized linear models). Using such gamma generalized linear models, we could best account for the strongly right-skewed, non-negative and continuous distribution of the dependent variables of absolute IQ differences (see Figure S1). Following suggestions from Gelman (2008), we standardized all predictor variables by dividing by 2 *SD*s. This way, regression coefficients are directly comparable in size between continuous and binary predictors. We deemed an effect significant if both the overall model (determined with a likelihood ratio test) and the predictor were significant at an alpha level of .05. To illustrate the variation of IQ differences across IQ level and age, we compared the resulting difference scores across IQ level (including six IQ groups: < 70, 70–84, 85–99, 100–114, 115–129, ≥ 130; see Figure S3) and across age (including different age groups depending on the test battery; see Figure S4). The IQ groups were based on the GIC with the largest number of subtests for each test battery (i.e., the $\text{IDS-2}_{\text{EBIQ(14)}}$, $\text{SB5}_{\text{FSIQ(10)}}$, and $\text{RIAS}_{\text{FSIQ(4)}}$). The same GICs were used for the predictor of IQ level in regression analyses.

To explore ways to deal with incomparability, we computed 95% CIs using the standard error of estimate together with the estimated true score (Lord & Novick, 1968; see also Dudek, 1979). For each test battery, we then calculated the percentage of participants for whom the 95% CIs for the IQs overlapped. We varied the reliability coefficients used for the calculation of 95% CIs to investigate their influence on individual-level comparability. The 95% CIs were based on overall internal consistencies (95CI; for IDS-2 and RIAS: Cronbach's alphas and for SB5: split-half reliabilities), age-specific internal consistencies (95CI_{age}; see Table S9 for age groups), and test–retest reliabilities (95CI_{rtt}) obtained from the test manuals (Grob & Hagmann-von Arx, 2018b; Grob et al., 2019a; Hagmann-von Arx & Grob, 2014b; see Table 1 for reliabilities and CIs).

Additionally, we calculated 95% CIs based on age- and IQ-specific internal consistencies according to the manuals using a formula provided by Lienert and Raatz (1998, p. 330; $95\text{CI}_{\text{ageIQ}}$; e.g.,

for 5- to 6-year-olds with IQ < 85; see Table 1 for IQ and age groups). Finally, we investigated the comparability of the IQs' corresponding nominal categories (NomIQ; < 70 = lower extreme, 70–84 = below average, 85–115 = average, 116–130 = above average, > 130 = upper extreme; see also Grob et al., 2013) as well as the comparability of the 95% CIs with overall internal consistencies in nominal categories (NomCI; e.g., average to above average for an interval of 112 to 120). For each of these six resulting criteria—95CI, $95CI_{age}$, $95CI_{ageIQ}$, $95CI_{rtt}$, NomIQ, and NomCI—two IQs were deemed comparable on an individual level if their intervals overlapped. Just as for IQ differences, we compared the percentages of participants with overlapping intervals across IQ level and age using the same groups.

Results

Group-Level Analyses

The seven GICs considered were normally distributed; their means were close to 100 (99.53 to 100.11) and standard deviations close to 15 (14.49 to 15.11, see Table 2). The IDS-$2_{FSIQ(7)}$ had the narrowest range with 55 to 142, and the $RIAS_{ABIQ(2)}$ had the widest range with 40 to 160. We compared the GICs within each test battery using *t* tests and Pearson correlations and found very small mean differences that were non-significant in all but one case (d = -0.002 for the IDS-$2_{FSIQ(7)}$ vs. the IDS-$2_{ABIQ(2)}$ to d = 0.031 for the $RIAS_{FSIQ(4)}$ vs. the $RIAS_{ABIQ(2)}$; the latter being significant, $t(2108) = 3.73$, $p < .001$). Intercorrelations both uncorrected and corrected for unreliability of both IQs were all significant and high to very high (r = .76 for the $SB5_{FSIQ(10)}$ and the $SB5_{ABIQ(2)}$ to r = .95 for the IDS-$2_{EBIQ(14)}$ and the IDS-$2_{FSIQ(7)}$, and r_{corr} = .77 for the $SB5_{FSIQ(10)}$ and the $SB5_{ABIQ(2)}$ to r_{corr} = .99 for the $RIAS_{FSIQ(4)}$ and the $RIAS_{ABIQ(2)}$, all with $p < .001$).

Table 2. *Descriptive Statistics, Paired-Samples* t *Tests and Pearson Correlations, and Intraindividual Absolute Differences in IQs*

GIC	*M*	*SD*	Range	Skewness	Kurtosis	*t*	Cohen's *d*	*r*	r_{corr}	M_{diff}	Md_{diff}	$Range_{diff}$
IDS-$2_{EBIQ(14)}$	100.04	14.70	55–145	-0.49	0.65	-0.61	-0.01	.95***	.98***	3.68	3	0–20
IDS-$2_{FSIQ(7)}$	100.11	14.79	55–142	-0.44	0.48	-0.12	-0.00	.82***	.86***	7.00	6	0–39
IDS-$2_{ABIQ(2)}$	100.08	15.11	55–144	-0.30	0.10	-0.12	-0.00	.77***	.80***	7.94	7	0–37
$SB5_{FSIQ(10)}$	99.96	14.80	55–145	-0.02	0.22	-0.18	-0.00	.76***	.77***	8.12	7	0–38
$SB5_{ABIQ(2)}$	99.92	14.95	55–145	-0.06	0.00							
$RIAS_{FSIQ(4)}$	99.53	14.77	45–158	-0.49	0.91	3.73***	0.03	.93***	.99***	4.37	4	0–20
$RIAS_{ABIQ(2)}$	99.98	14.49	40–160	-0.79	1.63							

Note. IDS-2: N = 1,622; SB5: N = 1,829; RIAS: N = 2,109. The last six columns refer to the comparison between the respective GIC and the one in the row below it (for the IDS-$2_{ABIQ(2)}$: with the IDS-$2_{EBIQ(14)}$). Cohen's d was calculated using the formula from Dunlap, Cortina, Vaslow, and Burke (1996) for paired samples. GIC = general intelligence composite; EBIQ = Extended Battery IQ; FSIQ = Full-Scale IQ; ABIQ = Abbreviated Battery IQ; IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; r_{corr} = Pearson correlation corrected for unreliability of both GICs; M_{diff}/Md_{diff}/$Range_{diff}$ = Mean/Median/Range of intraindividual absolute IQ difference.

***$p < .001$.

Intraindividual Differences

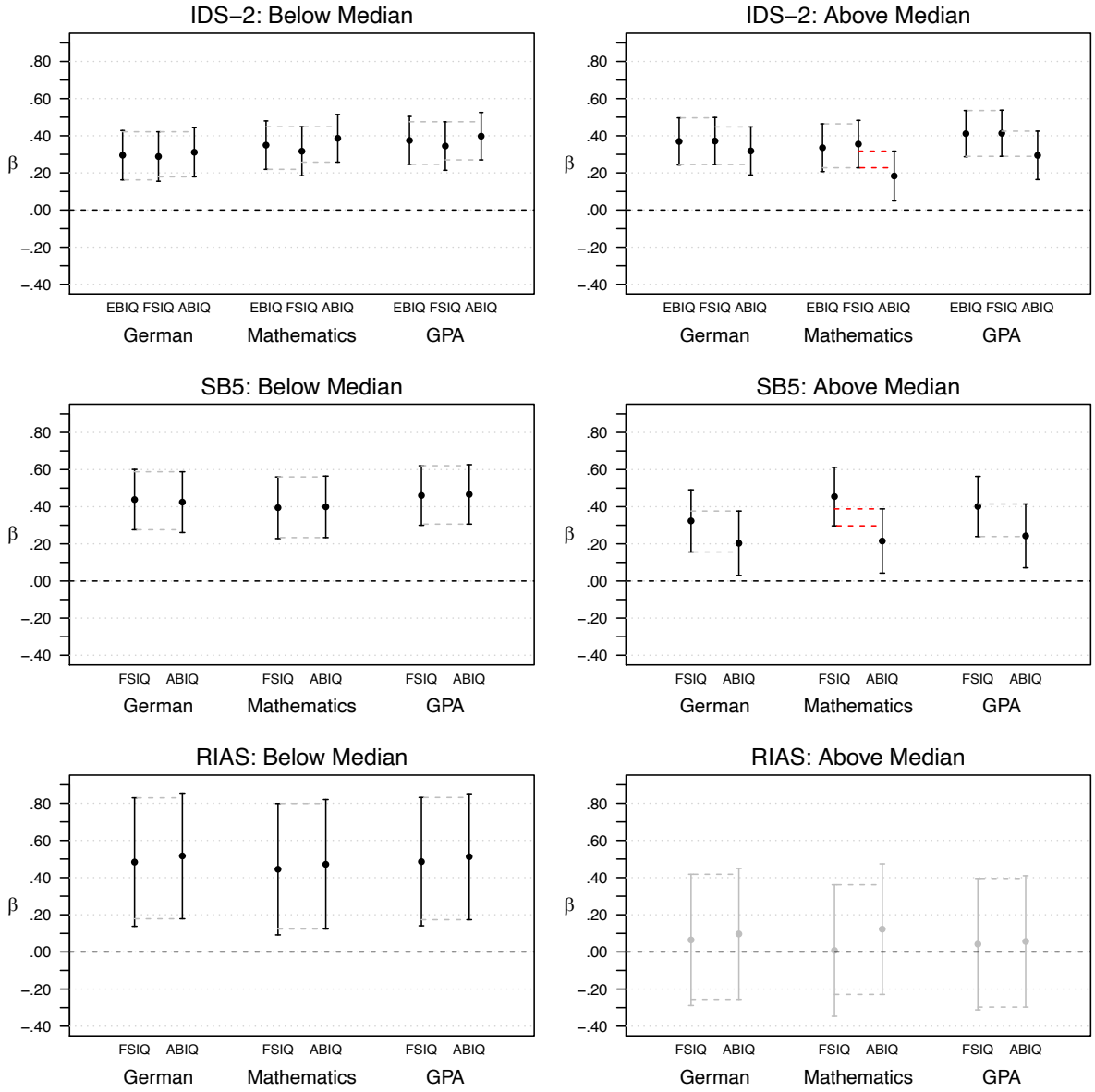
The mean (and median) intraindividual absolute differences ranged between 3.68 (*Mdn* = 3) IQ points for the IDS-$2_{EBIQ(14)}$ versus the IDS-$2_{FSIQ(7)}$ and 8.12 (*Mdn* = 7) IQ points for the $SB5_{FSIQ(10)}$ versus the $SB5_{ABIQ(2)}$, with ranges between 0 and 20 (IDS-$2_{EBIQ(14)}$ vs. IDS-$2_{FSIQ(7)}$ and $RIAS_{FSIQ(4)}$ vs. $RIAS_{ABIQ(2)}$) and 0 and 39 IQ points (IDS-$2_{FSIQ(7)}$ vs. IDS-$2_{ABIQ(2)}$; see Table 2). The relative differences were normally distributed around 0 (see Figure S2). Absolute differences across IQ groups and age are displayed in Figures S3 and S4, respectively (see also Table S2). For most comparisons, differences tended to increase with higher IQs and for the IDS-$2_{EBIQ(14)}$ versus the IDS-$2_{FSIQ(7)}$ and the $RIAS_{FSIQ(4)}$ versus the $RIAS_{ABIQ(2)}$, they tended to decrease with lower IQs. Regarding age, differences were lowest for middle childhood for the $SB5_{FSIQ(10)}$ versus the $SB5_{ABIQ(2)}$, but highest for the same age period for the $RIAS_{FSIQ(4)}$ versus the $RIAS_{ABIQ(2)}$. Otherwise, differences showed little variation across age.

Differential Relationships With School Grades

To compare the GICs' external validity, we investigated their differential relationships with school grades in German and mathematics, and with the GPA. Comparisons of 95% CIs for the betas revealed that the relationship with the FSIQ was significantly higher than that with the ABIQ only for the SB5 and mathematics (see Figure S5 and Table S3).

In a post hoc analysis, we repeated the external validity analyses for subsamples with small (below median) and large (above median) IQ differences to see how incomparability might affect external validity (see Figure 1 and Table S4). For individuals with small IQ differences, we found small to medium relationships that were all highly significant (β = .29 for the IDS-$2_{FSIQ(7)}$ and German to β = .52 for the $RIAS_{ABIQ(2)}$ and German, all with $p < .001$), and there were no significant differences in betas between the GICs. For individuals with large IQ differences, however, betas were still significant for the IDS-2 and SB5 (β = .18, p = .008 for the IDS-$2_{ABIQ(2)}$ and mathematics to β = .46, $p < .001$ for $SB5_{FSIQ(10)}$ and mathematics), but lower for the SB5 and no longer significant for the RIAS (β = .01, p = .965 for the $RIAS_{FSIQ(4)}$ and mathematics to β = .12, p = .483 for the $RIAS_{ABIQ(2)}$ and mathematics). For the IDS-2 and SB5, relationships with the ABIQ were also consistently smaller compared to those with the FSIQ and the EBIQ, although for both, this difference in betas was only significant for mathematics (see Figure 1).

Figure 1. *Comparison of 95% Confidence Intervals for Standardized Beta Coefficients for General Intelligence Composites Predicting School Grades in German and Mathematics and Grade Point Average (GPA)*



Note. The samples were split into subsamples with participants with intraindividual absolute differences in IQs below (IDS-2: $n = 203$, SB5: $n = 122$, RIAS: $n = 29$) and above (IDS-2: $n = 211$, SB5: $n = 127$, RIAS: $n = 35$) the median. A difference in betas was deemed significant if confidence intervals overlapped to a maximum of 50% (indicated in red). Significant betas are in black, nonsignificant betas in gray. Data for the IDS-2 and SB5 are cross-sectional; data for the RIAS are longitudinal. IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; EBIQ = Extended Battery IQ; FSIQ = Full-Scale IQ; ABIQ = Abbreviated Battery IQ.

Possible Predictors of IQ Differences

Next, we investigated possible predictors of IQ differences. Only the models for the comparisons of the IDS-$2_{EBIQ(14)}$ versus the IDS-$2_{ABIQ(2)}$, the $SB5_{FSIQ(10)}$ versus the $SB5_{ABIQ(2)}$, and the $RIAS_{FSIQ(4)}$ versus the $RIAS_{ABIQ(2)}$ were significant. Therein, IQ level and age and/or their interaction were the only consistent predictors (see Table 3; see Table S5 for results for all comparisons). Larger differences occurred for younger individuals for the SB5 and the RIAS, for individuals with a below-average IQ for the IDS-2 and the RIAS, and for individuals with an above-average IQ for the RIAS. Finally, there was a significant interaction effect for age and below-average IQ for the IDS-2 and for age and above-average IQ for the SB5 (see Figure S6). For the former, age was negatively associated with differences for individuals with below-average IQ, but not for individuals with average or above-average IQ. For the latter, although there was no main effect of IQ level, age was positively associated with differences for individuals with an above-average IQ, but negatively associated with differences for individuals with an average or below-average IQs (see supplementary material for a detailed description of results).

Table 3. *Gamma Generalized Linear Models With Possible Predictors of Absolute Differences in IQs*

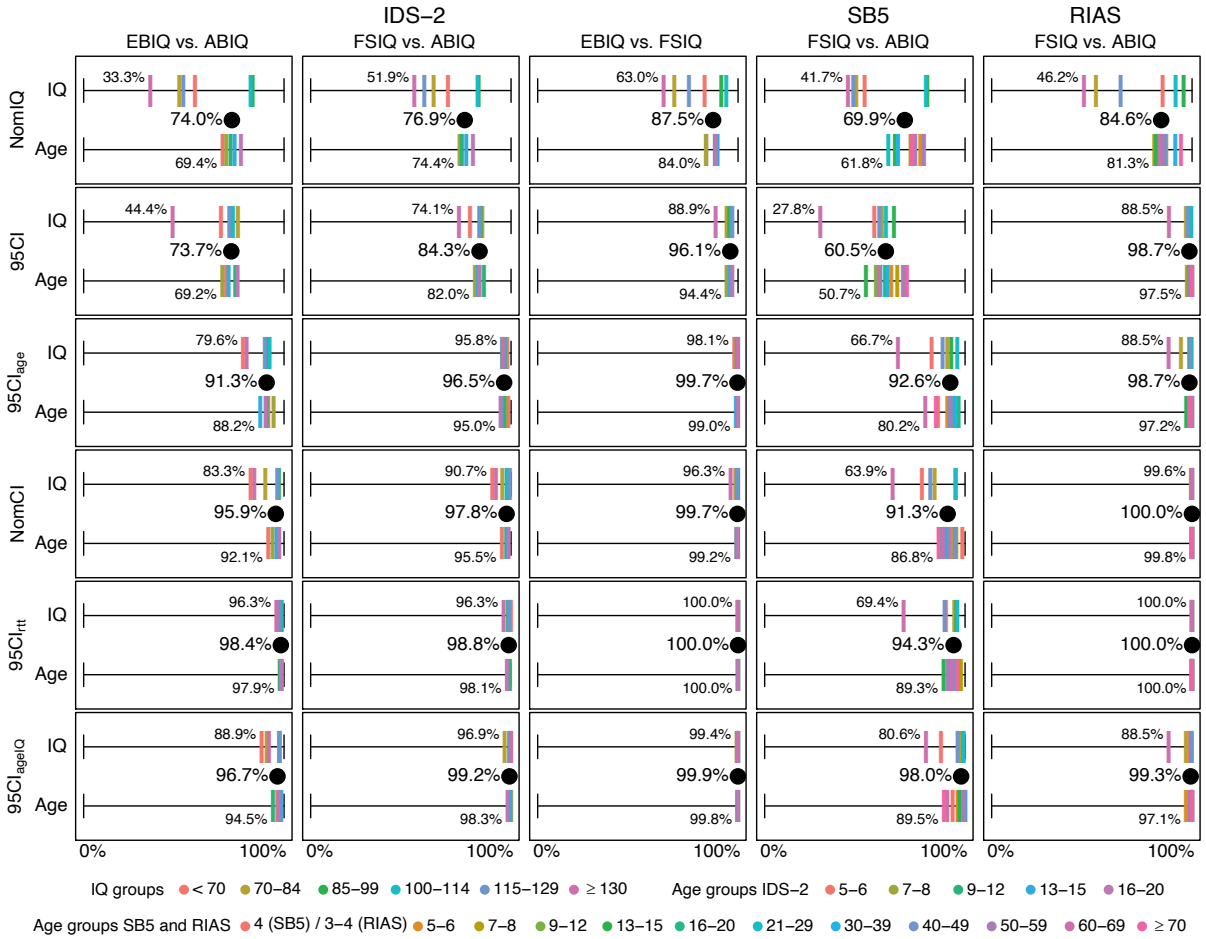
	IDS-2	SB5	RIAS
Predictor	EBIQ vs. ABIQ	FSIQ vs. ABIQ	FSIQ vs. ABIQ
Age	-0.00	-0.15**	-0.08*
Sex	0.00	0.03	0.03
AD(H)D	0.12	0.14	
Native language			
Bilingual	-0.14	0.00	-0.01
Other language	-0.04**	0.07	-0.01
Education	0.06	0.05	0.05
IQ level			
Below-Average IQ	0.01***	0.07	0.27***
Above-Average IQ	0.06	0.26	0.17**
Participation	0.00		
Cooperation		0.05	
Understanding		0.05	
Age*Below-Average IQ	-0.59***	0.12	-0.07
Age*Above-Average IQ	-0.10	0.31**	-0.16
Likelihood	24.04*	27.45**	29.52***

Note. IDS-2: $N = 1{,}566$, SB5: $N = 1{,}775$, RIAS: $N = 1{,}979$. Displayed are regression coefficients standardized by dividing by two standard deviations (Gelman, 2008). Sex: 0 = male, 1 = female; AD(H)D: 0 = no, 1 = yes; Bilingual: 0 = German, 1 = bilingual; Other language: 0 = German, 1 = other native language; Education (of participants or their mothers): 0 = no university degree; 1 = university degree; Below average IQ: $0 = 85 \leq IQ \leq 115$, $1 = IQ < 85$; Above average IQ: $0 = 85 \leq IQ \leq 115$, $1 = IQ > 115$; Cooperation (in the test situation) and Understanding (of instructions): 0 = yes, 1 = partly/no. AD(H)D = attention deficit/hyperactivity disorder or attention deficit disorder; Participation = participation in the test situation; IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; EBIQ = Extended Battery IQ; FSIQ = Full-Scale IQ; ABIQ = Abbreviated Battery IQ.
$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

Comparability Using Different Criteria

Table 1 shows the reliabilities and widths of the corresponding 95% CIs for all seven GICs. The width of the 95% CIs based on overall internal consistencies ranged between 6 ($SB5_{FSIQ(10)}$) and 15 ($RIAS_{ABIQ(2)}$) IQ points. Those based on age-specific internal consistencies and test–retest reliabilities were considerably larger, and those based on age- and IQ-specific internal consistencies reached up to 30 IQ points for some combinations of IQ > 115 and different age groups. The lowest age- and IQ-specific internal consistencies, resulting in the largest CIs, were found exclusively in groups with IQ > 115 and did not coincide with the lowest sample sizes for any of the IQs.

Figure 2. *Percentage of Participants With Comparable IQs (i.e., Overlapping Intervals) Determined by Six Criteria*



Note. The percentage of participants with comparable IQs both overall (black dots) and across IQ and age groups (color palette) are displayed. Numbers are displayed for the IQ and age group with the lowest percentage of participants with comparable IQs for each comparison. Ages given in years. NomIQ = IQ in nominal categories (e.g., "average" for IQ 85–115); 95CI = 95% CI with overall internal consistencies; $95CI_{age}$ = 95% CI with age-specific internal consistencies; NomCI = 95% CIs with overall internal consistencies in nominal categories; $95CI_{rtt}$ = 95% CI with test–retest reliabilities; $95CI_{ageIQ}$ = 95% CI with age- and IQ-specific internal consistencies; IDS-2 = Intelligence and Development Scales–2; SB5 = Stanford–Binet Intelligence Scales–Fifth Edition, German adaptation; RIAS = Reynolds Intellectual Assessment Scales, German adaptation; EBIQ = Extended Battery IQ; FSIQ = Full-Scale IQ; ABIQ = Abbreviated Battery IQ; CI = confidence interval.

The percentage of participants with comparable IQs (i.e., overlapping intervals) varied considerably across the different criteria and across IQ and age groups (see Figure 2 and Tables S6 to S11). With the 95CI criterion, overall comparability was between 60.5% and 98.7%. Across IQ groups it ranged between 27.8% and 99.6% and across age groups between 50.7% and 100%. The overall comparability was lowest for the NomIQ (69.9% to 87.5%) and the 95CI (60.5% to 98.7%) criteria and highest for the $95CI_{rtt}$ (94.3% to 100.0%) and the $95CI_{ageIQ}$ (96.7% to 99.9%) criteria. The same pattern was evident across IQ and age groups, with the lowest comparability for the NomIQ and the 95CI and highest comparability for the $95CI_{rtt}$ and the $95CI_{ageIQ}$. In general, comparability was lowest for the comparison of the $SB5_{FSIQ(10)}$ versus the $SB5_{ABIQ(2)}$ and highest for the comparison of the $RIAS_{FSIQ(4)}$ versus the $RIAS_{ABIQ(2)}$.

Discussion

The primary objective of this study was to investigate the individual-level internal comparability of different GICs. As expected, all GICs were highly intercorrelated and—with one exception—there were no significant mean differences. Despite this high correspondence on the group level, individual-level comparability was not always satisfactory. Intraindividual absolute differences reached up to 39 IQ points and tended to be larger for above-average IQ and younger ages. With respect to external validity, the EBIQ and FSIQ explained more variance in school grades compared to the ABIQ only for individuals with large IQ differences and only for the IDS-2 and SB5, with significant differences only for mathematics. Regarding possible predictors, IQ level and age, and/or their interaction, were the only consistent predictors of IQ differences. Finally, regarding ways to deal with incomparability, comparability varied considerably across criteria and again across both IQ level and age both within and between comparisons. While comparability for the NomIQ and 95CI was often unsatisfactory, it was very high for the $95CI_{rtt}$ and $95CI_{ageIQ}$.

Group-Level Comparability and Intraindividual Differences

On the group level, all GICs within each test battery were highly comparable, with the exception of the $RIAS_{FSIQ(4)}$ and $RIAS_{ABIQ(2)}$, where we found a significant mean difference despite a very high correlation. However, the effect size was very small, suggesting the effect is negligible. Despite high comparability on the group level, intraindividual absolute differences between GICs varied considerably, from 0 to more than 2.5 *SD*s (*M* between 0.25–0.53 *SD*s), depending on the comparison. There were no systematic differences in one direction; the relative differences were normally distributed around 0 for all comparisons. The mean IQ differences were slightly lower than those found in previous studies investigating individual-level comparability of FSIQs between test batteries (Bünger et al., 2021; Floyd et al., 2008; Hagmann-von Arx et al., 2018). Still, the size of the differences seems remarkable, given that the subtests used for the GICs overlap, that transient error is kept to a minimum, that the GICs were standardized on the same individuals, and that between-battery variance in general is ruled out completely. Revisiting the requirements introduced above that need to be fulfilled as a

prerequisite for equal scores (Dorans & Holland, 2000), these findings indicate that the equity requirement is violated for the compared GICs, and thus the scores are not exchangeable.

Differential External Validity

Analyses on differential external validity revealed that, as could be expected due to its lower reliability, the ABIQ tended to show weaker relationships with school grades compared to the FSIQ and EBIQ for most comparisons. However, this discrepancy in relationships with school grades was only significant for participants with large IQ differences and only for the IDS-2 and SB5 and mathematics. The two comparisons with the largest discrepancies also featured the largest IQ differences.

Apparently, the ABIQs miss aspects of intelligence that are contained in the EBIQ and FSIQs that are especially important for mathematical achievement. For the IDS-2, this probably concerns additional working memory aspects and visual-spatial skills known to be especially important for mathematical achievement (e.g., Bull & Lee, 2014; Kahl et al., 2021; McCrink & Opfer, 2014), that are included in the FSIQ and EBIQ, but not the ABIQ. For the SB5, the incremental validity of the FSIQ is probably mostly due to the quantitative knowledge subtests, and the working memory and visual-spatial processing subtests as well. Moreover, relationships were smaller for individuals with large compared to small IQ differences for the SB5 and RIAS, to the point that, for the RIAS (longitudinal analysis), they were no longer significant for individuals with large IQ differences.

From these findings we conclude that a GIC based on more subtests is not necessarily a better predictor for school grades compared to one based on fewer subtests, especially for individuals with low IQ differences. We also conclude that larger IQ differences do have consequences for external validity, as the GICs for which larger intraindividual differences occurred were also the ones with larger disparities in relationships with school grades, and as relationships tended to be lower in general for individuals with high IQ differences. Finally, differences in content seem to be more important than differences in the number of subtests per se for differential external validity.

Possible Predictors of IQ Differences

Given results from previous studies showing that most error variance in IQs was systematic (Floyd et al., 2008; Irby & Floyd, 2016, 2017), it is likely that the IQ differences we found are not entirely due to random error. Our results suggest that characteristics of the testee are likely one systematic influence, as IQ differences varied across IQ level and age, and those two and/or their interaction were the only systematic predictors in regression analyses. These results are in line with Hagmann-von Arx et al. (2018) and Bünger et al. (2021), where, for some comparisons, IQ differences were larger for younger participants and at the tails of the IQ distribution as well.

With respect to age, younger participants had higher IQ differences for the SB5 and the RIAS (age range: early childhood to late adulthood) but not for the IDS-2 (age range: early childhood to late adolescence). In Bünger et al. (2021), age was also not systematically linked to IQ differences, indicating that the effect of age might also depend on individual test characteristics. With respect to IQ

level, the finding of larger differences toward the tails of the IQ distribution is to be expected due to regression to the mean (Campbell & Kenny, 1999). In Hagmann-von Arx et al. (2018), IQ level was a significant predictor of IQ differences also only for some comparisons, while in Bünger et al. (2021), it was for all included comparisons. In both studies, all effects went in the direction of larger differences toward the tails of the IQ distribution as well.

Besides regression to the mean, floor and ceiling violations could also explain larger differences at the tails of the IQ distribution and at younger ages (e.g., Irby & Floyd, 2017). The raw scores are usually not scaled homogenously across the full spectrum of scores, such that small differences in the number of correct responses will have a disproportionate effect on scaled scores at the extremes of the ability spectrum (i.e., very high or very low ability, or very young age).[2] This disproportionate influence at the extremes is more pronounced the fewer subtests/items are included in a composite, further questioning the use of really short measures (cf. Irby & Floyd, 2016, 2017).

A third and related explanation for larger differences towards the tails of the IQ distribution is the composite score extremity effect (Schneider, 2016), that is, the fact that a composite score tends to be more extreme than the average of the subtest scores it is composed of. This effect is larger the more subtests are included in a composite. Hence, a GIC composed of more subtests should render higher scores for above-average IQ, and lower scores for below-average IQ, compared to a GIC composed of less subtests. Table S12 illustrates this effect for our comparisons. However, this influence was less pronounced, as absolute IQ differences were not necessarily larger for comparisons of GICs with larger differences in the number of subtests (see below).

Fourth and lastly, larger IQ differences at the upper extreme of the IQ distribution are probably also in part due to Spearman's law of diminishing returns (SLODR, Spearman, 1927). In line with SLODR, it has been shown that the general factor loadings of CHC broad ability factors decreased, and their specific variance increased with increasing IQ level (e.g., Reynolds, 2013; Tucker-Drob, 2009). Consequently, the validity of a GIC from the five broad ability composites also decreased with increasing IQ level. It can therefore be expected that GICs that sample different broad abilities (or the same, but to varying extents) will differ more for individuals with higher IQ. Thus, the effect of SLODR might cumulate with the abovementioned factors decreasing comparability at high IQ levels, and at the same time might diminish the effect of said other factors at low IQ levels. Our results of slightly larger differences at the upper tail of the IQ distribution compared to the lower tail support this notion.

In our study (and not investigated in previous studies) there were also significant interaction effects between IQ level and age. From the above considerations follows that the disproportionate influence of few items should be especially pronounced for older individuals with high IQ and for younger individuals with low IQ. Regression results support this in that the significant interaction

[2] For tests that only cover an age span in childhood, typically the same is true for individuals at the upper tail of the age distribution of the standardization sample. This was not the case for any of the test batteries in our study.

effects went in the expected direction. All in all, our findings indicate that these two variables—IQ level and age—should be considered in conjunction with each other when calculating reliability coefficients.

Finally, the included predictors explained a significant amount of variance for only three of the five comparisons. It is likely that other variables that could not be sufficiently considered in the present study contribute to systematic variance in IQ differences, for example (achievement) motivation, attention span, or alertness.

Thus, there are at least two characteristics of the testee (i.e., IQ level and age) that explain some of the variance in IQ differences. These findings indicate that the population invariance requirement is violated, possibly due at least in part to violations of the same construct and equal reliability requirements (Dorans & Holland, 2000).

A second source of systematic variability, characteristics of the composites, likely played a role as well. Three such characteristics are number, general factor loadings, and content of subtests included in the composites. Farmer et al. (2020) showed that the most accurate composites are those derived from numerous (12 to 13) diverse subtests with high general factor loadings, where high general factor loadings are more important compared to heterogeneity. Their results also suggest that including fewer than four subtests results in substantial losses of accuracy. In line with common practice, the ABIQs included in our study are all composed of only two subtests. Further, although all three ABIQs fulfill the heterogeneity criterion with the two subtests representing different broad abilities, only the subtests for the IDS-$2_{ABIQ(2)}$ were chosen based on the highest general factor loadings. The $SB5_{ABIQ(2)}$ is composed of the subtests with the lowest (Nonverbal Fluid Reasoning) and third lowest (Verbal Knowledge) general factor loading (Grob et al., 2019a), which might at least partly explain the larger differences we found for the SB5 compared to the IDS-2 and the RIAS.

Subtest content may also play a role. In this regard, it is especially interesting to compare the comparisons of IDS-$2_{EBIQ(14)}$ versus IDS-$2_{FSIQ(7)}$ and $RIAS_{FSIQ(4)}$ versus $RIAS_{ABIQ(2)}$. Both comparisons have the same degree of overlap in content (100%, see Table 1) and the same ratio of subtests (2:1) but different absolute numbers of subtests (4 and 2 vs. 14 and 7) and different numbers of broad abilities tapped (2 vs. 7). Differences for IDS-$2_{EBIQ(14)}$ versus IDS-$2_{FSIQ(7)}$ are slightly lower than for $RIAS_{FSIQ(4)}$ versus $RIAS_{ABIQ(2)}$, but both are considerably lower compared to the other comparisons.

To conclude, the same construct requirement is likely also violated, and larger overlap in content and high general factor loadings—thus, the fulfilment of the same construct requirement—seems to be more important than the sheer number of subtests for individual-level comparability. However, as our set of comparisons is very limited, these findings clearly need replication, ideally with comparisons of composites systematically varied in content, general factor loadings, and number of subtests.

Ways to Deal with Incomparability

We explored several alternatives to exact IQ scores—namely, nominal categories and 95% CIs based on different reliability coefficients—with the aim of achieving a more dependable intelligence

estimate. Results on percentages of participants with overlapping 95% CIs or nominal IQs reflect results on IQ differences in that they varied both between the different comparisons and across IQ level and age. Although all investigated criteria consider unreliability in some way, comparability still tended to be lower at younger ages and toward the tails of the IQ distribution.

Furthermore, comparability varied considerably between the different criteria. Although the overall percentages of participants with overlap of the 95CI and the NomIQ tended to be higher compared to those found in previous studies on between-battery comparisons (Bünger et al., 2021; Hagmann-von Arx et al., 2018), they were still unsatisfactory. Especially when calculated separately for IQ and age groups, the percentage of participants with comparable IQs was sometimes very low, down to 28%. Rates of comparability were higher for the $95CI_{age}$ and the NomCI criteria but the highest rates were achieved with the $95CI_{rtt}$ or the $95CI_{ageIQ}$ criteria. This is to be expected, given that the intervals were also often widest for these criteria. Which of the two—$95CI_{rtt}$ or $95CI_{ageIQ}$—provides a better trade-off between comparability and precision (interval width) is difficult to pin down as this varies across GICs and across GIC comparisons. It is important to note here that we had to rely on fairly rough groups for IQ (< 85, 85–115, and > 115) and for age in adulthood (e.g., age 30–59 years for the SB5 and age 21–59 years for the RIAS). Additionally, group sizes varied considerably and were sometimes very low (IDS-2: $n = 31$ to $n = 352$; SB5: $n = 15$ to $n = 222$; RIAS: $n = 23$ to $n = 175$). The comparability versus precision trade-off could probably be improved for the $95CI_{ageIQ}$ if larger, more fine-graded groups were considered, which would necessitate sampling more participants of diverse ages at the tails of the IQ distribution. Finally, both internal consistency and test–retest reliability miss certain kinds of measurement error. While internal consistency does not consider transient error, test–retest reliability does not consider specific factor error (i.e., errors due to individual interpretation of items; Schmidt et al., 2003). Therefore, other approaches may be even more beneficial. The coefficient of equivalence and stability (Cronbach, 1947), for example, considers both specific factor error and transient error. As this coefficient requires the administration of two parallel test forms on two different measurement occasions, we were not able to consider it in our study.

Finally, given the numerous equality requirements that are violated, more accurate CIs can be only part of the solution to incomparability, mainly as a means for practitioners to deal with incomparability of results from existing intelligence tests. Given the substantial differences we found, the consequences they have for validity, and the large intervals needed to achieve satisfactory individual-level comparability, the long-term goal must be to create more reliable and valid intelligence measures. To achieve a higher individual-level comparability, it might be necessary to question our current understanding of general intelligence and to refrain from multidimensional measures (i.e., subtests intended to measure both general intelligence and a broad ability; see also Beaujean & Benson, 2019). Instead, test developers could try to create unidimensional measures of specific broad abilities with a firmer theoretical and neurological basis (e.g., Beaujean & Benson, 2019; Kovacs & Conway, 2019). In this vein, using fluid reasoning measures instead of GICs composed of multiple broad abilities

might be beneficial for diagnostic utility, especially at the upper end of the IQ distribution, as Reynolds (2013) found fluid reasoning to be the only composite not influenced by SLODR and being the best indicator for general intelligence across IQ levels and all investigated age levels (except 5-6 year-olds, where Comprehension–Knowledge was slightly better). For IQ > 115, it was even better than a GIC composed of all five broad abilities. More narrowly defined constructs and carefully developed, theory-driven instruments to measure these constructs as reliably and validly as possible are a prerequisite for the same construct requirement—and with this also the equity and population invariance requirements (Dorans & Holland, 2000)—to be fulfilled and for the interpretation of test results to have meaning beyond the particular test that was used.

Implications

Our findings have implications for the construction, validation, and application of intelligence tests. First, they raise awareness that choosing the subtests with the highest general factor loadings for a short form does not necessarily result in comparable results to those for the full test battery. However, it is certainly better than choosing subtests with lower general factor loadings (see also Farmer et al., 2020).

Second, our results indicate that in terms of both individual-level comparability and external validity there are no large gains between the 7- and 14-subtest composites (the FSIQ and the EBIQ, respectively) for the IDS-2. In line with results from Farmer et al. (2020), this suggests a diminishing marginal utility of additional subtests—especially if they do not introduce other broad abilities—from a certain number of subtests on.

Third, our results speak against using one internal consistency coefficient derived from the whole sample for the calculation of CIs. Instead, we recommend the use of test–retest reliabilities, age- and IQ-specific internal consistencies or, probably even better, the coefficient of equivalence and stability (Schmidt et al., 2003). The additional resources spent on the construction and application of a parallel test form would be compensated for by more accurate reliability estimates and by a test battery that could be administered twice to the same testee without introducing learning effects. Ideally, the test–retest sample should also be large enough to permit at least a rough division into IQ and age groups to enable the use of age- and IQ-specific test–retest reliabilities or coefficients of equivalence and stability for the calculation of CIs.

Fourth, we encourage test developers to reconsider the current understanding of general intelligence, and to try to develop purer (i.e., unidimensional) measures guided by formal theories (e.g., Beaujean & Benson, 2019), as clearly defined constructs are a prerequisite for individual-level comparability of test scores.

Fifth, exact IQ scores should not be used for the interpretation or communication of test results. Indeed, in line with Bünger et al. (2021), our results show that even the 95% CI might not necessarily be valid enough for clinical interpretation, but it is certainly more appropriate than an exact IQ score. As done before (Bünger et al., 2021), we again call for a paradigm shift away from exact IQ scores

toward intervals that consider the unreliability of intelligence composites in clinical interpretation. Instead of requiring an IQ score to fall above or below a certain threshold, the upper and lower levels of the 95% CI should be considered.

Sixth, our results demonstrate that the differences between the FSIQ and the ABIQ are largest especially in those ranges where most clinical questions arise—namely, at the tails of the IQ distribution. This is true even if 95% CIs are based on the expected true score, thus accounting for regression to the mean. To avoid the risk of missing diagnostically meaningful information, we suggest using a short test of at least four subtests (see Farmer et al., 2020) instead of an ABIQ with less subtests for screening purposes. A context, gaining importance in many Western countries, in which very short measures should be especially avoided, is for testees with low familiarity with (standardized) testing or test content as well as with difficulties in understanding task instructions. Following insights from dynamic testing (Beckmann, 2014; Beckmann & Dobat, 2000; Cho & Compton, 2015; Guthke & Wiedl, 1996), test performance for these testees increases in predictive validity with increasing time spent with the tasks. For example, it was shown that in a test–retest design, performance in the posttest was a better predictor for scholastic achievement compared to performance in the pretest, especially for disadvantaged children (Guthke & Wiedl, 1996). The use of a screening instrument thus bears the risk of underestimating an individual's intellectual potential especially in these contexts.

Finally, IQ differences are linked to the prediction of school grades. For individuals with higher IQ differences, relationships with school grades tended to be lower in general, and especially for the ABIQ. In the long run, GICs might not even be predictive at all for school grades for these individuals. It is therefore important to identify these individuals, for example, through multiple testing, and to be aware of the possibility of reduced reliability and (external) validity of GICs in these cases.

Future research should determine to what extent the present results are applicable to broad ability composites as well. If two subtests are likely not enough for a GIC, this should be even less appropriate for a broad ability composite, given the small unique variance over and above the general factor such broad ability composites already capture (e.g., Cucina & Howardson, 2017). At the same time, content overlap should be larger for broad ability composites, raising the possibility of higher comparability of these scores compared to more heterogeneous GICs, at least after unreliability is taken into account. Interestingly, this is exactly what Bünger et al. (2021) found for verbal index scores from different intelligence test batteries. Comparability of CIs for broad ability composites reported in Floyd et al. (2005) also tended to be larger compared to the comparability of GICs reported in Bünger et al. (2021), Floyd et al. (2008), and Hagmann-von Arx et al. (2018), despite often larger absolute differences in IQ points for broad ability composites.

We also advocate the use of individual-level comparisons in addition to group-level analyses for validation of a test procedure intended for individual diagnostics. More research is needed to further investigate characteristics of both the testee and the test itself that are associated with individual-level incomparability of intelligence composites. Finally, in addition to internal and structural validation, a

greater emphasis should be placed on external validation, but also on diagnostic and treatment utility, of test scores to determine their usefulness as a diagnostic instrument in practice.

Strengths and Limitations

We investigated group- and individual-level internal comparability of GICs for a set of three test batteries based on large, representative samples covering a large age span from early childhood to late adulthood. In comparing GICs within test batteries, we were able to eliminate all kinds of variance between test batteries or test situations (including carryover effects, differences in standardization samples and global test characteristics, and transient errors), leaving characteristics of the testee and the test itself as the primary systematic sources of variance.

A limitation of this study is that we could form only broad IQ groups for age- and IQ-specific 95% CIs (i.e., below average, average, above average) due to small sample sizes within age groups. Greater oversampling of participants of different ages at the tails of the IQ distribution is needed to achieve more fine-graded groups and with this to ensure reliability and validity at the extremes.

Furthermore, we used school grades as a single criterion of external validity. Although school grades are strongly related to general intelligence (Roth et al., 2015), future research should consider differential relationships of GICs based on different numbers of subtests with additional criteria for scholastic achievement, such as scholastic aptitude tests or teacher ratings of school performance, as well as with criteria that are also valid for adults, for example, educational attainment or occupational success.

Finally, we could include only a limited number of test batteries and composites in our study. Systematic comparisons of the kind performed in Farmer et al. (2020)—comparisons of composites systematically varied in characteristics such as number, general factor loadings, and content of subtests—but on an individual level and within multiple test batteries are needed to further clarify the number and nature of subtests necessary to achieve more reliable and valid measures of general intelligence.

Conclusion

Our findings raise awareness of the limitations of ABIQs as a means to get a first impression of an individual's intellectual potential. Despite high comparability on the group level, individual-level comparability of GICs derived from the same test battery was often unsatisfactory. We therefore advocate to acknowledge a lower reliability of GICs to achieve more accurate intelligence assessments. One step in that direction would be to refrain from using internal consistencies and to instead use test–retest reliabilities or, probably even better, the coefficient of equivalence and stability (Cronbach, 1947) as a basis for CIs. The systematic effects of IQ level and age on IQ differences we found suggest that reliabilities should also be computed separately for age and IQ groups. Most importantly, our results demonstrate that the interpretation of exact IQ scores should be avoided. However, despite limited comparability with the FSIQ, we found that ABIQs did not necessarily display less external validity.

But GICs in general, and especially ABIQs, tended to be worse predictors of school grades, especially in mathematics, for individuals with large intraindividual IQ differences.

To conclude, our results point to substantial intraindividual IQ differences that have consequences for external validity and are at least in part explained by IQ level and age. Our results demonstrate that a focus on CIs based on reasonable reliability coefficients is one way to deal with incomparability. Yet, further research is needed to learn more about the number and kind of subtests necessary to achieve an accurate measurement of general intelligence on the individual level.

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APPENDIX F: Curriculum Vitae

Academic Background

08/2017 – present	**PhD in Psychology** Division of Developmental and Personality Psychology | Department of Psychology | University of Basel
08/2015 – 07/2017	**Master of Science in Psychology** **Major in Developmental and Personality Psychology** University of Basel (Grade: 6.0/6.0) Master's thesis: "Factor structure of the cognitive functions of the Intelligence and Development Scales for children and adolescents" (Grade: 6.0/6.0)
08/2012 – 07/2015	**Bachelor of Science in Psychology** University of Basel (Grade: 5.8/6.0) Bachelor's thesis: "Parental risk factors prospectively associated with conduct disorder in children and adolescents" (Grade: 5.5/6.0)

Professional Activities

08/2017 – present	**Assistant** Division of Developmental and Personality Psychology | Department of Psychology | University of Basel
02/2019 – present	**Instructor** Hogrefe AG | Bern and Göttingen
02/2015 – 07/2017	**Student Assistant** Dean of Students Office | Department of Psychology | University of Basel
06/2016 – 08/2016 06/2015 – 08/2015	**Intern** University of Basel and Psychiatrie Baselland | Liestal
06/2014 – 07/2014 01/2013 – 12/2013	**Administrative Assistant** Facility Management | Psychiatrie Baselland | Liestal

Teaching and Supervision

Seminars

2017 – 2021	**Master's Colloquium in Personality and Developmental Psychology** | Master | University of Basel
2017 – 2021	**Journal Club** | Master | University of Basel
2019 – 2020	**Test Diagnostics** | Bachelor | University of Basel
2020	**SON-R and CFT: Language Free and Culture Fair Intelligence Diagnostics** | Hogrefe AG, Bern
2019	**IDS-2: Intelligence and Development Diagnostics** | Hogrefe Verlag GmbH & Co. KG, Göttingen
2018	**Development in Families With Migration Background: Risks and Opportunities** | Master | University of Basel
2018	**Intelligence as the Key to Success in Life?** | Master | University of Basel

Supervision

2017 – 2021	Three Bachelor's Theses | University of Basel
2018 – 2019	Three Master's Theses | University of Basel

Publications

Grieder, S. & Steiner, M. D. (in press). Algorithmic jingle jungle: A comparison of implementations of principal axis factoring and promax rotation in R and SPSS. *Behavior Research Methods*. PsyArXiv Preprint. https://doi.org/10.31234/osf.io/7hwrm

Grieder, S., Bünger, A., Odermatt, S. D., Schweizer, F., & Grob, A. (in press). Limited internal score comparability of general intelligence composites: Impact on external validity, possible predictors, and practical remedies. *Assessment*.

Grieder, S., Timmerman, M. E., Visser, L., Ruiter, S. A. J., & Grob, A. (2021). *Factor structure of the Intelligence and Development Scales–2: Measurement invariance across the Dutch and German versions, sex, and age.* Manuscript submitted for publication. PsyArXiv Preprint. https://doi.org/10.31234/osf.io/vtw3g

Bünger, A., **Grieder, S.**, Schweizer, F., & Grob, A. (2021). *The comparability of intelligence test results: Group- and individual-level comparisons of seven intelligence tests.* Manuscript submitted for publication.

Canivez, G. L., **Grieder, S.**, & Bünger, A. (2021). Construct validity of the German Wechsler Intelligence Scale for Children–Fifth Edition: Exploratory and confirmatory factor analyses of the 15 primary and secondary subtests. *Assessment*, *28*(2), 327–352. https://doi.org/10.1177/1073191120936330

Schweizer, F., **Grieder, S.**, Bünger, A., & Grob, A. (2021). Vergleich von Intelligenztestleistungen bei monolingualen und bilingualen Kindern und Jugendlichen in den Intelligence and Development Scales–2 (IDS-2) [Comparison of intelligence test performance of monolingual and bilingual children and adolescents in the Intelligence and Development Scales–2 (IDS-2)]. *Diagnostica, 67*(1), 36–46. https://doi.org/10.1026/0012-1924/a000260

Steiner, M. D. & **Grieder, S.** (2020). EFAtools: An R package with fast and flexible implementations of exploratory factor analysis tools. *Journal of Open Source Software, 5*(53), Article 2521. https://doi.org/10.21105/joss.02521

Grieder, S. & Grob, A. (2020). Exploratory factor analyses of the Intelligence and Development Scales–2: Implications for theory and practice. *Assessment, 27*(8), 1853–1869. https://doi.org/10.1177/1073191119845051

Conference Presentations

Grieder, S., Bünger, A., & Canivez, G. L. (2020, July). Confirmatory factor analyses with the 10 German WISC–V primary subtests. In G. L. Canivez (Chair). Construct validity of international WISC-V versions: Informing evidence based assessment. Symposium accepted at the 12th International Test Commission (ITC) Conference, Esch-sur-Alzette, Luxembourg (postponed).

Grieder, S. & Steiner, M. D. (2020, July). Algorithmic jingle jungle: comparison of implementations of an EFA procedure in R psych versus SPSS. Poster accepted at the 12th ITC Conference, Esch-sur-Alzette, Luxembourg (postponed).

Grieder, S., Bünger, A., & Canivez, G. L. (2019, July). Hierarchical exploratory factor analysis of the German WISC-V primary and secondary subtests. In G. L. Canivez (Chair). Validity investigations for international versions of the WISC-V: Informing evidence based assessment. Symposium conducted at the 41st International School Psychology Association Conference (ISPA), Basel, Switzerland.

Grieder, S., Odermatt, S. D., Bünger, A., Schweizer, F., & Grob, A. (2019, July). Are we overestimating IQ reliability? Intraindividual comparability of screening IQ and full-scale IQ for three test batteries. Poster presented at the 41st ISPA Conference, Basel, Switzerland.

Grieder, S., Odermatt, S. D., Bünger, A., Schweizer, F., & Grob, A. (2019, May). Are we overestimating IQ reliability? Intraindividual comparability of screening IQ and full-scale IQ for three test batteries. Poster presented at the 31st Annual Convention of the Association for Psychological Science (APS), Washington D.C., USA.

Grob, A. & **Grieder, S.** (2018, September). Die Struktur sozial-emotionaler Kompetenzen und deren Zusammenhang mit relevanten Entwicklungsfunktionen im Kindes- und Jugendalter [The structure of socioemotional skills and their relationship with relevant developmental functions in childhood and adolescence]. In K. Voltmer (Chair). Emotionale Kompetenzen im Kindesalter Teil I: Methoden der Erfassung [Emotional skills in childhood part I: Assessment methods]. Symposium conducted at the 51st Deutsche Gesellschaft für Psychologie (DGPs) Congress, Frankfurt, Germany.

Further Education and Training

10/2020	**Thesis Defense Training** | Seminar | University of Basel
09/2020	**Learning How to Lead and to Build a Successful Work Environment** | Transferable Skills | University of Basel
02/2020	**Exploratory Data Analysis With R** | The R Bootcamp | University of Basel
12/2018	**Cultural Sensitivity** | Course part of the Master of Advanced Studies in Child and Adolescent Psychology | University of Basel
11/2018	**Python for Psychologists** | Seminar | University of Basel
10/2018	**Writing to Be Published: Academic Writing Conventions and Style** | Transferable Skills | University of Basel
10/2018	**Writing Productivity: Tools and Techniques** | Transferable Skills | University of Basel
05/2018	**Advanced Quantitative Methods Summer School** | University of Oxford
12/2017	**Teaching Methods for Seminar, Workshops, and Block Courses That Promote Learning** | University Didactics | University of Basel
12/2017	**Hands-On Practical Organization Semester Planning** | University Didactics | University of Basel

Academic Skillset

R	Proficient | Author of the *EFAtools* package that implements exploratory factor analyses tools; Data preparation, analysis, visualization, and reporting
SPSS, Amos	Advanced | Data preparation and analysis
Python	Basic | Data wrangling and visualization
MS Office	Advanced | Word: Writing scientific articles, project-related work, admin; Excel: Tables, advanced formulas, plotting; PowerPoint: Teaching and scientific presentations
LaTeX	Intermediate | Writing scientific articles