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The Flynn Effect: A Meta-Analysis

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The *Flynn effect* refers to the observed rise in IQ scores over time, which results in norms obsolescence. Although the Flynn effect is widely accepted, most efforts to estimate it have relied upon "scorecard" approaches that make estimates of its magnitude and error of measurement controversial and prevent determination of factors that moderate the Flynn effect across different IQ tests. We conducted a meta-analysis to determine the magnitude of the Flynn effect with a higher degree of precision, to determine the error of measurement, and to assess the impact of several moderator variables on the mean effect size. Across 285 studies ($N = 14{,}031$) since 1951 with administrations of 2 intelligence tests with different normative bases, the meta-analytic mean was 2.31, 95% CI [1.99, 2.64], standard score points per decade. The mean effect size for 53 comparisons ($N = 3{,}951$, excluding 3 atypical studies that inflate the estimates) involving modern (since 1972) Stanford-Binet and Wechsler IQ tests (2.93, 95% CI [2.3, 3.5], IQ points per decade) was comparable to previous estimates of about 3 points per decade but was not consistent with the hypothesis that the Flynn effect is diminishing. For modern tests, study sample (larger increases for validation research samples vs. test standardization samples) and order of administration explained unique variance in the Flynn effect, but age and ability level were not significant moderators. These results supported previous estimates of the Flynn effect and its robustness across different age groups, measures, samples, and levels of performance.

Keywords: Flynn effect, IQ test, intellectual disability, capital punishment, special education

The *Flynn effect* refers to the observed rise over time in standardized intelligence test scores, documented by Flynn (1984b) in a study on intelligence quotient (IQ) score gains in the standardization samples of successive versions of Stanford-Binet and Wechsler intelligence tests. Flynn's study revealed a 13.8-point increase in IQ scores between 1932 and 1978, amounting to a 0.3-point increase per year, or approximately 3 points per decade. More recently, the Flynn effect was supported by calculations of IQ score gains between 1972 and 2006 for different normative versions of the Stanford-Binet (SB), Wechsler Adult Intelligence Scale (WAIS), and Wechsler Intelligence Scale for Children (WISC; Flynn, 2009b). The average increase in IQ scores per year was 0.31, which was consistent with Flynn's (1984b) earlier findings.

The Flynn effect implies that an individual will likely attain a higher IQ score on an earlier version of a test than on the current version. In fact, a test will overestimate an individual's IQ score by an average of about 0.3 points per year between the year in which the test was normed and the year in which the test was administered. The ramifications of this effect are especially pertinent to the diagnosis of intellectual disability in high-stakes decisions when an IQ cut point is used as a necessary part of the decision-making process. The most dramatic example in the United States is the determination of intellectual disability in capital punishment cases. These determinations in so-called Atkins hearings represent life-and-death decisions for death row inmates scheduled for execution. Because an inmate may have received several IQ scores with different normative samples over time, whether to acknowledge the Flynn effect is a major bone of contention in the legal system. In addition, the Flynn effect figures in access to services and accommodations, such as determining eligibility for special education and American Disability Act services and Social Security Disability Insurance (SSDI) in the United States.

More generally, conceptions about IQ as a predictor of success in various domains are pervasive in many domains of the behavioral sciences and in Western societies. Many studies use IQ scores as an outcome variable or in characterizing the sample. In clinical practice, most assessments routinely administer an IQ test, and most applied training programs teach administration and interpretation of IQ test scores. Organizations such as MENSA set IQ levels associated with "genius," and people commonly refer to others as "bright" or use more pejorative terms as an indicator of their level of ability. Although the meaningfulness of these uses of IQ scores is beyond the scope of this investigation, they illustrate the pervasiveness of concepts about IQ scores as indicators of individual differences and level of performance.

The Flynn effect is less well known and often is not taught in behavioral science training programs (Hagan, Drogin, & Guilmette, 2008). It is important because the normative base of the test directly influences the interpretation of the level of IQ. MENSA, the "high IQ society," requires an IQ score in the top 2% of the population (www.us.mensa.org/join/testscores/qualifyingscores). The organization accepts scores from a variety of tests, often with

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no specification of which version of the test. The Stanford-Binet 4 and Stanford-Binet 5 are both permitted. If a person applied and took an IQ test in 2014, the required score of 132 on the Stanford-Binet 4 would be equivalent to a score of 126 on the recently normed Stanford-Binet 5 because the normative sample was formed 20 years ago. Although the Flynn effect is not necessarily of general interest to psychology, the pervasive use of IQ test scores in clinical practice and research, in high-stakes decisions, and in Western society suggests that it should be. It is not surprising that a PsycINFO search shows that the number of articles on the Flynn effect rose from 6 in 2001–2002 to 54 in 2010–2011. Most significant is the use of IQ scores in identifying intellectual disabilities and the death penalty, where there are literally hundreds of active cases in the judicial system, and in determining eligibility for social services and special education.

Definition of Intellectual Disability

The identification of an intellectual disability in the United States requires the presence of significant limitations in intellectual functioning and adaptive behavior prior to age 18 (American Association on Intellectual and Developmental Disabilities [AAIDD], 2010). An IQ score at least two standard deviations below the mean (i.e., ≤ 70) is a common indicator of a significant limitation in intellectual functioning and captures approximately 2.2% of the population. Although the gold standard AAIDD criteria stress the importance of exercising clinical judgment in the interpretation of IQ scores (e.g., accounting for measurement error), a cutoff score of 70 commonly is used to indicate a significant limitation in intellectual functioning (Greenspan & Switzky, 2006). Thus, were an adult to have attained an IQ score of 73 on the Wechsler Intelligence Scale for Children—Revised (WISC–R) as a child, he or she might not be identified as having a significant limitation in intellectual functioning. However, suppose the WISC–R had been administered in 1992, 20 years after the test was normed. The Flynn effect would have inflated test norms by 0.3 points per year between the year in which the test was normed (1972) and the year in which the test was administered (1992). Correction for that inflation would reduce the person's IQ score by 6 points, to 67, thereby indicating a significant limitation in intellectual functioning and highlighting the problems with obsolete norms. Further, the WISC–III, published in 1989, would have been the current edition of the test when the child was tested. This underscores the importance of testing practices (e.g., acquiring and administering the current version of a test) in formal education settings.

High-Stakes Decisions

Capital punishment. The Eighth Amendment of the U.S. Constitution prohibits cruel and unusual punishment, and that prohibition informed the Court's decision in *Atkins v. Virginia* (2002) to abstain from imposing the death penalty on a defendant with an intellectual disability. In this case, Daryl Atkins, a man determined to have a mild intellectual disability, was convicted of capital murder. The Supreme Court of Virginia initially imposed the death penalty on Atkins; however, the United States Supreme Court reversed the decision due to the presumed difficulty people with intellectual disabilities have in understanding the ramifications of criminal behavior and the emergence of statutes in a growing number of states barring the death penalty for defendants with an intellectual disability.

In 2008, a report indicated that since the reversal of the death penalty in the Atkins case, more than 80 death penalty pronouncements have been converted to life in prison (Blume, 2008). This number has increased significantly since 2008. Of importance, *Walker v. True* (2005) set a precedent for the consideration of the Flynn effect in capital murder cases. The defendant argued in an appeal that his sentence violated the Eighth Amendment; when corrected for the Flynn effect, his IQ score of 76 on the WISC, administered to the defendant in 1984 when he was 11 years old, would be reduced by 4 points to 72. He alleged that a score of 72 fell within the range of measurement error recognized by the AAIDD (2010) and the American Psychiatric Association (2000) for a true score of 70. The judges agreed that the Flynn effect and measurement error should be considered in this case. There are hundreds of Atkins hearings involving the Flynn effect in some manner and other issues related to the use of IQ tests (see AtkinsMR/IDdeathpenalty.com).

Special education. Demonstration of an intellectual disability or a learning disability is an eligibility criterion for receipt of special education services in schools. Kanaya, Ceci, and Scullin (2003) and Kanaya, Scullin, and Ceci (2003) documented a pattern of "rising and falling" IQ scores in children diagnosed with an intellectual disability or learning disability as a function of the release date of the new version of an intelligence test. One study (Kanaya, Ceci, & Scullin, 2003) mapped IQ scores obtained from children's initial special education assessments between 1972 and 1977, during the transition from the WISC to the WISC–R, and between 1990 and 1995, during the transition from the WISC–R to the WISC–III. The authors reported a reduction in IQ scores during the fourth year of each interval (1 year after the release of the new test version) followed by an increase in IQ scores during subsequent years. In a second study (Kanaya, Scullin, & Ceci, 2003), the authors reported a 5.6-point reduction in IQ score for children initially tested with the WISC–R and subsequently tested with the WISC–III, with a significantly greater proportion of these children being diagnosed with an intellectual disability during the second assessment than children who completed the same version of the WISC during both assessments. More recent studies have supported these patterns in children assessed for learning disabilities with the WISC–III (Kanaya & Ceci, 2012).

Taken together, these studies suggest that the use of obsolete norms leads to inflation of the IQ scores of children referred for a special education assessment as a function of the time between the year in which the test was normed and the year in which the test was administered. The use of a test with obsolete norms reduces the likelihood of a child being identified with an intellectual disability and receiving appropriate services, and it may increase the prevalence of learning disabilities; the inflated IQ score helps produce a discrepancy between intellectual functioning and achievement, which in education settings has often been interpreted as indicating a learning disability (Fletcher, Lyon, Fuchs, & Barnes, 2007). These studies also highlight the importance of using the current version of a test in education settings, a practice that may be thwarted by a school district's budgetary constraints and challenges associated with learning the administration and scoring procedures for the new test (Kanaya & Ceci, 2007).

Social security disability. As with determination of the death penalty and eligibility for special education, IQ testing remains an important component of the decision-making process for determining eligibility for SSDI as a person with an intellectual disability. Like the AAIDD, the Social Security Administration (2008) requires significant limitations in intellectual functioning and adaptive behavior for a diagnosis of intellectual disability; however, these limitations must be present prior to age 22. Moreover, individuals with an IQ at or below 59 are eligible de facto for SSDI, whereas those with an IQ between 60 and 70 must demonstrate work-related functional limitations resulting from a physical or other mental impairment or two other specified functional limitations (e.g., social functioning deficits). The manual, like the AAIDD manual, explicitly discusses the importance of correcting for the Flynn effect, but it acknowledges that precise estimates are not available.

Flynn's Work

Flynn's (1984b) landmark study, which revealed increasing IQ at a median rate of 0.31 points per year between 1932 and 1978 across 18 comparisons of the SB, WAIS, WISC, and Wechsler Preschool and Primary Scale of Intelligence (WPPSI), was the first analysis of its kind. Seventy-three studies totaling 7,431 participants provided support for this effect. Whereas Flynn's (1984b) study focused on comparisons documented in publication manuals of primarily the first editions of the Stanford-Binet and Wechsler tests, a second study investigated IQ gains in 14 developed countries using a variety of instruments, including Raven's Progressive Matrices, Wechsler, and Otis-Lennon tests (Flynn, 1987). IQ gains amounted to a median of 15 points in one generation, described by Flynn (1987) as "massive." An extension of Flynn's (1984b) work documented a mean rate of IQ gain equaling approximately 0.31 IQ points per year across 12 comparisons of the SB, WAIS, and WISC standardization samples (Flynn, 2007), a value highly consistent with earlier findings. Further, 14 comparisons of Stanford-Binet and Wechsler standardization samples, accounting for the recent publication of the WAIS–IV, revealed an annual rate of IQ gain equaling 0.31 (Flynn, 2009b). These latter findings, based on the simple averaging of IQ gains across studies, were supported by the only meta-analysis addressing the Flynn effect (Fletcher, Stuebing, & Hughes, 2010). For these 14 studies, Fletcher et al. (2010) calculated a weighted mean rate of IQ gain of 2.80 points per decade, 95% CI [2.50, 3.09], and a weighted mean rate of IQ gain of 2.86, 95% CI [2.50, 3.22], after excluding comparisons that included the WAIS–III because effect sizes produced by comparisons between the WAIS–III and another test differed considerably from the effect sizes produced by comparisons between other tests. The puzzling effects produced by comparisons including the WAIS–III were consistent with Flynn's (2006b) study, wherein he demonstrated that IQ score inflation on the WAIS–III was reduced because of differences in the range of possible scores at the lower end of the distribution.

Other notable investigations conducted by Flynn include the computation of a weighted average IQ gain per year of 0.29 between the WISC and WISC–R across 29 studies comprising 1,607 participants (Flynn, 1985); a rate of IQ gain per year of 0.31 between the WISC–R and the WISC–III across test manual studies and a selection of studies carried out by independent researchers (Flynn, 1998b); and a rate of IQ gain per year of 0.20 between the WAIS–R and WAIS–III across test manual studies (Flynn, 1998b). Prior to these studies, Flynn (1984a) also reported SB gains across standardization samples and both real and simulated gains for the WPPSI and the first two versions of the WISC and WAIS. Flynn noted consistent gains between the WISC ($N = 93$) and WISC–R ($N = 296$) in Scottish children (Flynn, 1990); for the Matrices and Instructions tests in an Israeli military sample totaling approximately 26,000 participants per year between 1971 and 1984 (Flynn 1998b); between the WISC–III and an earlier version of the test in samples from the United States, West Germany, Austria, and Scotland totaling 3,190 participants (Flynn, 2000); and for the Coloured Progressive Matrices in British standardization samples totaling 1,833 participants (Flynn, 2009a). The existence of the Flynn effect is rarely disputed. However, a working magnitude and measurement error associated with the Flynn effect are not well established, leaving unanswerable the question of how much of a correction—if any—to apply to IQ test scores to account for the norming date of the test. Further, there is considerable contention over factors that may cause the Flynn effect (Flynn, 2007, 2012; Neisser, 1998).

Proposed Causes of the Flynn Effect

There are multiple hypotheses about the basis for the Flynn effect, including genetic and environmental factors and measurement issues.

Genetic hypotheses. Mingroni (2007) hypothesized that IQ gains are the result of increasingly random mating, termed heterosis (or hybrid vigor), a phenomenon that produces changes in traits governed by the combination of dominant and recessive alleles. However, Lynn (2009) noted that the Flynn effect in Europe has mirrored the effect in the United States despite evidence of minimal migration to Europe prior to 1950 and limited intermating between native and immigrant populations since then. A more comprehensive argument against a genetic cause for the Flynn effect has been made by Woodley (2011).

Environmental factors. Woodley (2011) argued that "the [Flynn] effect only concerns the non-*g* variance unique to specific cognitive abilities" (p. 691), presumably bringing environmental explanations for the Flynn effect to the forefront. Environmental factors hypothesized as moderators of the Flynn effect include sibship size (Sundet, Borren, & Tambs, 2008) and prenatal and early postnatal nutrition (Lynn, 2009). In Norway, Sundet et al. demonstrated that an increase in IQ scores paralleled a decrease in sibship size, with the greatest increase in IQ scores occurring between cohorts with the greatest decrease in sibship size. For example, between birth cohort 1938–1940 and 1950–1952, the percentage of sibships composed of 6+ children decreased from 20% to 5%, and IQ score increased by 6 points.

With rates of Development Quotient score gains in infants mirroring IQ score gains of preschool children, school-age children, and adults, Lynn (2009) questioned the validity of explanations whose effects would emerge later in development, such as improvements in child rearing (Elley, 1969) and education (Tuddenham, 1948); increased environmental complexity (Schooler, 1998), test sophistication (Tuddenham, 1948), and test-taking confidence (Brand, 1987); and the effects of genetics (Jensen, 1998) and the individual and social multiplier phenomena (Dickens &

Flynn, 2001a, 2001b). Lynn (2009) proposed improvements in pre- and postnatal nutrition as likely causes of the Flynn effect, citing a parallel increase in infants of other nutrition-related characteristics, including height, weight, and head circumference. Improvement to the prenatal environment is also supported by trends in the reduction of alcohol and tobacco use during pregnancy (Bhuvaneswar, Chang, Epstein, & Stern, 2007; Tong, Jones, Dietz, D'Angelo, & Bombard, 2009).

Neisser (1998) suggested that increasing IQ scores have mirrored socioenvironmental changes in developing countries. If IQ test score changes are a product of socioenvironmental improvements, then as living conditions optimize, IQ scores should plateau. This suggestion has been echoed by Sundet, Barlaug, and Torjussen (2004), who documented a plateau in IQ scores in Norway (Sundet et al., 2004) and speculated that changes in family life factors (e.g., family size, parenting style, and child care) might be partly responsible for this pattern. A decline in IQ scores has even been noted in Denmark (Teasdale & Owen, 2008, 2005), a pattern that the authors suggested might be due to a shift in educational priorities toward more practical skills manifest in the increasing popularity of vocational programs for postsecondary education.

Although Flynn (2010) acknowledged that his "scientific spectacles" hypothesis may no longer explain current IQ gains, he maintained that there was a period of time when it was the foremost contributor. Putting on scientific spectacles refers to the tendency of contemporary test takers to engage in formal operational thinking, as evidenced by a massive gain of 24 IQ points on the Similarities subtest of the WISC, a measure of abstract reasoning, between 1947 and 2002, a gain unparalleled by any other subtest (Flynn & Weiss, 2007). Conceptualizing IQ gains as a shift in thinking style from concrete operational to formal operational rather than an increase in intelligence per se would explain why previous generations thrived despite producing norms on IQ tests that overestimated the intellectual abilities of future generations (Flynn, 2007). However, this difference may be more simply attributed to changes across different versions of Similarities and other verbal subtests (Kaufman, 2010) of the WISC. Nonetheless, Dickinson and Hiscock (2010) reported a Flynn effect for WAIS Similarities of 4.5 IQ points per decade for WAIS to WAIS–R and 2.6 IQ points per decade for WAIS–R to WAIS–III. The average was 3.6 IQ points per decade or 0.36 IQ points per year. This change in adult performance is only moderately less than Flynn's 0.45 points per year for the WISC between 1947 and 2002.

Measurement issues. Tests of verbal ability, compared with performance-based measures, have been reported to be less sensitive to the Flynn effect (Flynn, 1987, 1994, 1998a, 1999), which may be related to changes in verbal subtests. Beaujean and Osterlind (2008) and Beaujean and Sheng (2010) used item response theory (IRT) to determine whether increases in IQ scores over time reflect changes in the measurement of intellectual functioning rather than changes in the underlying construct (i.e., the latent variable of cognitive ability). Although changes in scores on the Peabody Picture Vocabulary Test—Revised were negligible (Beaujean & Osterlind, 2008), it is a verbal test that differs in many respects from Wechsler and Stanford-Binet tests. Wicherts et al. (2004) found that intelligence measures were not factorially invariant, such that the measures displayed differential patterns of gains and losses that were unexpected given each test's common factor means. Taken together, these studies suggest that increases in IQ scores over time may be at least partly a result of changes in the measurement of intellectual functioning. Moreover, Dickinson and Hiscock (2010) reported that published norms for age-related changes in verbal and performance subtests do not take into account the Flynn effect. In comparisons of subtest scores from the WAIS–R and WAIS–III in 20-year-old and 70-year-old cohorts, the Flynn-corrected difference in Verbal IQ between 20-year-olds and 70-year-olds was 8.0 IQ points favoring the 70-year-olds (equivalent to 0.16 IQ points per year). In contrast, the younger group outscored the older group in Performance IQ by a margin of 9.5 IQ points (equivalent to 0.19 IQ points per year). These findings suggested that apparent age-related declines in Verbal IQ between the ages of 20 and 70 years are largely artifacts of the Flynn effect and that, even though age-related declines in Performance IQ are real declines, the magnitudes of those declines are amplified substantially by the Flynn effect.

Some studies have examined intercorrelations among subtests of IQ measures to determine the variance in IQ scores explained by *g*, with preliminary evidence suggesting that IQ gains have been associated with declines in measurement of *g* (Kane & Oakland, 2000; te Nijenhuis & van der Flier, 2007). Flynn (2007), on the other hand, has discounted the association between *g* and increasing IQ scores, and a dissociation between *g* and the Flynn effects has been claimed by Rushton (2000). However, Raven's Progressive Matrices, renowned for its *g*-loading, has demonstrated a rate of IQ gain of 7 points per decade, more than double the rate of the Flynn effect as manifested on WAIS, SB, and other multifactorial intellectual tests (Neisser, 1997).

What Is Rising?

The theories highlighted above offer explanations for the Flynn effect but leave an important question unanswered: What exactly does the Flynn effect capture (i.e., what is rising)? Although much of the previous research on the Flynn effect has focused on the rise of mean IQ scores over time, studies distinguishing rates of gain among elements of IQ tests more readily answer the question of what is rising. Relative to scores produced by verbal tests, there have been greater gains in scores produced by nonverbal, performance-based measures like Raven's Progressive Matrices (Neisser, 1997) and Wechsler performance subtests (Dickinson & Hiscock, 2011; Flynn, 1999). These types of tests are strongly associated with fluid intelligence, suggesting less of a rise in crystalized intelligence that reflects the influence of education, such as vocabulary. A notable exception is the increasing scores produced by the Wechsler verbal subtest Similarities (Flynn, 2007; Flynn & Weiss, 2007), although this subtest taps into elements of reasoning not required by the other subtests comprising the Wechsler Verbal IQ composite.

Dickens and Flynn (2001b) provided a framework for understanding the rise in more fluid versus crystallized cognitive abilities. They identified social multipliers as elements of the sociocultural milieu that contributed to rising IQ scores among successive cohorts of individuals. Flynn (2006a) highlighted two possible sociocultural contributions to the Flynn effect, one related to patterns of formal education and the other to the influence of science. Specifically, years of formal education increased in the years prior to World War II, whereas priorities in formal education shifted from rote learning to problem solving in the years following World War II. As time continued to pass, the value placed on problem solving in the workplace and leisure time spent on cognitively engaging activities continued to exert an effect on skills assessed by nonverbal, performance-based measures. The second sociocultural contributor, science, refers to the simultaneous rise in the influence of scientific reasoning and the abstract thinking and categorization required to perform well on nonverbal, performance-based measures.

The Current Study

Our primary objective in this meta-analysis was to determine whether the Flynn effect could be replicated and more precisely estimated across a wide range of individually administered, multifactorial intelligence tests used at different ages and levels of performance. Answers to these research questions will assist in determining the confidence with which a correction for the Flynn effect can be applied across a variety of intelligence tests, ages, ability levels, and samples. By completing the meta-analysis, we also hoped to provide evidence evaluative of existing explanations for the Flynn effect, thus contributing to theory.

With the exceptions of the Flynn (1984b, 2009a) and Flynn and Weiss (2007) analyses of gains in IQ scores across successive versions of the Stanford-Binet and Wechsler intelligence tests, most research comparing IQ test scores has focused on correlations between two tests and/or average mean difference between two successive versions of the same test. This study will expand the literature on estimates of the Flynn effect by computing more precisely the magnitude of the effect over multiple versions of several widely used, individually administered, multifactorial intelligence tests; namely, Kaufman, Stanford-Binet, and Wechsler tests and versions of the Differential Ability Scales, McCarthy Scales of Children's Abilities, and the Woodcock-Johnson Tests of Cognitive Abilities. The data for these computations were obtained from validity studies conducted by test publishers or independent research teams. In addition to providing more precise weighted meta-analytic means, meta-analysis allows estimates of the standard error and evaluation of potential moderators.

This study deliberately focused on sources of heterogeneity (i.e., moderators) that could be readily identified through meta-analytic searches and that helped explain variability in estimates of the magnitude of the Flynn effect. Investigation of these moderators is needed to advance understanding of variables that might limit or promote confidence in applying a correction for the Flynn effect in high-stakes decisions. Here, the IQ tests that are used are variable in terms of test and normative basis, with the primary focus on the composite score. The tests are given to a broad age range and to people who vary in ability. It is not clear that the standard Flynn effect estimate can be applied among individuals of all ability levels and ages who took any of a number of individually administered, multifactorial tests. In addition, there may be special circumstances related to test administration setting that might influence the numerical value of the Flynn effect. If the selected moderators (i.e., ability level, age, IQ tests administered, test administration setting, and test administration order) influence the estimate of the Flynn effect, the varying estimates will contribute to the tenability of the theories offered above for the existence and meaning of the Flynn effect.

The evidence for influences of these moderators is mixed, with no clear directions. Recent evidence has suggested that middle and lower ability groups (IQ = 79–109) demonstrate the customary 0.31–0.37-point increase per year, whereas higher ability groups (IQ = 110+) demonstrate a minimal increase of 0.06–0.15 points per year (Zhou, Zhu, & Weiss, 2010). Whereas some previous studies have supported this finding (e.g., Lynn & Hampson, 1986; Teasdale & Owen, 1989), others have not. Two studies found the opposite pattern (Graf & Hinton, 1994; Sanborn, Truscott, Phelps, & McDougal, 2003), and one study indicated smaller gains at intelligence levels both above and below average, with the highest gains evident in people at the lowest end of the ability spectrum (Spitz, 1989). Little research has been conducted to investigate the relation between age and gains in IQ score. Cross-sectional research has indicated no difference among young children, older children, and adults (Flynn, 1984a) and no difference among adult cohorts ranging in age from 35 to 80 years (Rönnlund & Nilsson, 2008).

Research on the Flynn effect has focused almost exclusively on the effect produced from administrations of the Stanford-Binet and Wechsler tests. This study expanded the scope by including a wider range of individually administered, largely multifactorial intelligence tests. Comparisons of older and more recently normed versions of the Stanford-Binet and Wechsler tests were conducted to facilitate comparisons with previous work and help determine if the Flynn effect has remained constant over time.

Another potential moderator pertains to study sample. Study data were collected by test publishers or independent researchers for validation purposes or by mental health professionals for clinical decision-making purposes. Validation studies conducted by test publishers likely employed the most rigorous procedures with regard to sampling, selection of administrators, and adherence to administration and scoring protocols. However, the more homogenous samples examined in the research and clinical studies (e.g., children suspected of having an intellectual disability or juvenile delinquents) may produce results that are more generalizable to specific populations and that permit comparison of Flynn effect values across those special populations.

Another set of moderators involves measurement issues, such as changes in subtest configuration and order effects. These issues were addressed by Kaufman (2010), who pointed out that changes in the instructions and content of specific Wechsler subtests (e.g., Similarities) could make comparing older and newer versions akin to comparing apples and oranges. However, other research has shown that estimates of the size of the Flynn effect based on changes in subtest scores yield values similar to estimates from the composite scores (Agbayani & Hiscock, 2013; Dickinson & Hiscock, 2010). Kaufman's concern related to interpretations of the basis of the Flynn effect and not to its existence, and we did not pursue this question because it has been addressed in other studies (Dickinson & Hiscock, 2011). Subtest coding of a larger corpus of tests was difficult because the data were often not available. However, Kaufman also suggested that the Flynn effect could be the result of prior exposure when taking the newer version of an IQ test first and then transferring a learned response style to the older IQ test, with test takers thus receiving higher scores when the older test is given second. In order for order effects to occur, the interval between the administration of the new and old tests would have to be short enough for the examinee to demonstrate learning, which is often the case in studies comparing different versions of an IQ test, the basis for determination of the Flynn effect.

Although the Flynn effect has been well documented during the 20th century, the meta-analytic method used during the current study is a novel approach to documenting this phenomenon. The method of the current study aligns with a key research proposal identified by Rodgers (1999) as important in advancing our understanding of the Flynn effect; namely, a formal meta-analysis. Although many of Rodgers's (1999) proposals have since been implemented, there remains room for understanding the meaning of the Flynn effect, how the Flynn effect is reflected in batteries of tests over time, and how the Flynn effect manifests itself across subsamples defined by ability level or other characteristics.

Method

Inclusion and Exclusion Criteria

Studies identified from test manuals or peer-reviewed journals were included if they reported sample size and mean IQ score for each test administered; these variables were required for computation of the meta-analytic mean. All English-speaking participant populations from the United States and the United Kingdom were included. Variations in study design were acceptable. Administration of both tests must have occurred within 1 year of one another. Studies could have been conducted at any point prior to the completion date of the literature search in 2010.

We limited our primary investigation to comparisons between tests with greater than 5 years between norming periods, which is consistent with Flynn's (2009a) work. The rationale for this decision was that any difference in IQ scores from a short interval, even seemingly insignificant ones, would be magnified when converted to a value per decade (see Flynn, 2012). As a secondary analysis, we expanded our investigation to all comparisons between tests with at least 1 year between norming periods to assess whether our decision to limit our investigation to comparisons between tests with greater than 5 years between norming periods affected the results of the meta-analysis. We did not include comparisons between tests with 1 year or less between norming periods, because years between norming periods served as the denominator of our effect size. A value of zero, representing no difference in years between norming periods, produced an error in the effect size estimate. Finally, we did not include single construct tests, such as the Peabody Picture Vocabulary Test or the Test of Nonverbal Intelligence. There may be other multifactorial tests to consider, but the 27 we chose represent the major IQ tests in use over the past few decades.

Search Strategies

Twenty-seven intelligence test manuals for multifactorial measures were obtained, one for each version of the Differential Ability Scales (Elliot, 1990, 2007), Kaufman Adolescent and Adult Intelligence Test (Kaufman & Kaufman, 1993), Kaufman Assessment Battery for Children (Kaufman & Kaufman, 1983, 2004a), Kaufman Brief Intelligence Test (Kaufman & Kaufman, 1990, 2004b), McCarthy Scales of Children's Abilities (McCarthy, 1972), Stanford-Binet Intelligence Scale (Roid, 2003; Terman & Merrill, 1937, 1960, 1973; Thorndike, Hagen, & Sattler, 1986), Wechsler Abbreviated Scale of Intelligence (Wechsler, 1999), Wechsler Adult Intelligence Scale (Wechsler, 1955, 1981, 1997, 2008), Wechsler Intelligence Scale for Children (Wechsler, 1949, 1974, 1991, 2003), Wechsler Preschool and Primary Scale of Intelligence (Wechsler, 1967, 1989, 2002), and Woodcock-Johnson Tests of Cognitive Ability (Woodcock & Johnson, 1977, 1989; Woodcock, McGrew, & Mather, 2001).

Also, we completed a systematic literature review using PsycINFO, crossing the keywords *comparison, correlation,* and *validity* with the full and abbreviated titles of the measures. The first author reviewed each study in full unless abstract review determined the study was not relevant (e.g., some test validation studies included comparisons between tests not under consideration in this meta-analysis). A formal search for unpublished studies was not undertaken; it was presumed that the results of test validation studies would provide important information irrespective of the findings and would therefore constitute publishable data.

Coding Procedures

The first author, who had prior training and experience in coding studies for meta-analyses, coded all of the studies in the current meta-analysis. Two undergraduate volunteers were trained by the first author, and each volunteer coded half the studies. Agreement between the first author and the volunteers on each variable was calculated for blocks of 10 studies. These estimates ranged from 90.5 to 99.1% per block, with an average agreement of 95.8% per block. Discrepancies were resolved through discussion, during which the first author and volunteers referred to the original article. Discrepancies were commonly the result of a coder typo or failure of a coder to locate a particular value in an article.

Moderator Analyses

Moderators included ability level, age, test set, order of administration, and sample. Ability level was coded as the sample's score on the most recently normed test, and age was coded as the sample's age in months. Each comparison was assigned to a test set, as follows. First, due to Flynn's focus on the Stanford-Binet and Wechsler tests, these tests were grouped together and were further separated into an old set and a modern set. The old set included comparisons of only Wechsler and Stanford-Binet tests normed before 1972, with the modern set representing versions normed since 1972. The latter set aligned with comparisons published in Flynn and Weiss (2007) and Flynn (2009a). If a modern test was compared to an old test, the comparison was coded old. The Differential Ability Scales, Kaufman Adolescent and Adult Intelligence Test, and Woodcock-Johnson Tests of Cognitive Abilities were grouped together as non-Wechsler/Binet tests with modern standardization samples. The Kaufman Brief Intelligence Test and the Wechsler Abbreviated Scale of Intelligence were grouped together as screening tests. The Kaufman Assessment Battery for Children was separately analyzed due to its grounding in Luria's model of information processing that addressed differences in simultaneous and sequential processing. Fourteen effects remained from the original set of 285 after sorting effects into these groupings. All of these comparisons contained the McCarthy Scales but with multiple old and modern tests.

Order of administration was included as a moderator variable. Tests were frequently counterbalanced so that approximately half of the sample got each test first. However, in a substantial number of the studies, one test was uniformly given first. We coded these by the percentage of examinees given the old test first: 100 means that 100% of the examinees got the old test first; 0 means that all examinees got the new test first; 50 means that the tests were counterbalanced. In 7 of these effects, a different value was reported, and these were rounded to 0, 0.50, or 100. For example, 14% (given the old test first) was rounded to 0, and 94% was rounded to 100.

Each comparison was also grouped by study sample. *Standardization* studies were completed during standardization and were reported in test manuals. *Research* studies appeared in peer-reviewed journals and examined comparisons among a small selection of intelligence tests. *Clinical* studies reported results from assessments completed of clinical samples, including determination of special education needs.

Statistical Methods

Effect size metric. Comprehensive Meta-Analysis software (Borenstein, Hedges, Higgins, & Rothstein, 2005) was used for the core set of analyses. We employed the module that requires input of an effect size and its variance for each study. Effects were coded as the difference between the old test mean and the new test mean. Positive effects reflect a positive Flynn effect, with the score on the old test higher than the score on the new test despite being taken by the same individuals at approximately the same time. The effect size calculated from each study was the raw difference between the mean score on the old and new tests divided by the number of years between the norming dates of the two tests. This metric is directly interpretable as the estimated magnitude of the Flynn effect per year. Because the scales used by all of the tests were virtually the same ($M = 100$, $SD = 15$ or 16), no further standardization (such as dividing by population standard deviation [*SD*]) was required (Borenstein, Hedges, Higgins, & Rothstein, 2009). The actual *SD* for each test was used in computing the variance of the effects.

Effect size weighting. The variance for each effect is required for computation of the weight given to each effect in the overall analysis. The weight is the inverse of the variance, so studies with the smallest variance are given the most weight. Small variance (high precision) for an effect is achieved via (a) large sample sizes; (b) high reliabilities for both tests and high content overlap between tests, which are jointly reflected in the correlation between the tests; and (c) long intervals between the norming periods of the two tests. The formula (Borenstein et al., 2009) used for the variance of typical pretest–posttest effects in meta-analysis is

$$\text{Variance} = \frac{SD^2_{New} + SD^2_{Old} - 2rSD_{New}SD_{Old}}{x}, \tag{1}$$

where SD^2_{New} is the variance of the more recently normed test, SD^2_{Old} is the variance of the less recently normed test, r is the reported correlation between the two tests, and N is the total sample size. In the numerator, actual reported correlations were used when available. For 54 of the 285 studies, no correlation was reported. In these cases, if there were other studies that compared the same two tests, the correlations from the other studies were converted to Fisher's z. These were then averaged and converted back to a correlation and used in place of the missing value. If no other studies compared the same two tests, the mean correlation for the entire set of studies was computed and substituted in for the missing value. This occurred for two study results. The mean correlation for each pair of tests was also retained and used in a parallel analysis to determine the impact of using the sample-specific correlation rather than a population correlation in the estimator of the effect variance.

To allow for the differential precision in effects due to the years between norming periods of the two tests being compared, we adapted a formula from Raudenbush and Xiao-Feng (2001) that allows calculation of the change in variance as a function of the change in duration in years of the period between the norming of the two tests, holding number of time points constant. With D representing a duration of 1 year, D' representing a different duration, either longer or shorter, and $\omega = D'/D$ representing the factor of increase or decrease from 1 year, the proportion of the variances is equal to

$$\frac{V'}{V} = \frac{1}{\omega^2}. \tag{2}$$

In other words, the variance (V') for an effect with a 5-year duration between norming periods will be 1/25th the size of the variance (V) of an effect with a 1-year duration between norming periods, all other things being equal. Thus, the variance we entered into the Comprehensive Meta-Analysis software for each effect size was

$$\text{Variance} = \frac{SD^2_{New} - SD^2_{Old} - 2rSD_{New}SD_{Old}}{N\omega^2}. \tag{3}$$

The numerator of the above formula is the variance of the difference between the two tests being compared. The denominator adjusts this variance by the sample size (N) and by the duration in years of the period between the norming of the two tests.

Credibility intervals. In a random effects model, the true variance of effects is estimated. The standard deviation of this distribution is represented by tau [τ]. Tau is used to form a credibility interval around the mean effect, capturing 95% of the distribution of true effects by extending out 1.96 τ from the mean in both positive and negative directions. The credibility interval acknowledges that there is a distribution of true effects rather than one true effect. In interpreting the credibility interval, it is helpful to consider width as well as location. Even a distribution of true effects that is centered near 0 (where the mean effect might not be significant) may contain many members that might be meaningfully large in either direction. Moderator analysis may be used to try to find subsets of effects within this distribution, to narrow the uncertainty about how large the effect might be in a given situation; however, in the case of true random effects, each causal variable might explain a very small portion of the variance and moderator analysis might not improve prediction substantially.

Selection of random effects model. A random effects analytic model was employed because the studies were not strict replications of each other, in which case it would make sense to expect a single underlying fixed effect. Rather, the studies varied in multiple ways, each of which was expected to have some impact on the observed Flynn effect. These factors include but are not limited to (a) the specific test pair being compared, (b) the unique population being tested, (c) the age of the sample (which was not always reported quantitatively), (d) the interval between the presentation of the old and new test, (e) the order of presentation of the tests, (f) unusual administration practices (e.g., Spruill & Beck, 1988), and (g) interactions among these factors. The result of these multiple causes is a distribution of true effects, rather than a single effect.

In a random effects model, the mean effect is ultimately interpreted as the mean of a distribution of true population effects. Additionally, in a random effects model, the variance of the effects has two variance components. One is due to the true variance in population effects, and the second is due to sampling variance around the population mean effect. The result is that the weight given each study is a function of both within-study precision due to sample size and between-study variability. Sample size thus has less effect in the precision of each study. Large sample size studies are given less weight than they would have been in a fixed effects study, and studies with smaller samples are given more weight (Borenstein et al., 2009).

Heterogeneity in effect sizes. Heterogeneity describes the degree to which effect sizes vary between studies. The Q statistic is employed to capture the significance of this variance and is calculated by summing the squared differences between individual study effect sizes and the mean effect size. It is distributed as a chi-square statistic with $k - 1$ degrees of freedom, where k is the number of studies. In addition, I^2 is employed to capture the extent to which detected heterogeneity is due not to chance but to true, identifiable variation between studies. I^2 is calculated

$$I^2 = \left(Q - df\right)/Q, \tag{4}$$

and once multiplied by 100 is directly interpretable as the proportion of variance due to true heterogeneity.

Publication bias. We did not expect to find evidence for publication bias in this meta-analysis. The descriptive data collected from each study in the form of sample sizes, means, and correlations between tests is not typically the type of data that is subject to tests of significance and thus would not be a direct cause of failure to publish due to nonsignificance. Additionally, many of the effects were gleaned from the technical manuals of the tests being compared where no publication bias is expected. However, we did evaluate the distributions of effects within each portion of our analysis via funnel plots.

Results

Citations

The literature review produced a total of 4,383 articles. This total does not reflect unique articles, because each article would often appear in multiple keyword searches. One hundred fifty-four empirical studies and 27 test manuals met inclusion criteria, from which 378 comparisons were extracted, 285 of which were normed more than 5 years apart. The chronological range of the Flynn effect data collected was from 1951, upon publication of Weider, Noller, and Schramm's (1951) comparison study of the WISC and SB, to 2010, the year in which the literature review was completed. Table 1 shows the effect size produced by each of the 378 comparisons and includes information pertaining to sample size and age in months.

Overall Model

The mean effect over 285 total studies ($n = 14{,}031$) in the random effects model was 0.231 IQ points per year, 95% CI [0.20, 0.26], $z = 14.10$, $p < .0001$, with a confidence interval and p value indicating that the Flynn effect is different from zero.[1] The effects were significantly heterogeneous, $Q_{(284)} = 4{,}710$, $p < .0001$. The estimated I^2, or proportion of the total variance due to true study variance, was 0.94. The tau, or estimated standard deviation of the true effects, was 0.25, resulting in a credibility interval of −0.26 to 0.72. Eighty-two percent of the distribution of true effects was above zero.

Distribution of Effects

The effects were plotted against their standard error in a funnel plot (see Figure 1). There is no apparent publication bias, which would be represented by a gap on the lower left side of the plot. A similar absence of a gap is seen on the lower right side of the plot. What is most apparent in the funnel plot is that many effects fall outside the 1.96 standard error line, suggesting that there is important true heterogeneity in these effects that is not consistent with sampling error alone.

Moderator Analysis

We first modeled the significant heterogeneity in the effect sizes as a function of test set. There was a significant between-test group effect, $Q_{(5)} = 231$, $p < .0001$, with test group explaining 5.2% of the explainable variance in effects. We then regressed all effects on ability level using unrestricted maximum likelihood for mixed metaregression within Comprehensive Meta-Analysis software (Borenstein et al., 2005). The range of ability means in the set of effects was 40.6–132.7 standard score points. The intercept was significant ($a = 0.38$, $z = 2.58$, $p < .01$), but the slope was not ($b = -.002$, $z = -1.08$, $p < .28$), indicating that the effect did not change significantly over the range of ability levels represented in this set of effects.

Further Analysis Within Test Groups

We completed separate meta-analyses within test groups to place the results of the modern tests within the context of this larger set. This was done so we could meaningfully compare our results to Flynn's (1984b, 2009b) and Flynn and Weiss' (2007) results, which were based on data published after 1972. Because

[1] A systematic literature search for manual and empirical studies published since 2010 produced five new studies (Wechsler, 2011; Wechsler Abbreviated Scale of Intelligence [WASI] II vs. Kaufman Brief Intelligence Test [KBIT] II, WASI-II vs. WAIS-IV, WASI-II vs. WASI, WASI-II vs. WISC-IV; Wilson & Gilmore, 2012; WISC-IV vs. SB5), three of which included tests with norming dates at least five years apart. The mean effect over three studies with norming dates at least five years apart in the random effects model was 0.297 IQ points per year, 95% CI [.09, .51]. The mean effect over all five studies in the random effects model was 0.283 IQ points per year, 95% CI [.01, .47]. These results are consistent with the overall results.

Table 1
Sample Size, Sample Age, Tests Administered, and Effect Sizes by Study

Source	*N*	Age[a]	Newer test	Older test	Effect size
		Modern ≥5[b]			
1. Bower & Hayes (1995)	26	132.88	SB4	SB 72	0.08
2. Carvajal & Weyand (1986)	23	109.5	SB4	WISC–R	0.13
3. Carvajal et al. (1987)	32	227	SB4	WAIS–R	0.37
4. Clark et al. (1987)	47	63	SB4	SB 72	0.07
5. Doll & Boren (1993)	24	114	WISC–III	WISC–R	0.35
6. Gordon et al. (2010)	17	194	WISC–IV	WAIS–III	1.75
7. Gunter et al. (1995)	16	132	WISC–III	WISC–R	−0.19
8. Krohn & Lamp (1989)	89	59	SB4	SB 72	0.11
9. Lamp & Krohn (2001)	89	59	SB4	SB 72	0.10
10. Nelson & Dacey (1999)	42	248.04	SB4	WAIS–R	2.09
11. Quereshi & Seitz (1994)	72	75.1	WPPSI–R	WISC–R	0.34
12. Quereshi et al. (1989)	36	197.9	WAIS–R	WISC–R	0.1
13. Quereshi et al. (1989)	36	197.9	WAIS–R	WISC–R	1.11
14. Quereshi et al. (1989)	36	197.05	WAIS–R	WISC–R	0.18
15. Quereshi et al. (1989)	36	197.05	WAIS–R	WISC–R	1.11
16. Robinson & Nagle (1992)	75	111	SB4	WISC–R	0.25
17. Robinson et al. (1990)	28	30	SB4	SB 72	0.97
18. Roid (2003)	87	744	SB5	WAIS–III	0.91
19. Roid (2003)	66	132	SB5	WISC–III	0.41
20. Roid (2003)	71	48	SB5	WPPSI–R	−0.46
21. Roid (2003)	104	108	SB5	SB4	0.22
22. Roid (2003)	80	84	SB5	SB L-M	0.12
23. Rothlisberg (1987)	32	93.19	SB4	WISC–R	0.53
24. Sabatino & Spangler (1995)	51	163.2	WISC–III	WISC–R	−0.04
25. Sandoval et al. (1988)	30	197.5	WAIS–R	WISC–R	0.18
26. Sevier & Bain (1994)	35	110	WISC–III	WISC–R	0.76
27. Spruill (1991)	32		SB4	WAIS–R	2.24
28. Spruill (1991)	38		SB4	WAIS–R	2.14
29. Thompson & Sota (1998)	23	196	WISC–III	WAIS–R	0.60
30. Thompson & Sota (1998)	23	196	WISC–III	WAIS–R	−0.23
31. Thorndike et al. (1986)	21	234	SB4	WAIS–R	1.32
32. Thorndike et al. (1986)	47	233	SB4	WAIS–R	0.5
33. Thorndike et al. (1986)	205	113	SB4	WISC–R	0.21
34. Thorndike et al. (1986)	19	155	SB4	WISC–R	0.10
35. Thorndike et al. (1986)	90	132	SB4	WISC–R	0.23
36. Thorndike et al. (1986)	61	167	SB4	WISC–R	0.06
37. Thorndike et al. (1986)	139	83	SB4	SB 72	0.17
38. Thorndike et al. (1986)	82	88	SB4	SB 72	1.01
39. Thorndike et al. (1986)	14	100	SB4	SB 72	−0.22
40. Thorndike et al. (1986)	22	143	SB4	SB 72	−0.10
41. Urbina & Clayton (1991)	50	79	WPPSI–R	WISC–R	0.48
42. Wechsler (1981)	80	192	WAIS–R	WISC–R	0.15
43. Wechsler (1989)	50	79	WPPSI–R	WISC–R	0.47
44. Wechsler (1991)	189	192	WISC–III	WAIS–R	0.36
45. Wechsler (1991)	206	132	WISC–III	WISC–R	0.31
46. Wechsler (1997)	184	192	WAIS–III	WISC–III	−0.11
47. Wechsler (1997)	24	219.6	WAIS–III	WISC–R	0.33
48. Wechsler (1997)	26	343.2	WAIS–III	SB4	0.15
49. Wechsler (1997)	192	522	WAIS–III	WAIS–R	0.17
50. Wechsler (1997)	88	583.2	WAIS–III	WAIS–R	0.14
51. Wechsler (2002)	176	60	WPPSI–III	WPPSI–R	0.08
52. Wechsler (2003)	183	192	WISC–IV	WAIS–III	0.45
53. Wechsler (2003)	233	132	WISC–IV	WISC–III	0.19
54. Wechsler (2008)	238	632.4	WAIS–IV	WAIS–III	0.26
55. Wechsler (2008)	24	386.4	WAIS–IV	WAIS–III	0.37
56. Wechsler (2008)	24	348	WAIS–IV	WAIS–III	0.2
		Other ≥5[c]			
57. Appelbaum & Tuma (1977)	20	121	WISC–R	WISC	0.07
58. Appelbaum & Tuma (1977)	20	120	WISC–R	WISC	0.15
59. Arinoldo (1982)	20	57	MSCA	WPPSI	0.20

(*table continues*)

Table 1 (*continued*)

Source	N	Age[a]	Newer test	Older test	Effect size
60. Arnold & Wagner (1955)	50	102	WISC	SB 32	0.07
61. Axelrod & Naugle (1998)	200	519.6	KBIT	WAIS–R	−0.4
62. Barratt & Baumgarten (1957)	30	126	WISC	SB 32	0.58
63. Barratt & Baumgarten (1957)	30	126	WISC	SB 32	0.08
64. Bradway & Thompson (1962)	111	354	WAIS	SB 32	0.66
65. Brengelmann & Renny (1961)	75	442.92	WAIS	SB 32	−0.35
66. Brooks (1977)	30	96	WISC–R	WISC	0.29
67. Brooks (1977)	30	96	SB 72	WISC	0.37
68. Byrd & Buckhalt (1991)	46	149	DAS	WISC–R	0.12
69. Carvajal, Karr, et al. (1988)	21	69	SB4	MSCA	−0.1
70. Carvajal, Hardy, et al. (1988)	20	66	SB4	WPPSI	0.05
71. Chelune et al. (1987)	43	576	WAIS–R	WAIS	0.40
72. Cohen & Collier (1952)	51	89	WISC	SB 32	0.32
73. Covin (1977)	30	102	WISC–R	WISC	−0.00
74. Craft & Kronenberger (1979)	15	196.44	WISC–R	WAIS	0.72
75. Craft & Kronenberger (1979)	15	196.8	WISC–R	WAIS	0.54
76. Davis (1975)	53	69	MSCA	SB 60	0.57
77. Edwards & Klein (1984)	19	451.2	WAIS–R	WAIS	0.13
78. Edwards & Klein (1984)	19	451.2	WAIS–R	WAIS	0.37
79. Eisenstein & Engelhart (1997)	64	500.4	KBIT	WAIS–R	−0.25
80. Elliot (1990)	23	54	WPPSI–R	K-ABC	0.5
81. Elliot (1990)	49	41.5	DAS	MSCA	0.42
82. Elliot (1990)	40	42.5	DAS	MSCA	0.45
83. Elliot (1990)	66	110	DAS	WISC–R	0.50
84. Elliot (1990)	60	180	DAS	WISC–R	0.35
85. Elliot (1990)	23	54	DAS	K-ABC	0.67
86. Elliot (1990)	27	72	DAS	K-ABC	1.25
87. Faust & Hollingsworth (1991)	33	53.9	WPPSI–R	MSCA	0.07
88. Field & Sisley (1986)	17	360	WAIS–R	WAIS	0.22
89. Field & Sisley (1986)	25	360	WAIS–R	WAIS	0.25
90. Fourqurean (1987)	42	116	K-ABC	WISC–R	−0.71
91. Frandsen & Higginson (1951)	54	116	WISC	SB 32	0.21
92. Gehman & Matyas (1956)	60	182	WISC	SB 32	−0.10
93. Gehman & Matyas (1956)	60	133	WISC	SB 32	−0.12
94. Gerken & Hodapp (1992)	16	54	WPPSI–R	SB 60	0.08
95. Giannell & Freeburne (1963)	38	218.88	WAIS	SB 32	0.55
96. Giannell & Freeburne (1963)	36	219.96	WAIS	SB 32	0.50
97. Giannell & Freeburne (1963)	35	224.28	WAIS	SB 32	0.35
98. Hamm et al. (1976)	22	121.68	WISC–R	WISC	0.32
99. Hamm et al. (1976)	26	153.73	WISC–R	WISC	0.29
100. Hannon & Kicklighter (1970)	13	192	WAIS	WISC	−0.04
101. Hannon & Kicklighter (1970)	13	192	WAIS	WISC	−2.03
102. Hannon & Kicklighter (1970)	32	192	WAIS	WISC	0.95
103. Hannon & Kicklighter (1970)	33	192	WAIS	WISC	−0.50
104. Hannon & Kicklighter (1970)	11	192	WAIS	WISC	1.12
105. Hannon & Kicklighter (1970)	18	192	WAIS	WISC	1.03
106. Harrington et al. (1992)	10	48	WPPSI–R	WJTCA	−0.66
107. Harrington et al. (1992)	10	60	WPPSI–R	WJTCA	−0.16
108. Hartlage & Boone (1977)	42	126	WISC–R	WISC	0.20
109. Hartwig et al. (1987)	30	135.6	SB4	SB 60	−0.05
110. Hays et al. (2002)	85	408	WASI	KBIT	0.22
111. Holland (1953)	23		WISC	SB 32	0.10
112. Holland (1953)	29		WISC	SB 32	0.10
113. Jones (1962)	80	96	WISC	SB 32	0.54
114. Jones (1962)	80	108	WISC	SB 32	0.46
115. Jones (1962)	80	120	WISC	SB 32	0.38
116. Kangas & Bradway (1971)	48	498	SB 60	WAIS	−2
117. Kaplan et al. (1991)	30	57	WPPSI–R	WPPSI	0.36
118. Karr et al. (1992)	21	69	SB4	MSCA	−0.17
119. Karr et al. (1993)	32	63.6	WPPSI–R	MSCA	0.07
120. Kaufman & Kaufman (1990)	64	257	KBIT	WAIS–R	−0.11
121. Kaufman & Kaufman (1990)	41	66	KBIT	K-ABC	−0.13
122. Kaufman & Kaufman (1990)	35	128	KBIT	WISC–R	0.35
123. Kaufman & Kaufman (1990)	70	100	KBIT	K-ABC	0.07
124. Kaufman & Kaufman (1990)	39	136	KBIT	K-ABC	−0.48

(*table continues*)

Table 1 (*continued*)

Source	N	Age[a]	Newer test	Older test	Effect size
125. Kaufman & Kaufman (1993)	118	156	KAIT	WISC–R	0.23
126. Kaufman & Kaufman (1993)	71	208.8	KAIT	WAIS–R	0.14
127. Kaufman & Kaufman (1993)	108	312	KAIT	WAIS–R	0.21
128. Kaufman & Kaufman (1993)	90	494.4	KAIT	WAIS–R	0.47
129. Kaufman & Kaufman (1993)	74	747.6	KAIT	WAIS–R	0.25
130. Kaufman & Kaufman (1993)	124	135.6	KAIT	K-ABC	0.47
131. Kaufman & Kaufman (2004b)	54	68	KBIT-II	KBIT	0.10
132. Kaufman & Kaufman (2004a)	48	120	K-ABC-II	K-ABC	0.30
133. Kaufman & Kaufman (2004a)	119	126	K-ABC-II	WISC–III	0.09
134. Kaufman & Kaufman (2004a)	29	174	K-ABC-II	KAIT	0.13
135. Kaufman & Kaufman (2004b)	53	135	KBIT-II	KBIT	0.24
136. Kaufman & Kaufman (2004b)	74	383	KBIT-II	KBIT	0.16
137. Kaufman & Kaufman (2004b)	43	122	KBIT-II	WISC–III	0.24
138. Kaufman & Kaufman (2004b)	67	384	KBIT-II	WAIS–III	0.78
139. King & Smith (1972)	24	72	WPPSI	WISC	−0.15
140. King & Smith (1972)	24	72	SB 60	WISC	−0.51
141. Klanderman et al. (1985)	41	102	K-ABC	SB 72	0.56
142. Klanderman et al. (1985)	41	102	K-ABC	WISC–R	0.40
143. Klinge et al. (1976)	16	169.32	WISC–R	WISC	−0.12
144. Klinge et al. (1976)	16	169.32	WISC–R	WISC	0.40
145. Krohn et al. (1988)	38	51	K-ABC	SB 72	−0.32
146. Krohn & Lamp (1989)	89	59	K-ABC	SB 72	−0.12
147. Krugman et al. (1951)	38	60	WISC	SB 32	0.72
148. Krugman et al. (1951)	20	174	WISC	SB 32	0.24
149. Krugman et al. (1951)	38	72	WISC	SB 32	0.64
150. Krugman et al. (1951)	43	84	WISC	SB 32	0.25
151. Krugman et al. (1951)	44	96	WISC	SB 32	0.39
152. Krugman et al. (1951)	31	108	WISC	SB 32	0.66
153. Krugman et al. (1951)	29	120	WISC	SB 32	0.36
154. Krugman et al. (1951)	37	132	WISC	SB 32	0.42
155. Krugman et al. (1951)	22	144	WISC	SB 32	0.42
156. Krugman et al. (1951)	30	156	WISC	SB 32	0.42
157. Kureth et al. (1952)	50	60	WISC	SB 32	0.72
158. Kureth et al. (1952)	50	72	WISC	SB 32	0.36
159. Lamp & Krohn (2001)	89	59	K-ABC	SB 72	−0.11
160. Larrabee & Holroyd (1976)	24	129	WISC–R	WISC	0.25
161. Larrabee & Holroyd (1976)	14	129	WISC–R	WISC	0.50
162. Levinson (1959)	57	65.54	WISC	SB 32	0.76
163. Levinson (1959)	60	66.65	WISC	SB 32	0.66
164. Levinson (1960)	117	66.1	WISC	SB 32	0.71
165. Lippold & Claiborn (1983)	30	619.56	WAIS–R	WAIS	0.34
166. McCarthy (1972)	35	75	MSCA	SB 60	1.02
167. McCarthy (1972)	35	75	MSCA	WPPSI	0.36
168. McGinley (1981)	12	141	WISC–R	WISC	0.17
169. McGinley (1981)	9	141	WISC–R	WISC	0.37
170. McKerracher & Scott (1966)	31	384	SB 60	WAIS	0.64
171. Milrod & Rescorla (1991)	50	59	WPPSI–R	WPPSI	0.38
172. Milrod & Rescorla (1991)	30	59	WPPSI–R	WPPSI	0.05
173. Mishra & Brown (1983)	88	359.76	WAIS–R	WAIS	0.19
174. Mitchell et al. (1986)	35		WAIS–R	WAIS	0.15
175. Munford (1978)	10	141	WISC–R	WISC	0.04
176. Munford (1978)	10	141	WISC–R	WISC	−0.36
177. Munford & Munoz (1980)	11	150.5	WISC–R	WISC	−0.07
178. Munford & Munoz (1980)	9	150.5	WISC–R	WISC	0.34
179. Nagle & Lazarus (1979)	30	197.5	WISC–R	WAIS	0.69
180. Naglieri (1984)	35	105	K-ABC	WISC–R	−0.92
181. Naglieri (1984)	33	105	K-ABC	WISC–R	0.59
182. Naglieri (1985)	37	117	K-ABC	WISC–R	−0.83
183. Naglieri (1985)	51	91	K-ABC	MSCA	−0.11
184. Naglieri & Jensen (1987)	86	128.4	K-ABC	WISC–R	0.43
185. Naglieri & Jensen (1987)	86	129.6	K-ABC	WISC–R	0.08
186. Naugle et al. (1993)	200	519.6	KBIT	WAIS–R	−0.39
187. Oakland et al. (1971)	24	72	SB 60	WISC	−0.52
188. Oakland et al. (1971)	24	74	WPPSI	WISC	0.21
189. Oakland et al. (1971)	24	72	WPPSI	WISC	−0.15

(*table continues*)

Table 1 (*continued*)

Source	*N*	Age[a]	Newer test	Older test	Effect size
190. Oakland et al. (1971)	24	74	SB 60	WISC	0.02
191. Obrzut et al. (1984)	19	110.06	K-ABC	WISC–R	0.28
192. Obrzut et al. (1984)	13	111.06	K-ABC	WISC–R	−0.47
193. Obrzut et al. (1987)	29	114.96	K-ABC	SB 72	−0.38
194. Obrzut et al. (1987)	29	114.96	K-ABC	WISC–R	−0.88
195. Phelps et al. (1993)	40	108	WISC–III	K-ABC	1.00
196. Phillips et al. (1978)	60	73.92	MSCA	WPPSI	1.17
197. Pommer (1986)	56	87.86	K-ABC	WISC–R	−1.08
198. Prewett (1992)	40	189	KBIT	WISC–R	0.02
199. Prifitera & Ryan (1983)	32	529.08	WAIS–R	WAIS	0.31
200. Quereshi (1968)	124	180.1	WAIS	WISC	0.6
201. Quereshi & Miller (1970)	72	208.65	WAIS	WISC	0.49
202. Quereshi & McIntire (1984)	24	74.5	WPPSI	WISC	0
203. Quereshi & McIntire (1984)	24	74.5	WPPSI	WISC	0.50
204. Quereshi & McIntire (1984)	24	74.5	WPSSI	WISC	0.16
205. Quereshi & McIntire (1984)	24	74.5	WISC–R	WISC	−0.14
206. Quereshi & McIntire (1984)	24	74.5	WISC–R	WISC	0.25
207. Quereshi & McIntire (1984)	24	74.5	WISC–R	WISC	0.18
208. Quereshi & McIntire (1984)	24	74.5	WISC–R	WPPSI	−0.49
209. Quereshi & McIntire (1984)	24	74.5	WISC–R	WPPSI	−0.33
210. Quereshi & McIntire (1984)	24	74.5	WISC–R	WPPSI	0.23
211. Quereshi & Ostrowski (1985)	72	230.9	WAIS–R	WAIS	0.15
212. Quereshi & Erstad (1990)	36	891.6	WAIS–R	WAIS	0.64
213. Quereshi & Erstad (1990)	36	891.6	WAIS–R	WAIS	0.43
214. Quereshi & Erstad (1990)	18	1032	WAIS–R	WAIS	0.67
215. Quereshi & Erstad (1990)	27	906	WAIS–R	WAIS	0.57
216. Quereshi & Erstad (1990)	27	786	WAIS–R	WAIS	0.41
217. Quereshi & Seitz (1994)	72	75.1	WPPSI–R	WPPSI	0.40
218. Quereshi & Seitz (1994)	72	75.1	WISC–R	WPPSI	0.53
219. Rabourn (1983)	52	308.4	WAIS–R	WAIS	0.27
220. Reilly et al. (1985)	26	84	WJTCA	MSCA	−0.05
221. Reynolds & Hartlage (1979)	66	152.4	WISC–R	WISC	0.18
222. Rohrs & Haworth (1962)	46	149.88	SB 60	WISC	−0.33
223. Ross & Morledge (1967)	30	192	WAIS	WISC	−0.36
224. Rowe (1977)	20	170.5	WISC–R	WISC	0.016
225. Rowe (1977)	24	170.5	WISC–R	WISC	0.34
226. Rust & Yates (1997)	67	102	WISC–III	K-ABC	0.01
227. Schwaeting (1976)	58	126	WISC–R	WISC	0.30
228. Sewell (1977)	35	62.29	SB 72	WPPSI	0.61
229. Shahim (1992)	40	74.4	WISC–R	WPPSI	−0.22
230. Sherrets & Quattrocchi (1979)	13	141.6	WISC–R	WISC	0.05
231. Sherrets & Quattrocchi (1979)	15	141.6	WISC–R	WISC	0.20
232. Simon & Clopton (1984)	29	354	WAIS–R	WAIS	−0.08
233. Simpson (1970)	120	192	WAIS	WISC	−0.96
234. Skuy et al. (2000)	21	114	K-ABC	WISC–R	−2.13
235. Skuy et al. (2000)	35	100.8	K-ABC	WISC–R	−0.38
236. Smith (1983)	35	247.2	WAIS–R	WAIS	−0.21
237. Smith (1983)	35	247.2	WAIS–R	WAIS	0.51
238. Solly (1977)	12	124	WISC–R	WISC	0.50
239. Solly (1977)	12	124	WISC–R	WISC	0.43
240. Spruill & Beck (1988)	23	306	WAIS–R	WAIS	0.37
241. Spruill & Beck (1988)	35	306	WAIS–R	WAIS	0.19
242. Spruill & Beck (1988)	25	306	WAIS–R	WAIS	−0.05
243. Spruill & Beck (1988)	25	306	WAIS–R	WAIS	−0.24
244. Stokes et al. (1978)	59	147	WISC–R	WISC	0.10
245. Swerdlik (1978)	100	108	WISC–R	WISC	0.23
246. Swerdlik (1978)	64	163.2	WISC–R	WISC	0.20
247. Templer et al. (1985)	15	347.16	WAIS–R	SB 60	0.75
248. Thorndike et al. (1986)	75	66	SB4	WPPSI	0.24
249. Triggs & Cartee (1953)	46	60	WISC	SB 32	1.06
250. Tuma et al. (1978)	9	119	WISC–R	WISC	0.12
251. Tuma et al. (1978)	9	119	WISC–R	WISC	0.29
252. Tuma et al. (1978)	9	123	WISC–R	WISC	−0.04
253. Tuma et al. (1978)	9	123	WISC–R	WISC	0.27
254. Urbina et al. (1982)	68	505.92	WAIS–R	WAIS	0.21

(*table continues*)

Table 1 (*continued*)

Source	N	Age[a]	Newer test	Older test	Effect size
255. Valencia & Rothwell (1984)	39	54.9	MSCA	WPPSI	0.18
256. Valencia (1984)	42	59.5	K-ABC	WPPSI	−0.10
257. Walters & Weaver (2003)	20	278.4	WAIS–III	KBIT	−0.51
258. Wechsler (1955)	52	252	WAIS	SB 32	0.23
259. Wechsler (1974)	40	203	WISC–R	WAIS	0.33
260. Wechsler (1974)	50	72	WISC–R	WPPSI	0.34
261. Wechsler (1981)	72	474	WAIS–R	WAIS	0.30
262. Wechsler (1989)	61	63.5	WPPSI–R	WPPSI	0.50
263. Wechsler (1989)	83	63.5	WPPSI–R	WPPSI	0.20
264. Wechsler (1989)	93	62.5	WPPSI–R	MSCA	0.14
265. Wechsler (1989)	59	61	WPPSI–R	K-ABC	0.9
266. Wechsler (1999)	176	137.52	WASI	WISC–III	0.02
267. Weider et al. (1951)	44	77.5	WISC	SB 32	0.47
268. Weider et al. (1951)	62	119.5	WISC	SB 32	0.00
269. Weiner & Kaufman (1979)	46	110	WISC–R	WISC	0.32
270. Wheaton et al. (1980)	25	119.76	WISC–R	WISC	−0.01
271. Wheaton et al. (1980)	25	116.16	WISC–R	WISC	0.36
272. Whitworth & Gibbons (1986)	25	252	WAIS–R	WAIS	0.18
273. Whitworth & Gibbons (1986)	25	252	WAIS–R	WAIS	0.30
274. Whitworth & Gibbons (1986)	25	252	WAIS–R	WAIS	0.21
275. Whitworth & Chrisman (1987)	30	58	K-ABC	WPPSI	0.35
276. Whitworth & Chrisman (1987)	30	58	K-ABC	WPPSI	0.13
277. Woodcock et al. (2001)	150	117.5	WJTCA-III	WISC–III	0.57
278. Woodcock et al. (2001)	122	120.6	WJTCA-III	DAS	0.42
279. Yater et al. (1975)	20	80.5	WPPSI	WISC	−0.11
280. Yater et al. (1975)	20	63.45	WPPSI	WISC	0.23
281. Yater et al. (1975)	20	68.15	WPPSI	WISC	−0.24
282. Zimmerman & Woo-Sam (1974)	22	72	SB 72	WPPSI	−0.01
283. Zimmerman & Woo-Sam (1974)	22	66	SB 72	WPPSI	−0.5
284. Zins & Barnett (1984)	40	111	K-ABC	SB 72	0.28
285. Zins & Barnett (1984)	40	111	K-ABC	WISC–R	0.58
		Modern <5[d]			
286. Brooks (1977)	30	96	WISC–R	SB 72	−7.76
287. Carvajal et al. (1991)	51	68.4	WPPSI–R	SB4	2.36
288. Carvajal et al. (1993)	32	123	WISC–III	SB4	−0.74
289. Klanderman et al. (1985)	41	102	WISC–R	SB 72	6.16
290. Lavin (1996)	40	127.2	WISC–III	SB4	0.28
291. Lukens & Hurrell (1996)	31	161	WISC–III	SB4	2.05
292. McCrowell & Nagle (1994)	30	60	WPPSI–R	SB4	0.63
293. Obrzut et al. (1987)	29	114.96	WISC–R	SB 72	17.2
294. Prewett & Matavich (1994)	73	116	WISC–III	SB4	2.23
295. Rust & Lindstrom (1996)	57	111.6	WISC–III	SB4	−0.37
296. Sewell & Manni (1977)	33	84	WISC–R	SB 72	7.4
297. Sewell & Manni (1977)	73	144	WISC–R	SB 72	5.08
298. Simpson et al. (2002)	20	108	WISC–III	SB4	1.86
299. Simpson et al. (2002)	20	111	WISC–III	SB4	0.88
300. Wechsler (1974)	29	114	WISC–R	SB 72	6.8
301. Wechsler (1974)	27	150	WISC–R	SB 72	6.8
302. Wechsler (1974)	29	198	WISC–R	SB 72	−8.4
303. Wechsler (1974)	33	72	WISC–R	SB 72	10
304. Wechsler (1989)	115	70	WPPSI–R	SB4	0.69
305. Wechsler (1991)	188	72	WISC–III	WPPSI–R	−4
306. Wechsler (2003)	254	132	WISC–IV	WASI	0.85
307. Wechsler (2008)	141	198	WAIS–IV	WISC–IV	0.28
308. Zins & Barnett (1984)	40	111	WISC–R	SB 72	−10.24
		Other <5[e]			
309. Arffa et al. (1984)	60	55	WJTCA	SB 72	−0.86
310. Arinoldo (1982)	20	93	WISC–R	MSCA	−6.5
311. Axelrod (2002)	72	644.4	WASI	WAIS–III	−0.98
312. Barclay & Yater (1969)	50	63.84	WPPSI	SB 60	1.51

(*table continues*)

Table 1 (*continued*)

Source	*N*	Age[a]	Newer test	Older test	Effect size
313. Bracken et al. (1984)	99	143	WJTCA	WISC–R	2.14
314. Bracken et al. (1984)	37	143	WJTCA	WISC–R	1.44
315. Coleman & Harmer (1985)	54	108	WJTCA	WISC–R	1.32
316. Davis (1975)	53	69	SB 72	MSCA	0.4
317. Davis & Walker (1977)	51	97	WISC–R	MSCA	−1.6
318. Dumont et al. (2000)	81	148	DAS	WJTCA-R	−2.8
319. Elliot (1990)	62	63	DAS	WPPSI–R	10.8
320. Elliot (1990)	23	54	DAS	WPPSI–R	5.6
321. Elliot (1990)	58	60	DAS	SB4	0.8
322. Elliot (1990)	55	119	DAS	SB4	1.16
323. Elliot (1990)	29	103	DAS	SB4	1.93
324. Elliot (2007)	95	57.6	DAS-II	WPPSI–III	0.72
325. Estabrook (1984)	152	120	WJTCA	WISC–R	1.38
326. Fagan et al. (1969)	32	65	WPPSI	SB 60	1.62
327. Gregg & Hoy (1985)	50	268.8	WAIS–R	WJTCA	1.06
328. Harrington et al. (1992)	10	36	WPPSI–R	WJTCA-R	−16.4
329. Hayden et al. (1988)	32	111.6	SB4	K-ABC	−1.85
330. Hendershott et al. (1990)	36	48	SB4	K-ABC	1.81
331. Ingram & Hakari (1985)	33	124.8	WJTCA	WISC–R	0.70
332. Ipsen et al. (1983)	27	108	WJTCA	WISC–R	0.68
333. Ipsen et al. (1983)	19	108	WJTCA	WISC–R	0.65
334. Ipsen et al. (1983)	14	108	WJTCA	WISC–R	0.60
335. Kaufman & Kaufman (1993)	79	204	KAIT	SB4	0.14
336. Kaufman & Kaufman (2004a)	86	138	K-ABC-II	WJTCA-III	−0.09
337. Kaufman & Kaufman (2004a)	56	138	K-ABC-II	WISC–IV	−4.6
338. Kaufman & Kaufman (2004a)	36	42	K-ABC-II	WPPSI–III	−2.8
339. Kaufman & Kaufman (2004a)	39	66	K-ABC-II	WPPSI–III	−7.6
340. Kaufman & Kaufman (2004b)	80	136	KBIT-II	WASI	0.76
341. Kaufman & Kaufman (2004b)	62	512	KBIT-II	WASI	1
342. Kaufman & Kaufman (2004b)	63	130	KBIT-II	WISC–IV	−1.3
343. King & Smith (1972)	24	72	WPPSI	SB 60	0.74
344. Knight et al. (1990)	30	115	SB4	K-ABC	0.54
345. Krohn & Traxler (1979)	22	39	SB 72	MSCA	−1.2
346. Krohn & Traxler (1979)	24	54	SB 72	MSCA	−5.73
347. Krohn & Lamp (1989)	89	59	SB4	K-ABC	0.61
348. Lamp & Krohn (2001)	89	59	SB4	K-ABC	0.56
349. Lamp & Krohn (2001)	72	81	SB4	K-ABC	1.41
350. Lamp & Krohn (2001)	75	104	SB4	K-ABC	0.28
351. Law & Faison (1996)	30	182.4	KAIT	WISC–III	−17.4
352. Naglieri & Harrison (1979)	15	88	SB 72	MSCA	−24.26
353. Oakland et al. (1971)	24	74	WPPSI	SB 60	0.7
354. Oakland et al. (1971)	24	72	WPPSI	SB 60	0.76
355. Pasewark et al. (1971)	72	67.11	WPPSI	SB 60	0.78
356. Phelps et al. (1984)	55	188	WJTCA	WISC–R	0.54
357. Prosser & Crawford (1971)	50	58	WPPSI	SB 60	1.5
358. Reeve et al. (1979)	51	111	WJTCA	WISC–R	3.04
359. Reilly et al. (1985)	26	84	WISC–R	MSCA	2.5
360. Reilly et al. (1985)	26	84	WJTCA	WISC–R	−0.65
361. Rellas (1969)	26	76	WPPSI	SB 60	3.40
362. Roid (2003)	145	96	SB5	WJTCA-III	0.46
363. Smith et al. (1989)	18	125	SB4	K-ABC	0.48
364. Thompson & Brassard (1984)	20	122.4	WJTCA	WISC–R	0.25
365. Thompson & Brassard (1984)	20	120	WJTCA	WISC–R	2.21
366. Thompson & Brassard (1984)	20	120	WJTCA	WISC–R	2.47
367. Thorndike et al. (1986)	175	84	SB4	K-ABC	−0.09
368. Thorndike et al. (1986)	30	107	SB4	K-ABC	0.4
369. Vo et al. (1999)	30	147	KAIT	WISC–III	−1.34
370. Vo et al. (1999)	30	175	KAIT	WISC–III	−4.28
371. Wechsler (1967)	98	66.5	WPPSI	SB 60	0.34
372. Wechsler (1991)	27	108	WISC–III	DAS	−2.8
373. Wechsler (1999)	248	623.76	WASI	WAIS–III	−0.14
374. Ysseldyke et al. (1981)	50	123	WJTCA	WISC–R	1.80

(*table continues*)

Table 1 (*continued*)

Source	*N*	Age[a]	Newer test	Older test	Effect size
375. Zimmerman & Woo-Sam (1970)	26	72	WPPSI	SB 60	1
376. Zimmerman & Woo-Sam (1970)	21	72	WPPSI	SB 60	2.54
377. Zimmerman & Woo-Sam (1974)	22	72	WPPSI	SB 60	1.2
378. Zimmerman & Woo-Sam (1974)	22	66	WPPSI	SB 60	2.54

Note. SB4 = Stanford-Binet Intelligence Scales–Fourth Edition; SB 72 = Stanford-Binet Intelligence Scales–Form L-M (1972 norms ed.); WISC–R = Wechsler Intelligence Scale for Children—Revised; WAIS–R = Wechsler Adult Intelligence Scale—Revised; WISC–III = Wechsler Intelligence Scale for Children–Third Edition; WISC–IV = Wechsler Intelligence Scale for Children–Fourth Edition; WAIS–III = Wechsler Adult Intelligence Scale–Third Edition; WPPSI–R = Wechsler Preschool and Primary Scale of Intelligence—Revised; SB5 = Stanford-Binet Intelligence Scales–Fifth Edition; SB L-M = Stanford-Binet Intelligence Scales–Form L-M; WPPSI–III = Wechsler Preschool and Primary Scale of Intelligence–Third Edition; WAIS–IV = Wechsler Adult Intelligence Scale–Fourth Edition; WISC = Wechsler Intelligence Scale for Children; MSCA = McCarthy Scales of Children's Abilities; WPPSI = Wechsler Preschool and Primary Scale of Intelligence; SB 32 = Stanford-Binet Intelligence Scales–Form L; KBIT = Kaufman Brief Intelligence Test; WAIS = Wechsler Adult Intelligence Scale; DAS = Differential Ability Scales; SB 60 = Stanford-Binet Intelligence Scales–Form L-M (1960); K-ABC = Kaufman Assessment Battery for Children; WJTCA = Woodcock-Johnson Tests of Cognitive Abilities; KBIT-II = Kaufman Brief Intelligence Test–Second Edition; K-ABC-II = Kaufman Assessment Battery for Children–Second Edition; KAIT = Kaufman Adolescent and Adult Intelligence Test; WJTCA-R = Woodcock-Johnson Tests of Cognitive Abilities–Revised; DAS-II = Differential Ability Scales–Second Edition; WJTCA-III = Woodcock-Johnson Tests of Cognitive Abilities–Third Edition; WASI = Wechsler Abbreviated Scale of Intelligence.
[a] Age reported in months. [b] Modern comparisons with at least 5 years between test norming periods. [c] All other comparisons with at least 5 years between test norming periods. [d] Modern comparisons with less than 5 years between test norming periods. [e] All other comparisons with less than 5 years between test norming periods.

our focus is on the modern set, we conducted moderator analyses only within that set.

Older Wechsler/Binet tests. The mean effect (k = 152, n = 5,550) of studies involving Wechsler/Binet scales normed before 1972 (and including other IQ tests with an older normative basis) in the random effects model was 0.23 IQ points per year, 95% CI [0.19, 0.27], z = 11.12, p < .0001. The effects were significantly heterogeneous, $Q_{(151)}$ = 3,237, p < .0001. The estimated I^2, or proportion of the total variance due to true study variance, was .95, indicating that very little of the variance in observed effects was attributable to sampling error or unreliability in the tests. The tau, or estimated standard deviation of the true effects, was 0.24, indicating a 95% credibility interval of −0.23 to 0.70. In other words, approximately 84% of the distribution of true effects was above zero.

Screening tests. The mean effect (k = 17, n = 1,325) in the random effects model was 0.02 IQ points per year, 95% CI [−0.15, 0.19], z = 0.21, p < .84. Although the mean effect was not significantly different from 0, the effects were significantly heterogeneous, $Q_{(16)}$ = 232, p < .0001. The estimated I^2, or proportion of the total variance due to true study variance, was .93. The tau, or estimated standard deviation of the true effects, was 0.33, indicating a 95% credibility interval of −0.63 to 0.66, indicating that more than half of the true effects were above zero.

KABC tests. The mean effect (k = 34, n = 1,611) in the random effects model was 0.02 IQ points per year, 95% CI [−0.16, 0.19], z = 0.19, p = .85. Although the mean effect was not significantly different from zero, the effects were significantly heterogeneous, $Q_{(33)}$ = 295, p < .0001. The estimated I^2, or proportion of the total variance due to true study variance, was .89. The tau, or estimated standard deviation of the true effects, was 0.47, indicating a 95% credibility interval of −0.90 to 0.93. Again, more than half of the true effects were positive.

Other modern tests. The mean effect (k = 12, n = 925) for the modern tests other than Wechsler and Binet pairs normed since 1972 in the random effects model was 0.30 IQ points per year, 95% CI [0.21, 0.40], z = 6.13, p < .0001. Although the mean effect was significantly different from zero, the effects were significantly heterogeneous, $Q_{(11)}$ = 44, p < .0001. The estimated I^2, or proportion of the total variance due to true study variance, was .75. The tau, or estimated standard deviation of the true effects, was 0.14, indicating a credibility interval of 0.03 to 0.57. For the other modern effects, 98.6% of the true effects were positive.

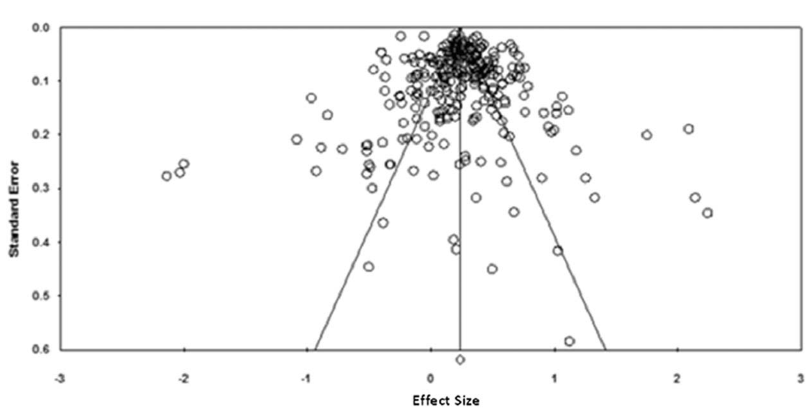

Figure 1. Study effect sizes and standard errors included in the overall model.

McCarthy test comparisons. The mean effect ($k = 14$, $n = 557$) in the random effects model involving the McCarthy was 0.33 IQ points per year, 95% CI [0.15, 0.51], $z = 3.60$, $p < .0001$. Although the mean effect was significantly different from zero, the effects were significantly heterogeneous, $Q_{(13)} = 74$, $p < .0001$. The estimated I^2, or proportion of the total variance due to true study variance, was .83. The tau, or estimated standard deviation of the true effects, was 0.28, indicating a credibility interval of −0.23 to 0.89. For this set of tests, 87.8% of the true effects were positive.

Modern Wechsler/Binet tests. The mean effect ($k = 56$, $n = 4{,}063$) for the Wechsler and Binet tests normed since 1972 in the random effects model was 0.35 IQ points per year, 95% CI [0.28, 0.42], $z = 10.06$, $p < .00001$. Although the mean effect was significantly different from zero, the effects were significantly heterogeneous, $Q_{(55)} = 597.34$, $p < .0001$. The estimated I^2, or proportion of the total variance due to true study variance, was .91. The tau, or estimated standard deviation of the true effects, was 0.23, indicating a credibility interval of −0.10 to 0.80. For the modern effects, 93.5% of the true effects were positive.

Moderator Analyses of the Modern Tests

Ability level. The first moderator selected to explore the significant heterogeneity of the modern tests was ability level. The significant mixed effects metaregression slope of effect size on ability level was $b = -.01$, 95% CI [–.016, –.004), $z = -3.37$, $p < .0007$. The Q for the model in this analysis was 11.38, accounting for 15.8% of the total variability as estimated by the unrestricted likelihood method.

Inspection of Figure 2 revealed an unusual bimodal pattern in the effects representing samples with the lowest ability. This pattern indicates that some of the lower ability samples had higher than average Flynn effects, whereas others had lower than average Flynn effects. In order to understand this pattern and its apparent contribution to the heterogeneity of the set of effects, we looked carefully at each of the 10 lowest ability studies. Of the five studies with the highest effect sizes in this group (Gordon, Duff, Davidson, & Whitaker, 2010; Nelson & Dacey, 1999; Spruill, 1991; Thorndike et al., 1986), four were comparisons between Stanford-Binet Fourth Edition (SB4) and Wechsler Adult Intelligence Scales—Revised (WAIS–R). The lowest possible score on the SB4 is 36, and the lowest possible score on the WAIS–R is 45. Individuals who obtain the lowest possible score on both tests will still have an apparent difference in their standard scores of 9 points. Consistent with the plot, as the scores get closer to the mean of 100, the differences in the scales become smaller, and the effects become smaller.

A different factor was noted in the three unusually low effects at the low ability side of the plot. For two of these effects, the administration of the tests was not counterbalanced. All participants received the old test first. It is possible that for these comparisons, the participants performed better on the second (newer) test than on the first due to an order effect (see below). Effects for the two noncounterbalanced studies fall below the regression line and are the second and fourth from the lowest in ability in that cluster. One (Thorndike et al., 1986) was a comparison of SB4 with Stanford-Binet L-M (floor = 36 points on both tests), and the other (Thorndike et al., 1986) was a comparison of SB4 with the Wechsler Intelligence Scales for Children—Revised (WISC–R). To evaluate the influence of these potentially highly influential but atypical effects to the analysis, we ran a cumulative analysis of the meta-analytic effect. We arranged all modern effects in descending order by ability level and then added them to the meta-analysis one at a time.

Figure 3 depicts a cumulative chart of all of the effects produced from the modern set, with scores ordered from left to right with ability on the horizontal axis and average effect size on the vertical axis. After inclusion of the study with the highest level of ability, the effect was approximately −0.05. With the addition of the second study, the average effect was about 0.45. By the time approximately 20 studies had been included, the effect stabilized, and when all but the lowest ability 10 studies had been included, the estimate was 0.28. The addition of the last effects did indeed have a large impact, bringing the overall mean back up to 0.35. Eliminating the three lowest ability effects results in a mean estimate of the remaining 53 effects ($n = 3{,}951$) of 0.293 points per year, 95% CI [0.23, 0.35], and the regression of effect on ability is no longer significant. The other five studies that are part of the bimodal distribution in Figure 2 do not appear to have significant impact on the overall estimate.

Age. Effect size was regressed on the average age of each sample in the set of 53 effects ($n = 3{,}951$) retained in the ability

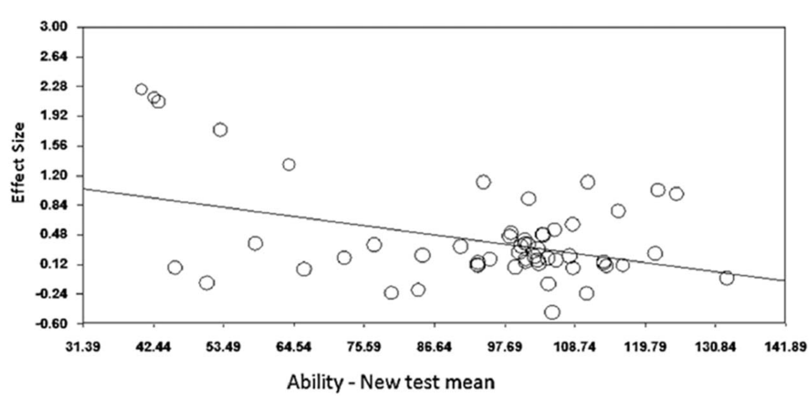

Figure 2. Study effect size regressed on sample ability in the modern set.

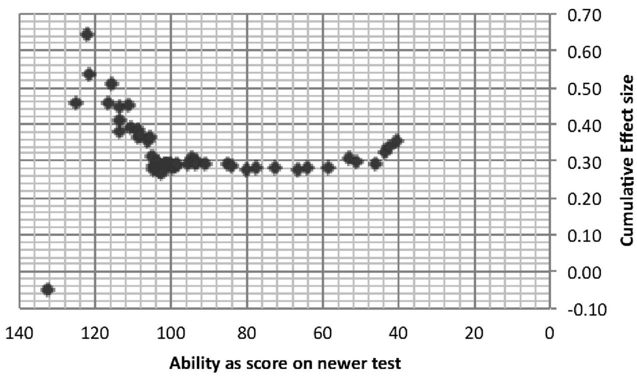

Figure 3. Cumulative Flynn effect by decreasing sample ability.

analysis above. The regression of effect size on age was nonsignificant, accounting for less than 1% of the variance in effect sizes.

Sample type. Each modern study ($k = 53$) was coded for sample type, which included clinical ($k = 1$, $n = 24$), research ($k = 22$, $n = 902$), and manuals ($k = 30$, $n = 3{,}025$). Because there was only 1 effect from a clinical sample, the moderator analysis was done on the remaining 52 effects. Although each group mean effect was significantly different from zero (see Table 2), type of sample was not significant in the random effects analysis, $Q_{(1)} = 3.14$, $p < .076$.

Order effects. Table 3 summarizes estimated Flynn effects (random effects model) by test group for studies that were counterbalanced. The pattern of effect sizes paralleled the overall study results for each test group. For the modern tests, summarized in Table 4, the estimate of 0.28 is close to the estimate of 0.29 for all 53 effects. Within the 53 modern effects, 50 provided information on test order. Most studies either uniformly gave the tests in the same order or counterbalanced so that half got the old test first and half got the new test first. The order effect was not significant in the random effects analysis, $Q_{(2)} = 4.30$ $p < .17$. The mean effects for the counterbalanced group ($k = 30$, $n = 2{,}912$, $M = 0.29$, 95% CI [0.23, 0.36]) and the group of effects where the old test was given second ($k = 8$, $n = 505$, $M = 0.54$, 95% CI [0.16, 0.91]) were significantly different from zero. The mean effect for the studies where the older test was given first ($k = 12$, $n = 396$) was not significantly different from zero ($M = 0.14$, 95% CI [–.04, 0.32]).

For the effects coded 100 where the old test was uniformly given first, negative effects due to prior exposure would be expected. In this ordering, Table 4 shows that prior exposure reduces the Flynn effect (.14 per year, *ns*). For effects coded 0, we would expect the mean effect to be amplified, reflecting a Flynn effect plus a prior exposure effect. Table 4 shows that the Flynn effect estimate is indeed larger (.54 per year). Finally, if the order was counterbalanced, the estimate should reflect the Flynn effect with less bias than either of the other two estimates. The estimate for the 30 counterbalanced groups is .29 per year. Although the order effect was not statistically significant, the estimates are different from 0 and the order test may not have been adequately powered. The patterns are consistent with hypothesis by Kaufman (2010).

Effect of pairing. Examining the counterbalanced tests permitted a comparison controlling for order effects when pairing Binet/Binet tests ($k = 8$, $n = 545$), Wechsler/Wechsler tests ($k = 18$, $n = 2{,}023$), and Wechsler/Binet tests ($k = 4$, $n = 344$). These comparisons yielded similar estimates close to the overall estimate of 0.293 per year: Binet/Binet: $M = 0.291$, 95% CI [0.14, 0.45]; Wechsler/Wechsler: $M = 0.296$, 95% CI [0.22, 0.38]; Wechsler/Binet: $M = 0.292$, 95% CI [0.17, 0.42].

Sensitivity Analysis

Finally, we explored the effect of our decisions on the results of the meta-analysis. First, the formula for the variance of each study included the sample-specific correlation between the two tests being compared in a given study. This correlation, however, is subject to sampling variance and to possible restriction of range within the sample studied. It is also potentially attenuated below the population correlation between the two tests if the administra-

Table 3
Flynn Effect by Test Group for Counterbalanced Administration Only

Group	*N*	Point estimate	*SE*	Lower CI	Upper CI
Modern SB/W[a]	30	0.29	0.03	0.23	0.36
Modern other[b]	7	0.33	0.08	0.17	0.49
Old SB/W[c]	81	0.26	0.03	0.21	0.31
K-ABC[d]	20	−0.08	0.14	−0.36	0.20
Screening[e]	6	0.09	0.06	−0.02	0.20
McCarthy[f]	12	0.36	0.11	0.15	0.56

Note. Atypical modern effects have been deleted from these analyses. *SE* = standard error; CI = 95% confidence interval; SB/W = Stanford-Binet/Wechsler; K-ABC = Kaufman Assessment Battery for Children.
[a] Modern SB/W effects include only Stanford-Binet and Wechsler tests normed in 1972 or later. [b] Modern other includes other tests normed in 1972 or later. [c] Old SB/W includes comparisons of Stanford-Binet and Wechsler tests only, where at least one test was normed before 1972. [d] K-ABC includes comparisons with the Kaufman Assessment Battery for Children test. [e] Screening includes effects on screening instruments. [f] McCarthy includes comparisons with the McCarthy Scales of Children's Abilities.

Table 2
Flynn Effect by Sample Type

Sample	*N*	*M*	*SE*	Lower CI	Upper CI	*z*	*p*<
Clinical	1	0.36	0.11	0.15	0.57	3.34	.001
Research	22	0.39	0.08	0.23	0.55	4.76	.0001
Manuals	30	0.23	0.03	0.17	0.30	7.11	.0001

Note. *SE* = standard error; CI = 95% confidence interval.

Table 4
Flynn Effect by Test Group for Modern Tests With Known Administration Order

Group	*N*	Point estimate	*SE*	Lower CI	Upper CI
Flynn effect plus practice effect	8	0.54	0.19	0.16	0.91
Flynn effect less practice effect	12	0.14	0.09	−0.04	0.32
Counterbalanced order	30	0.29	0.03	0.23	0.36

Note. CI = 95% confidence interval.

tion is done in such a way as to affect the actual reliability of the tests as given. For example, test directions might be misunderstood or misread, the testing environment might introduce distractions, or there might be inaccuracies in scoring. As an alternative, we calculated the average r for each pair of tests by converting all observed correlations to Fisher's z and averaging within test pairs, or by using the overall r, as above, if the specific study was missing the correlation and there were no other studies with the same test pair. For the overall analyses and within the test groups, mean effects differed by no more than 0.03 points per year. All significance tests and tests of heterogeneity resulted in the same conclusions reached above.

In addition to the 285 effects analyzed above, there were an additional 93 effects with norming gaps of 5 years or less. The mean effect over the combined 378 studies in the random effects model was 0.28 IQ points per year, 95% CI [0.25, 0.31], $z = 16.83$, $p < .0001$. The effects were significantly heterogeneous, $Q_{(377)} =$ 5,581, $p < .0001$. The estimated I^2, or proportion of the total variance due to true study variance, was .93, so very little of the variance in observed effects was attributable to sampling error or unreliability in the tests. The tau, or estimated standard deviation of the true effects, was 0.26, indicating a 95% credibility interval of -0.23 to 0.79. In other words, approximately 86% of the distribution of true effects was above zero. The funnel plot for the entire set of effects can be seen in Figure 4. Note that the 285 effects captured in Figure 1 constitute the tip of this pyramid. The range of standard errors in Figure 1 is from 0.0 to 0.6, whereas in Figure 4, the range is 0.0 to 20.0.

Discussion

Major Findings

The overall Flynn effect of 2.31 produced by this meta-analysis was lower than Flynn's (2009b) value of 3.11 and Fletcher et al.'s (2010) value of 2.80. It also fell below Dickinson and Hiscock's (2010) estimate of 2.60, which was the average of separate calculations for each of the 11 Wechsler subtests. However, our overall comparisons included all identified studies back to 1951. When a meta-analytic mean was calculated for the modern set (composed exclusively of 53 comparisons involving the Wechsler/Binet and excluding 3 atypical comparisons, and more comparable to the studies from Flynn, 2009a), the Flynn effect was 2.93 points per decade, a value larger than estimates based on studies that included older data. This value is the most reasonable estimate of the Flynn effect for Wechsler/Binet tests normed since 1972 and is similar to the 3 points per decade rule of thumb commonly recommended in practice. The standard error of this estimate is less than 1 point ($SE = 0.35$).

Moderator Analyses

Ability level. Defined as the score produced by the most recently normed IQ test, ability level did not explain a significant amount of variance in the Flynn effect in the overall model. Although the literature has produced inconsistent evidence with regard to the direction and/or linearity of the relation between ability level and mean Flynn effect (Graf & Hinton, 1994; Lynn & Hampson, 1986; Sanborn et al., 2003; Spitz, 1989; Teasdale & Owen, 1989;Zhou et al., 2010), the present data revealed no relation between these two variables in the overall analysis. This finding may be the result of a methodological difference between our meta-analysis, which treated ability level as a continuous variable, and previous studies, many of which treated ability level as a categorical variable.

Within the set of modern tests, ability level did explain a significant amount of variance in the Flynn effect, with lower ability samples producing higher Flynn effects. However, this was not a clearly reliable finding. The distribution of effects at lower ability levels was bimodal, with a subsample of comparisons producing higher than anticipated Flynn effects and another subsample of comparisons producing lower than anticipated Flynn effects. When the three effects with the lowest level of ability were deleted, ability was no longer a significant predictor of effect size. Thus, estimating the magnitude of the Flynn effect in lower ability individuals, for whom testing may have the greatest ramifications, appears to be more complex than estimating the magnitude of the Flynn effect in the remainder of the ability distribution. As noted previously, the distribution of Flynn effects that we observed at lower ability levels might be the result of artifacts found in studies of groups within this range of ability. When studies were added one at a time, we obtained stability at about 0.27–0.30 points per

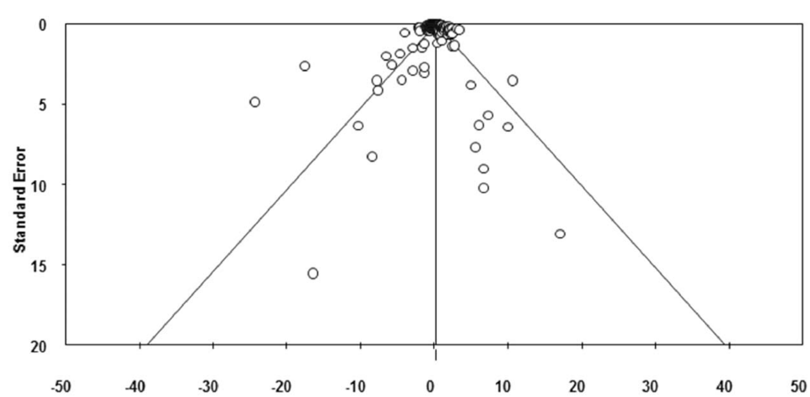

Figure 4. Complete set of study effect sizes and their standard errors.

year, with a mean of 0.293 points per year (excluding the three atypical low ability studies). These findings suggest that the mean magnitude of the Flynn effect may not change significantly with level of ability and that the correction can be applied to scores across the spectrum of ability level.

Age. Results revealed no difference in the Flynn effect based on participant age, suggesting that the Flynn effect is consistent across age cohorts. This finding is consistent with previous research (Flynn, 1984a, 1987).

Sample type. Although the sample type effect was not statistically significant, it was based on a small number of effects and the means were different from zero, with the patterns showing lower Flynn effect estimates for test manual than research studies. We might expect standardization samples to exercise the most control over variables related to participant selection, testing environment, and test administration procedures, so that the Flynn effect increases as control over these variables is relaxed. Because the sample size constituting the clinical set is so small ($k = 1$, $n = 24$), future research with a larger set of studies is needed.

Order of test administration. Test order was not a statistically significant moderator. However, the number of effects per comparison was small and the patterns were consistent with hypotheses by Kaufman (2010). For all test sets that were counterbalanced, the Flynn effect estimates were similar in magnitude and pattern across test sets to the overall estimates. In the modern set, where order varied, the effect for counterbalanced administrations only ($M = 0.293$, $k = 30$, $n = 2{,}912$) was the same as the overall estimate for the full set of modern tests ($M = 0.293$, $k = 53$, $n = 3{,}951$, excluding the three atypical low ability studies), reflecting the fact that the bulk of the effects ($k = 30$) were derived from counterbalanced studies. However, if the new test was given first, the estimate (0.54) was larger, reflecting the additive effects of prior exposure and norms obsolescence. If the old test was given first, the estimate (0.14) was smaller, reflecting the opposing influences of prior exposure and norms obsolescence. Our data do not address Kaufman's (2010) more specific concern about asymmetric order effects, such that taking the newer test first increased subsequent performance on the older test more than taking the older test first increases subsequent performance on the newer test. This putative pattern might be expected when the content or administration of an IQ test or subtest (e.g., Similarities subtest of the WISC–R) is changed in ways that could benefit a child who subsequently encounters the previous version of the same subtest. Given the variety of subtests underlying the IQ scores included in our meta-analyses, and the convergence of Flynn effect estimates around 0.29 for the modern tests, the order effect tends to be transitive with a mean magnitude of approximately $\pm$ .20. When the newer test is administered first, the Flynn effect estimate is approximately 0.29 + .20 and, when the older test is administered first, the Flynn effect estimate is approximately 0.35–.20.

Pairing. Examining just the modern tests administered in a counterbalanced order and excluding the three atypical studies showed that the estimates for pairings of Wechsler/Wechsler, Binet/Binet, and Wechsler/Binet tests (all about 0.29) were remarkably similar to the overall estimate of 0.293 per year. These results suggest that similar corrections can be made to different versions of the Wechsler and Binet tests normed since 1972.

Implications of the Flynn Effect for Theory and Practice

Theory.

Genetic hypotheses. As discussed above, there are multiple hypotheses about the basis of the Flynn effect, including genetic and environmental factors and measurement issues. Although genetic hypotheses have not gained much tractability, they make predictions about relations with age and cohort that can be compared to these results. The larger Flynn estimate in our study for newer than older tests provides no compelling support for the heterosis hypothesis.

Environmental factors. Our finding that the Flynn effect has not diminished over time and may be larger for modern than older tests is not consistent with Sundet et al.'s (2008) hypothesis relating increasing IQ scores and decreasing family size, although we do not have data for a direct evaluation.

The larger effect for modern than older tests could be regarded as consistent with Lynn's (2009) hypothesis pertaining to pre- and early postnatal nutrition. However, although we cannot directly address cohort effects in this meta-analysis, we note that the magnitude of increases in Wechsler and SB scores has remained close to the nominal value of 3 IQ points per decade since 1984 (Flynn, 2009a). Deviations from this constant value—such as the difference we found between modern and old tests—might indicate an IQ difference between older and younger cohorts, but they also might reflect other differences that have occurred over time, such as scaling changes, ceiling effects, or differences in the sampling of study participants (e.g., Hiscock, 2007; Kaufman, 2010).

Our study did not find evidence for the plateauing or decline of the Flynn effect in the United States, as has been documented in Norway (Sundet et al., 2004) and Denmark (Teasdale & Owen, 2005, 2008), respectively. Table 5.6 in the WAIS–IV manual (Wechsler, 2008) summarizes an excellent planned comparison of the WAIS–III (standardized in 1995) and the WAIS–IV (standardized in 2005) scores administered in counterbalanced order to 240 examinees. This table shows results similar to our meta-analysis, with average WAIS–III scores about 3 points higher than WAIS–IV scores. In addition, the effect was similar across age and ability level cohorts. To the extent that the United States and Scandinavia differ on at least the variables proposed to be related to the plateauing of scores in Scandinavia (e.g., family life factors, Sundet et al., 2004, and educational priorities, Teasdale & Owen, 2008, 2005), we might anticipate the difference in IQ score patterns noted. For example, Scandinavia's parental leave and subsidized child care might be indices of optimal socioenvironmental conditions and are generous relative to the United States. With regard to educational priorities, the relative value of a liberal arts education persists in the United States.

Measurement issues. Different types of tests yield different estimates of the Flynn effect. The effects were most apparent for multifactorial tests such as the Wechsler and Binet scales and extend to other modern tests with the exception of the KABC, which yielded little evidence of a Flynn effect. This is surprising, because the KABC minimizes the need for verbal responses and

Flynn effects tend to be relatively large for nonverbal tests such as the Wechsler Digit Symbol subtest (Dickinson & Hiscock, 2010). In addition, the variability of estimates for the KABC was very high, 95% CI [−0.16, 0.19], 95% credibility interval [–.90, .93]. Mean estimates were negligible for screening tests. This is surprising because most screening tests include matrix problem-solving tests, which historically have yielded large estimates for norms obsolescence. Again, the variability is high, 95% CI [−0.15, 0.19], 95% credibility interval [–.63, .66]). Altogether, these results suggest caution in estimating the degree of norms obsolescence for the KABC and different screening tests.

Practice.

Assessment and decision making. The results of this meta-analysis support the persistent findings of a significant and continuous elevation of IQ test norms as described by Flynn (1984a, 1987, 1998a, 1999, 2007). The rate of change obtained from the overall model was somewhat less pronounced than the 3 IQ points per decade typically cited. Nevertheless, when only the modern Wechsler/Binet tests were considered in isolation, the magnitude of the effect appears to be close to 3 points per decade and showed no evidence of reducing in magnitude. Our support for a robust Flynn effect, manifested across various tests in nearly 300 studies, underscores the importance of considering this factor in high-stakes decisions where the cut point on an IQ test is a salient criterion. These decisions include assessments for intellectual disability, which have implications for educational services received in schools, the death penalty, and financial assistance in cases where the individual is not competent to work.

Intellectual disability professionals have debated the necessity of correcting IQ scores for the Flynn effect in decisions about intellectual disability (e.g., Greenspan, 2006; Moore, 2006; Young, Boccaccini, Conroy, & Lawson, 2007). The present findings, which demonstrate the pervasiveness and stability of the Flynn effect across multiple tests and many decades, support the feasibility of correcting IQ according to the interval between norming and administration of the test (i.e., according to the degree to which the norms have become obsolete; Flynn, 2006b, 2009b). A precise correction, however, cannot be assured in all circumstances because the Flynn effect, as it applies to a given test, may strengthen or weaken at any time in the future. Moreover, the exact size of the Flynn effect may vary from one sample to another. Nonetheless, the rough approximation of 3 points per decade (plus or minus about 1 point based on the standard error and a 95% confidence interval) is consistent with the results of the modern studies in this meta-analysis.

Correction for the Flynn effect, although it increases the validity of the measured IQ (Flynn, 2006b, 2007, 2009b), does not justify using a conventional cut point as the sole criterion for determining intellectual disability (cf. Flynn & Widaman, 2008). In other words, increasing the validity of the measured IQ does not diminish the importance of other factors, including adaptive behavior. These include skills related to interpersonal effectiveness, activities of daily living, and the understanding of concepts such as money (AAIDD, 2010). Research has demonstrated a positive relation between IQ and measures of adaptive behavior (Bölte & Poustka, 2002; Schatz & Hamdan-Allen, 1995), and this supports the potential importance of considering both kinds of information when high-stakes decisions must be made (Flynn & Widaman, 2008).

The results of this meta-analysis suggest that examiners be mindful about the particular tests administered in situations where an individual is retested to assess for progress and to determine the necessity of special education services. The significant Flynn effect means that, when individuals are tested near the release of a newly normed assessment, the difference in IQ scores produced by the newer test and the older test would indicate that the individual is performing more poorly than what earlier testing may have suggested. A critical implication was highlighted in a recent article by Kanaya and Ceci (2012), who observed that children administered the WISC–R during a special education assessment and administered the WISC–III during a reevaluation were less likely to be rediagnosed with a learning disorder than were children administered the WISC–R on both occasions. Unawareness of the Flynn effect on the part of test examiners can compound this problem. For example, Gregory and Gregory (1994) raised concerns that, at the time of its publication, the Revised Neale Analysis of Reading Ability was producing lower scores than the older British Ability Scales (BAS) Word Reading scale. A critique of Gregory and Gregory's (1994) concerns by Halliwell and Feltham (1995) and possible explanations for the findings ensued, yet no mention of the possibility of norms obsolescence was presented. Our data show that norms obsolescence could have significant ramifications for the test results of students.

Further, in cases where an individual is assessed at two different sites (e.g., when a child moves and is assessed in a different school district), it may be possible for the child to have completed the newer version of a test first, especially if the assessments are occurring near to the release of a newly normed assessment. In this case, the IQ score produced by the second assessment may be particularly inflated due to both the Flynn effect and prior exposure. This child may be more likely to receive a diagnosis of a learning disability than a recommendation of special education services during this second assessment. This example underscores the importance of correcting for the Flynn effect in high-stakes decisions, a directive consistent with AAIDD's (2010) recommendation but addressed in few state special education standards for determining intellectual disability

Future research. The need for better estimates of the Flynn effect in research pertains to attempts to assess the breadth of the Flynn effect across cognitive domains. Several recent studies indicate that the Flynn effect is not limited to intelligence tests but may be measured in tests of memory (Baxendale, 2010; Rönnlund & Nilsson, 2008, 2009) and object naming (Connor, Spiro, Obler, & Albert, 2004), as well as certain commonly used neuropsychological tests (Dickinson & Hiscock, 2011). As Flynn effect estimates become more precise, it should be possible to differentiate not only the presence or absence of the effect but also gradations in the strength of the effect. Being able to quantify the magnitude of the Flynn effect in various domains would constitute an important advance toward answering the ultimate Flynn effect question (i.e., the underlying mechanism of the phenomenon).

From differences in the rates at which scores from the various Wechsler subtests have risen over time, Flynn (2007) has inferred characteristics of the intellectual skills that are rising rapidly and of the skills that are relatively static. We did not address this issue in this meta-analysis, partly because of the focus on the impact and precision of Flynn effect estimates for high-stakes decisions across a range of tests and partly because the greater impact of the Flynn effect on fluid versus crystallized intelligence is well established. More relevant would be additional knowledge about the strength of the Flynn effect on tests of memory and language and various neuropsychological tests, which would facilitate a more complete characterization of other higher mental functions that are susceptible to the Flynn effect in varying degrees. The data available from tests other than IQ tests are not likely to be sufficient in quality or quantity to yield precise Flynn effect estimates, but precise estimates for IQ tests will provide a reliable standard against which data from other tests can be evaluated.

Limitations

The objective in the current study was to build upon Flynn's (2009b) foundational work and Fletcher et al.'s (2010) meta-analytic study on the rate of IQ gain among modern Wechsler and Stanford-Binet tests per test manual validation studies by expanding the scope of investigation to other tests, eras, and samples. As such, the approach to the current study replicates the method of Flynn (2009b) and Fletcher et al. (2010) by examining intragroup change in IQ score as a function of the norming date of the test. An alternate approach, taken by Flynn (1987) and others since (e.g., Sundet et al., 2004, 2008), broadens the perspective from intragroup to intergroup change by focusing on draft board test performance within countries in the practice of administering IQ tests to all young men being assessed for suitability for conscription. For the study of a cohort phenomenon like the Flynn effect, this approach is appropriate. Unfortunately, no comparable data exist for American young men. Whereas the Raven's test administered to Scandinavian young men has not changed in format or content since its development, this is not the case for the Armed Services Vocational Aptitude Battery (arguably a measure of literacy rather than intelligence per se; Marks, 2010) administered to potential conscripts in the United States. In addition, the data collected from Scandinavian young men, most of whom are evaluated for suitability for the armed services, are more representative of the Scandinavian population than potential conscripts in the United States who self-select into the armed services are of the American population.

There are drawbacks to studying the Flynn effect on the basis of IQ test validation studies per the method of Flynn (2009b) and Fletcher et al. (2010): Sample sizes tend to be small; the earlier and later versions of the same test may differ significantly in format or content (e.g., Kaufman, 2010); there may be significant order effects; many tests are never renormed and therefore lie beyond the reach of this method; and direct within-examinee comparisons have not been made for many tests even if the tests have been renormed. In addition, validation studies rely on group-level data and presuppose a representative normative basis for the derivation of a standardized IQ score.

Even in the absence of speculation about the representativeness of a normative sample (see Flynn, 2009a, and Fletcher et al., 2010, for a discussion of the representativeness of the WAIS–III normative sample), normative sample sizes are significantly reduced once stratified by age. For example, 2,200 children constituted the WISC–IV standardization sample, from which were derived norms for subsets of 11 age groups. Similarly, 4,800 individuals constituted the SB5 standardization sample, from which were derived norms for subsets of 23 age groups.

Our alternative method involves relating mean scores on a test to the interval between norming and testing. This third method is capable of detecting changes in test performance over time without the need to track scores over many years or to restrict our analysis to tests for which repeated- measures data have been collected by test publishers. Our method is not as direct as Flynn's tracking of raw scores on Raven's Matrices, nor does it provide the detailed information that can be obtained by comparing old and new versions of the Wechsler and Stanford-Binet batteries in the same individuals. On the other hand, our method has the advantage of being applicable to a very large number of informative samples. Our study not only confirms the findings for the Wechsler and Stanford-Binet tests that were obtained using the second method, but it also expands those findings to include numerous tests on which the Flynn effect could not otherwise be assessed. The results show that the IQ increase is pervasive, not only with respect to geography and time but also with respect to the tests used to measure IQ. Our findings also suggest that the typical 6 IQ points per decade rise in Raven's Matrices score is unrepresentative of the Flynn effect magnitude measured with most other tests. Most of the tests included in our meta-analysis show rates of increase that are comparable to those measured for the Wechsler and Stanford-Binet batteries. Additionally, the large number of studies included in our meta-analysis provides a strong empirical basis for concluding that comparable IQ increases are evident in samples ranging from preschool children to elderly adults.

Relying on one numerical value to represent a continuous variable, including IQ score and age, results in a significant loss of information. For example, mean values can be greatly influenced by the number and magnitude of extreme values such that the resulting value may not be an adequate measure of central tendency nor an effective illustration of the relation between IQ score and the moderators assessed. Nonetheless, because the correction for the Flynn effect is a correction not to an individual score but to the normative basis to which individual scores are compared, concerns about applying group data to individual scores do not really apply (Flynn, 2006b).

The usefulness of a meta-analysis depends to a great extent on the accessibility of studies meeting inclusion criteria. Although a thorough review was conducted on PsycINFO and in test manuals, possibly there were studies meeting inclusion criteria that were not accessed. However, the number of comparisons included in this review appears more than sufficient to assess the magnitude of the Flynn effect and the precision of the obtained value and to address the additional research questions under consideration. Further, there was no dearth of effect sizes at the lower end of the distribution of effect sizes (see Figure 1), which suggests there was no oversampling of studies producing higher Flynn effects.

The homogeneity analysis indicated that there were sources of substantial heterogeneity among the studies included in the meta-analysis. In fact, 91% of the variance in the Flynn effect was due to true variance among studies. The selected moderator variables explained small amounts of the true variance in the modern set, suggesting that additional factors that explain variance in the Flynn effect have yet to be identified.

Conclusions

For the present, the need to correct IQ test scores for norms obsolescence in high-stakes decision making is abundantly clear. At average levels of IQ, a score difference of 95 and 98 is not critical. However, in capital punishment cases, life and death may reside on a 3-point difference of 76 versus 73, or 71 versus 68. This becomes especially important when IQ test scores are compared across a broad period of time and when IQ test scores obtained in childhood are brought to bear on an adult obtained score. Correcting for norms obsolescence is a form of scaling to the same standard. Weight standards often are adjusted each decade because people get larger over time. For these changes, the critical decision points are changed for obesity. For intellectually disability, we could (in theory) use the same test over time. Thus, if a child were assessed in 2013 with the WISC–R standardized in 1973, we could adjust the mean to 109 ($SD = 15$) and the cut point for intellectual disability to 79 (3 points). Because the convention in our society is to use a cut point of 70, corrections for norms obsolescence (i.e., the Flynn effect) must be made.

The existence of unknown factors that influence the Flynn effect should not obscure the major findings of this study: The mean value of the Flynn effect within the modern set centered around 3 points per decade, most of the estimated distribution of true effects was larger than zero, and the standard error of this estimate is 0.35 (resulting in a 95% CI that extends about .7, rounded to 1 point, on either side of 3 points per decade). These findings are consistent with previous research and with the argument that it is feasible and advisable to correct IQ scores for the Flynn effect in high-stakes decisions.

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