Article

Are Reading Interventions Effective for At-Risk Readers with ADHD? A Meta-Analysis

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Abstract

Objective: Utilizing a multi-level meta-analytic approach, this review is the first to systematically quantify the efficacy of reading interventions for school-aged children with ADHD and identify potential factors that may increase the success of reading-related interventions for these children. **Method:** 18 studies (15 peer-reviewed articles, 3 dissertations) published from 1986 to 2020 (*N* = 564) were meta-analyzed. **Results:** Findings revealed reading interventions are highly effective for improving reading skills based on both study-developed/curriculum-based measures (*g* = 1.91) and standardized/norm-referenced achievement tests (*g* = 1.11) in high-quality studies of children with rigorously-diagnosed ADHD. Reading interventions that include at least 30 hours of intervention targeting decoding/phonemic awareness meet all benchmarks to be considered a Level 1 (Well-Established) Evidence-Based Practice with Strong Research Support for children with ADHD based on clinical and special education criteria. **Conclusions:** Our findings collectively indicate that reading interventions should be the first-line treatment for reading difficulties among at-risk readers with ADHD. (*J. of Att. Dis. 2023; 27(2) 182-200*)

ADHD is a neurodevelopmental disorder that affects 5% of school-aged children (Polanczyk et al., 2014) and is associated with clinically significant impairments in the family, peer, and academic domains (Kofler et al., 2017) at an annual U.S. cost of illness of over $100 billion (Zhao, Page et al., 2019). Academic impairment is one of the foremost reasons for assessment and intervention for individuals with ADHD. An estimated 33% to 80% of children with ADHD demonstrate academic and learning difficulties (DuPaul & Langberg, 2015; Mayes & Calhoun, 2006) that frequently persist into adolescence and adulthood (Ek et al., 2011; Miranda et al., 2014). Reading difficulties are especially prominent in ADHD (for review, see Frazier et al., 2007), with 25% to 40% of children with ADHD presenting with a co-occurring diagnosis of reading disability (August & Garfinkel, 1990; DuPaul et al., 2013; Semrud-Clikeman et al., 1992). Notably, reading difficulties are apparent even in children with ADHD who do not meet formal criteria for reading disability (Ghelani et al., 2004). This is highly concerning given that studies have repeatedly demonstrated that early ADHD-related reading difficulties predict a host of negative outcomes, including later behavioral and conduct problems (Bennett et al., 2003; Fergusson et al., 1997), continued reading

difficulties into adolescence (McGee et al., 2002), increased rates of high school dropout (McGee et al., 2002), and later job instability (Maughan et al., 1985). Given the prevalence of reading underachievement in ADHD and associated negative outcomes (Loe & Feldman, 2007), it is imperative to evaluate the efficacy of reading interventions for at-risk readers with ADHD, as well as potential factors that may increase the success of reading-related interventions for these children.

ADHD and Reading Interventions

Presently, empirically supported treatments for ADHD primarily consist of psychostimulant medication and behavioral interventions (Evans et al., 2018; Pelham et al., 2022; Raggi & Chronis, 2006). These interventions demonstrate

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robust effects for decreasing ADHD symptoms (Fabiano et al., 2015; Swanson et al., 1995; cf. Sonuga-Barke et al., 2013). In contrast, these first-line interventions demonstrate minimal to no benefits for academic achievement broadly or reading achievement specifically (Boland et al., 2020; Fabiano et al., 2015; Kortekaas-Rijlaarsdam et al., 2019; Majewicz-Hefley & Carlson, 2007; MTA Cooperative Group, 1999; Rapport et al., 1994; Van der Oord et al., 2008). This replicated finding may be surprising given that the prevailing clinical model of ADHD (American Psychological Association, 2013) considers reading/academic underachievement to be a functional outcome of core ADHD symptoms (see Rapport et al., 2000). However, these findings indicate clearly that alternative approaches are needed to remediate these children's reading difficulties (Tamm et al., 2017; Tannock et al., 2018).

To that end, interventions that directly target reading skills reflect a highly promising option for at-risk readers with ADHD, given decades of replicated evidence for the efficacy of reading interventions within the developmental and educational literatures (e.g., Al Otaiba & Fuchs, 2002; Ron Nelson et al., 2003; Tran et al., 2011). Reading interventions provide direct and explicit instruction on core reading skills (for review, see National Reading Panel (US), National Institute of Child Health, Human Development (US), National Reading Excellence Initiative, National Institute for Literacy (US), & United States Department of Health, 2000). A primary goal of reading is to glean meaning and draw inferences from written text—a skill referred to as reading comprehension (Sideridis et al., 2006). According to the influential "simple view of reading," children's reading comprehension can be fully explained by two interrelated components (for review see Lonigan et al., 2018): decoding (i.e., converting written symbols to speech sounds to form words) and language comprehension (i.e., comprehension of language when not presented as text, such as oral language; Gough & Tunmer, 1986). Given children with ADHD experience deficits across all components of the simple view of reading (Friedman et al., 2017; Korrel et al., 2017; Miller et al., 2013, 2014), it can be hypothesized that reading interventions specifically targeting component(s) of the simple view of reading will lead to better reading outcomes. Indeed, within the developmental/educational literatures reading interventions that target "simple view" component(s) have been found to produce robust downstream effects on reading comprehension (National Reading Panel (US), National Institute of Child Health, Human Development (US), National Reading Excellence Initiative, National Institute for Literacy (US), & United States Department of Health, 2000). Moreover, such benefits have been found for children across socioeconomic status (Bus et al., 1995) and intellectual abilities (Hill, 2016), though reading interventions that intervene at younger ages generally result in larger benefits (Goodwin & Ahn, 2013; Hall & Burns, 2018)—at least for at-risk readers without ADHD.

Nevertheless, by nature of their neurodevelopmental diagnosis, children with ADHD are at an increased risk for attentional, behavioral, and neurocognitive difficulties that may impact learning—i.e., it is likely that reading problems in ADHD are due, at least in part, to interfering behaviors or alternate underlying causes rather than solely the skill-based deficits targeted by extant reading interventions (e.g., Aduen et al., 2018; Kofler et al., 2018). As such, (a) at-risk readers with ADHD may be less responsive to reading interventions than their non-ADHD peers (Rabiner & Malone, 2004; Ron Nelson et al., 2003), to the extent that reading difficulties are caused/maintained by different underlying mechanisms for children with ADHD than they are for children without ADHD; and (b) the effectiveness of the core components of these interventions may vary for children with and without ADHD. Indeed, the ADHD reading intervention literature is characterized by high levels of variability in outcomes, with some studies reporting that reading interventions are highly effective (e.g., $g=2.14$; Lane et al., 2001), while other studies report minimal (e.g., $g=0.15$–0.23; Gum, 2003) and even iatrogenic (e.g., $g=-0.86$; Hendricks, 2020) treatment outcomes.

This heterogeneity may reflect the wide range of methodologies and interventions studied. For example, intervention dosage—though a nonsignificant predictor of reading intervention efficacy in studies not recruiting based on ADHD status (Hall & Burns, 2018; Okkinga et al., 2018)—may affect reading intervention outcomes for children with ADHD (e.g., Miller et al., 2013). The hypothesized benefits of higher intervention dosage for children with ADHD may occur because children with ADHD are off-task during academic instruction about 25% of the time (Kofler et al., 2008), and demonstrate more frequent moment-to-moment shifts between attentive and inattentive behavior (Kofler et al., 2008; Rapport et al., 2009). In that context, it stands to reason that they may need to spend more time in treatment to obtain the same results as their more attentive peers.

Similarly, it has been hypothesized that adding ADHD treatment (e.g., behavioral management, psychostimulant medication) to reading interventions should produce greater improvements in reading by reducing the interfering effects of ADHD behaviors on learning (Bental & Tirosh, 2008; Williamson et al., 2014). However, the available evidence suggests that combining psychostimulant medication and/or behavioral management training with reading interventions yields no incremental benefits on reading outcomes relative to reading intervention alone (e.g., Denton et al., 2020; Strong Hilsmier et al., 2016; Tamm et al., 2017; Tannock et al., 2018).

ADHD reading intervention studies also differ markedly in terms of how they assess efficacy (e.g., standardized achievement tests versus study-developed/curriculum-based measures of reading); which component(s) of reading they target (e.g., phonetic decoding versus reading comprehension); the presence and quality of their control group(s); as well as the rigor of their ADHD diagnostic method (e.g., single-informant/method versus gold-standard multi-informant/method)—all of which have been shown to affect estimates of ADHD-related impairments and/or response to intervention in prior meta-analyses (e.g., Alderson et al., 2007; Kofler et al., 2008). A meta-analysis is clearly needed to determine the extent to which reading interventions work for at-risk readers with ADHD, as well as examine intervention-specific, methodological, and child factors that may maximize the efficacy of reading interventions for this population.

Prior Meta-Analyses

To our knowledge, no prior meta-analyses have examined reading interventions for school-aged children with ADHD specifically. However, two recent meta-analyses reported on reading interventions for struggling readers with emotional and/or behavioral disorders more generally (Benner et al., 2010; Roberts et al., 2020). Both studies found large improvements in word reading and reading comprehension ($g = 0.90–1.02$) but did not directly assess the extent to which these effects held specifically for children with ADHD. Interestingly, while Benner et al. (2010) did not test moderators of treatment outcome, Roberts et al. (2020) found that combining reading intervention with behavioral supports produced significantly *smaller* improvements in reading (i.e., $g = 0.26$ vs. $g = 1.29$), whereas study quality and design, participant grade, and intervention group size did not moderate treatment effects. From these findings, Roberts et al. (2020) concluded that adding behavioral interventions to reading treatment was unsupported. Taken together, both the Benner et al. (2010) and Roberts et al. (2020) meta-analyses supported the use of reading interventions for children with emotional and/or behavioral disorders *broadly*, but neither review specifically examined the efficacy of reading interventions for children with ADHD.

A partial exception to this critique is a recent meta-analysis of primarily single case design studies by Stewart and Austin (2020), who examined the efficacy of reading interventions that target higher-level reading skills such as comprehension, vocabulary, and fluency for older children with ADHD in grades 4 to 12. The authors found moderate treatment gains across 16 ADHD reading intervention studies (single-subject Tau-U $= 0.33–1.19$; between group $g = 0.41$ and 2.63). However, while Stewart and Austin's (2020) study represents an important initial step, the conclusions that can be drawn from this study appear limited due to their restricted sample size (65 total participants across 16 included studies) and exclusion of studies targeting key lower-order reading skills (e.g., decoding; Foorman et al., 2016). Similarly, Stewart and Austin's (2020) conclusion that reading interventions do not meet criteria as an evidence-based practice for children with ADHD warrants scrutiny due to their restrictive inclusion criteria, which resulted in the exclusion of all three randomized control trials (RCT) published during their specified dates of review. In addition, the authors excluded studies that included children younger than fourth grade, which may significantly limit conclusions that can be drawn given (a) evidence from non-ADHD samples that earlier reading intervention is likely to produce more robust improvements (Goodwin & Ahn, 2013; Hall & Burns, 2018), and that (b) the majority of research on reading interventions for children with ADHD has been conducted with samples that include children both younger and older than fourth grade (e.g., Tamm et al., 2017; Tannock et al., 2018).

Current Study

Taken together, reading problems are highly prevalent in children with ADHD (Daucourt et al., 2020; Willcutt et al., 2010), and untreated ADHD-related reading difficulties portend adverse near- and long-term behavioral (Bennett et al., 2003; Fergusson et al., 1997), academic (McGee et al., 2002), and occupational difficulties (Maughan et al., 1985). However, evidence-based treatments targeting ADHD behavioral symptoms generally do not produce downstream improvements in reading skills as noted above, and the evidence for reading-specific interventions for this high-risk population is surprisingly mixed.

The current meta-analysis systematically examined both peer-reviewed and unpublished literature to (a) quantify the extent to which reading interventions are effective for at-risk readers with ADHD; (b) investigate participant, study quality and design, and intervention characteristics associated with maximum improvement in reading skills for children with ADHD; and (c) evaluate the extent to which reading interventions can be classified as an evidence-based treatment for children with ADHD based on both clinical (Evans et al., 2018) and special education (Cook et al., 2015) standards.

Based upon the attentional, behavioral, and neurocognitive difficulties associated with ADHD that impact reading skill acquisition and performance (Kofler et al., 2019; Rucklidge & Tannock, 2002), we hypothesized that extant reading interventions would be effective for children with ADHD, but at a smaller magnitude than seen in children without ADHD. To our knowledge, no previous review has examined moderators of reading intervention efficacy for children with ADHD. However, based on prior work demonstrating minimal/no direct benefits of psychostimulant

medication and behavioral treatment on academic outcomes (e.g., Van der Oord et al., 2008), we hypothesized that adding ADHD treatment to reading intervention would not provide additive benefits relative to reading intervention alone. Finally, based on the influential "simple view of reading" model (e.g., Lonigan et al., 2018), we hypothesized that interventions targeting decoding and/or language comprehension skills would be associated with greater reading improvements than interventions targeting other aspects of reading.

Method

Transparency and Openness

We report how we determined our sample size, all data inclusion/exclusion criteria, all manipulations, and all measures in the study (Simmons et al., 2012). The meta-analysis was not preregistered; we transparently report our a priori plan and minor changes made after accessing the data below. Data were analyzed using the R statistical program "Metafor" package (RStudio Team, 2020; Viechtbauer, 2010). Study data can be found in Tables 1 and 2.

Literature Searches

Study selection was conducted following the Preferred Items for Systematic Reviews and Meta-analysis guidelines (PRISMA; Moher et al., 2015; see Figure 1). Multiple search methods were applied to include all relevant literature, not just published articles (McAuley et al., 2000; Rosenthal, 1994). First, comprehensive literature searches of both peer-reviewed studies as well as unpublished dissertations and theses were conducted using PsycINFO, ERIC, and ProQuest Dissertations and Theses. Search terms included permutations of the ADHD diagnostic label (ADHD, attention, hyperactiv*, inattent*, hyperkinesis, impulsivity, minimal brain dysfunction, minimal brain damage, MBD), reading intervention, reading treat*, RTI, response to intervention, and reading instruction. An asterisk after a root word directs search engines to include any derivative of the word that is followed by the asterisk. To avoid missing studies due to database misclassification, no search delimiters were selected. Searches were conducted by the first, second, and third authors (Chan, Shero, Hand). All included studies were reviewed by at least two of these authors, and disagreements were resolved via discussion after all three authors reviewed the study (100% consensus). After the initial searches, emails were sent to authors of both peer-reviewed studies and unpublished dissertations that examined reading interventions among children with ADHD or elevated ADHD symptoms but did not report sufficient data for effect size calculation (three of eight authors did not respond, four authors responded and indicated that

data were no longer accessible, and one author provided the requested data). Studies were additionally identified via (a) referral by experts on academic interventions for children with ADHD as well as (b) from articles cited in prior meta-analyses or systematic reviews on reading interventions for children with ADHD and/or emotional/behavioral concerns. Collectively, these procedures generated an initial database of 3,312 items including chapters, peer-reviewed articles, dissertations, and theses written since 1937. All search processes and study recruitment were terminated on June 21, 2021 (Figure 1).

Inclusion Criteria

Inclusion and exclusion criteria are described below with the number of studies omitted for each criterion in parentheses. The initial exclusion process included removing duplicates (132) and items that were not empirical studies (e.g., book chapters, letters to the editor, policy briefs, manuals; 787). Among the remaining studies, several studies failed to meet multiple inclusion criteria; the counts below indicate first failed criteria identified. The following served as inclusion criteria for the review: Studies (a) conducted in English (7) that included (b) an intervention targeting a reading skill (2,056) for children and/or adolescents with a diagnosis of ADHD or related labels (e.g., attention problems, hyperactive) and co-occurring reading difficulties (302) that (c) used performance-based measures of reading achievement (11) from which (d) effect sizes could be estimated based on reported data or test statistics or could be obtained from the study author (6). Exclusion criteria included: (a) participants with gross neurological, sensory, or motor impairment, history of a seizure disorder, psychosis, or intellectual disability (3); (b) studies of second-language English learners (2); and (c) single case study designs (6). Single-case study designs were excluded because single-case effect sizes are incompatible with effect sizes derived from between-group studies (Rindskopf et al., 2012), and because they have been summarized recently by Stewart and Austin (2020).

A total of 18 studies (15 peer-reviewed studies, 3 dissertations) published from 1986 to 2020 met all study criteria and were included in one or more sets of analyses.[1] Nine of these 18 studies reported data for two or more independent subsamples (defined as ADHD samples with non-overlapping participants), resulting in $k = 24$ independent samples reporting on $N = 564$ participants. Several studies reported multiple outcome variables, resulting in a total of 57 prepost intervention effect sizes (1–5 effect sizes per independent sample).

Coding of Moderators

All potential moderator variables were coded by the first and second authors according to the characteristics reported

Table 1. Participant Characteristics.

Study first-author	N	Mean age years /grade	% Male	% Med	IQ	% High SES	% White	% SLD-RD	% ESE	Self-contained (Y/N)
Clarfield and Stoner (2005)	3	0	1.0	0.67	-	-	1.0	-	0.67	N
Crabtree et al. (2010)	2	2	1	-	-	-	1	1	1	N
Denton et al. (2020) 1	72	1	0.67	0	86.2	0.22	0.18	-	0.36	N
Denton et al. (2020) 2	63	1	0.47	1	86.7	0.27	0.23	-	0.42	N
Gum (2003)	8	2	0.88	0.38	-	-	-	0.38	1	N
Hedin et al. (2011)	2	1	1.0	1.0	-	-	-	-	-	N
Hendricks 1 (2020)	4	1	-	-	-	-	-	-	-	N
Hendricks 2 (2020)	7	1	-	-	-	-	-	-	-	N
Johnson et al. (2012)	2	2	1	1	-	-	1	0	1	N
Lane et al. (2001)	7	0	0.71	-	87.9	-	0.57	-	0	N
Luckey (2009)	135	1	-	-	-	-	-	0	-	N
Raffaele Mendez et al. (2016)	2	0	1.0	-	-	-	1.0	-	0	N
Richardson et al. (1987)	42	-	-	1	-	-	-	1	0	N
Rogevich and Perin (2008)	15	2	1	-	92.4	0	0.47	-	1	Y
Saddler et al. (2017)	2	1	1	0.5	-	-	0	-	1	Y
Shimabukuro (1996)	3	2	1	-	-	-	-	1	1	Y
Strong Hilsmier et al. (2016)	2	1	0.5	-	-	-	-	-	1	Y
Tamm et al. (2017) 1	66	1	0.47	1	86.7	0.27	0.23	-	0.42	N
Tamm et al. (2017) 2	73	1	0.67	0	86.2	0.22	0.18	-	0.36	N
Tannock et al. (2018) 1	17	0	0.83	1	92.9	-	-	1	0	N
Tannock et al. (2018) 2	10	0	0.92	1	90.1	-	-	1	0	N
Tannock et al. (2018) 3	11	1	0.7	0	87.1	-	-	1	0	N
Tannock et al. (2018) 4	13	0	0.64	0	97.2	-	-	1	0	N
Walcott et al. (2014)	3	0	1	-	-	-	0.67	-	0	N

Note. "-"=not reported; Mean age years/grade: 0=mean age of ≤8.5 years or grades K-2; 1=mean age of 8.6 to 11.0 years or grades 3 to 5 or; 2=mean age 11+ years or grades 6+; %Male=percentage of the sample male; %Med=percentage of the sample taking psychostimulant medication; IQ=standard scores from standardized/norm-referenced IQ tests; SES=socioeconomic status; %White=percentage of the sample white; %SLD-RD=percentage of the sample diagnosed with a DSM-V specific learning disorder in reading; %ESE=percentage of the sample receiving exceptional student education services; Self-contained (Y/N)=whether students in the sample are placed in a self-contained classroom.

in Tables 1 and 2. Each study was coded on several domains (defined below), including subject characteristics, study design and treatment characteristics, and outcome measures. Continuous variables were used whenever possible (Hedges & Pigott, 2004). Fifty percent of studies were double-coded by the first and second authors for reliability, which was excellent (r=.997); discrepancies were resolved collaboratively.

Study and sample characteristics. Study characteristics were defined as follows: continuous demographic variables included publication year and the percentage of each sample who were (a) taking psychostimulant medication for ADHD and (b) receiving exceptional student education (ESE) services based on each study's chosen indicator. Most studies defined ESE status in terms of whether children had a pre-existing Individualized Education Plan (IEP), received additional help from a paraprofessional, or were diagnosed with a reading disability. In addition, a subset of studies included children placed in a self-contained classroom, which was coded as an additional dichotomous variable given that this reflects a more restrictive educational placement that may reflect more severe learning and/or behavioral difficulties (Fuchs & Fuchs, 2006).

A publication year of 2020 was assigned for studies where unpublished data were provided by authors. For child age/grade, a continuous measure was not feasible due to inconsistent reporting across studies. As such, an ordinal variable was created by splitting studies into three approximately equal groups (0=Grades K-2 or mean age of ≤8.5 years; 1=Grades 3–5 or mean age of 8.6–11.0; 2=Grades 6+ or mean age 11+ years). We also initially coded child IQ (reported in only k=12 of 24 samples), SES (k=5 of 24 samples), race/ethnicity (k=11 of 24 samples), and co-occurring reading disability status (k=12 of 24 samples) but these factors could not be examined as potential moderators due to limited reporting across studies/samples.

Diagnostic method was coded ordinally as an index of study quality based on the recommendations for gold standard diagnosis of ADHD in prior meta-analytic reviews (e.g., Alderson et al., 2007; Kofler et al., 2008, 2016; Lipszyc & Schachar, 2010), with higher values reflecting more rigorous diagnostic procedures: 0=referral or prior diagnosis only; 1=single informant rating scale and/or interview; 2=multiple informants and settings (multiple informant report based on standardized/norm-referenced questionnaires and/or clinical interviews). We also initially coded additional indices of study rigor including (a) use of

Table 2. Study Characteristics.

Study first-author	Mean effect (g)	Intervention target	Treatment outcome	Treatment match outcome (Y/N/both)	Measure	Diagnostic rigor (0/1/2)	Read plus behavior (Y/N/both)	Treatment format	RCT (Y/N)	Control group (Y/N)	Treatment dosage (Min)	Treatment duration (Weeks)
Clarfield and Stoner (2005)	2.03	Decoding	Decoding	Y	Standardized	0	N	Individual	N	N	800	6–9
Crabtree et al. (2010)	1.55	Comp	Comp	Y	CBM	1	N	Group	N	N	157.5	-
Denton et al. (2020)	0.36	Multi	Both	Y	Standardized	2	Both	Group	Y	Y	2,475	16
Gum (2003)	–0.08	Comp	Both	Both	CBM	0	N	Individual	N	N	701	9–12
Hedin et al. (2011)	2.19	Comp	Comp	Y	CBM	1	N	Individual	N	N	420	14
Hendricks (2020)	0.53	Comp	Both	Both	Standardized	0	N	Group	N	N	700	14–15
Johnson et al. (2012)	2.54	Comp	Comp	Y	CBM	1	N	Group	N	N	195	-
Lane et al. (2001)	1.82	Decoding	Decoding	Y	Standardized	0	N	Group	N	N	900	10
Luckey (2009)	0.80	Decoding	Decoding	Y	Standardized	1	N	Individual	N	N	6,000	12–20
Raffaele Mendez et al. (2016)	0.67	Decoding	Both	Both	Standardized	0	N	Group	N	N	2,240	16
Richardson et al. (1987)	0.69	Decoding	Comp	N	Standardized	0	N	Group	N	N	-	-
Rogevich and Perin (2008)	2.74	Comp	Comp	Y	CBM	1	N	Group	N	Y	225	1–2
Saddler et al. (2017)	–0.10	Comp	Comp	Y	CBM	1	N	Group	N	N	210	3–4
Shimabukuro (1996)	2.61	Comp	Comp	Y	CBM	1	N	Group	N	N	362.5	10
Strong Hilsmier et al. (2016)	0.22	Decoding	Both	Both	Standardized	1	Both	Individual	N	N	960	12
Tamm et al. (2017)	1.12	Multi	Both	Y	Standardized	1	Both	Group	Y	Y	2,880	16
Tannock et al. (2018)	0.36	Decoding	Both	Both	Standardized	2	N	Group	Y	Y	2,100	10
Walcott et al. (2014)	3.18	Decoding	Decoding	Y	Standardized	0	N	Group	N	N	320	4

Note. "-" = not reported; g = hedge's g; g = 0.2 is a small effect; g = 0.5 is a medium effect; g = 0.8 or greater is a large effect; Comp = comprehension; Multi = multiple reading skills (e.g., decoding + reading comprehension); Treatment Match Outcome (Y/N/both) = whether target of intervention matched assessed treatment outcome; Standardized = standardized/norm-referenced assessment; CBM = study-developed/curriculum-based measures; Diagnostic Rigor = higher values reflect greater diagnostic rigor; 0 = single informant/questionnaire; 1 = referral/prior diagnoses; 2 = gold standard diagnostic practice; Read Plus Behavior (Y/N) = reading intervention with adjunctive behavioral intervention; RCT (Y/N) = whether intervention study was a RCT; Treatment Format = whether treatment delivered in an individual or small group/combined setting; Control Group (Y/N) = whether study had a control group; Treatment Dosage (Min) = average total minutes of treatment; Treatment Duration (Weeks) = average number of weeks to complete treatment.
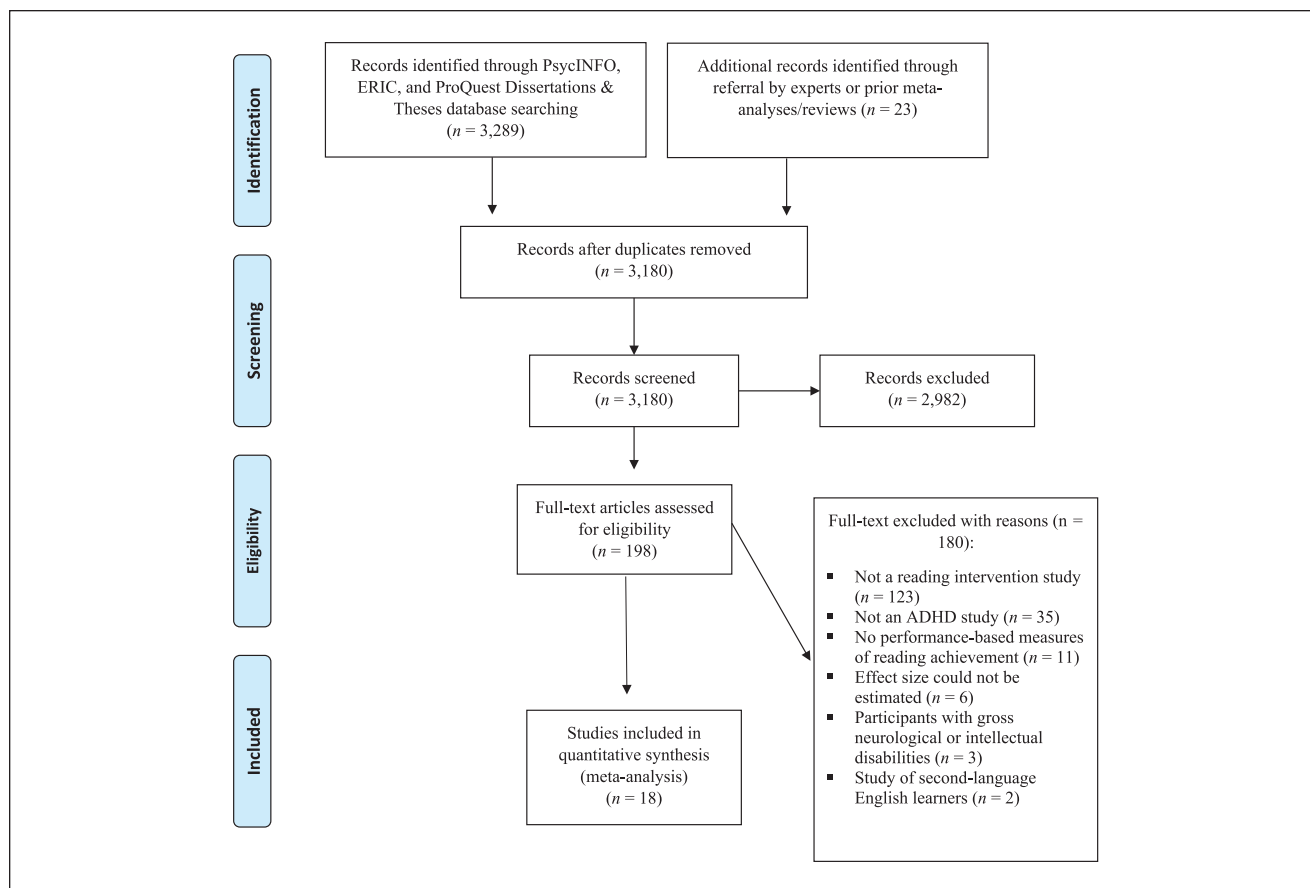
Figure 1. PRISMA flow diagram of included studies.

a randomized control trial design, (b) inclusion of any control group, and (c) inclusion of a gold-standard active, credible control group (all 0=No; 1=Yes). However, these variables were found to be multicollinear with diagnostic method (and each other) and were therefore excluded (i.e., studies using gold standard ADHD diagnostic practices also tended to include active, credible control groups, and RCT methods; $r > .90, p < .001$). Diagnostic method was retained over these alternate indices of study quality because our primary aim was to quantify reading intervention outcomes unique to children with ADHD, and less rigorous diagnostic practices have been shown to produce "ADHD" groups that contain a large percentage of children without ADHD (e.g., Alderson et al., 2007; Sciutto & Eisenberg, 2007). Nonetheless, given this multicollinearity, we focus our interpretation of this moderator on both diagnostic and methodological quality in the Discussion.

Intervention and outcome characteristics. Intervention dosage was coded as a continuous variable based on the mean (or midpoint for studies only reporting a range) number of minutes of intervention. Two variables were coded to assess the potential incremental benefits of adding ADHD treatment

to reading interventions: (a) Intervention type was coded as 0="reading intervention only" versus 1="reading intervention + behavioral treatment" and (b) the percentage of the sample who received psychostimulant medication during treatment as described above.

All other intervention characteristics were coded dichotomously/dummy coded: Intervention target (Decoding/Phonemic Awareness, Multicomponent, or Reading Comprehension) was dummy coded as a set of $k-1$ variables with Decoding/Phonemic Awareness as the reference variable. Studies were coded as Multicomponent if they targeted multiple reading skills (e.g., decoding + reading comprehension). Intervention outcome was coded as 0="simple view of reading component skills" (i.e., decoding skills) versus 1="reading comprehension". Intervention target and outcome were coded based on the simple view of reading model as described above (Lonigan et al., 2018). We had intended to also include language comprehension here, but none of the included studies directly targeted non-print-based understanding of language. We also created a dichotomous variable to code whether the treatment target matched the treatment outcome (0=No; 1=Yes) given the expectation that "near

transfer" gains on skills directly taught in the intervention would be greater than "far transfer" gains on skills not explicitly taught in the intervention (e.g., Simons et al., 2016).

Finally, we coded whether each study assessed intervention effects using standardized measures of reading achievement (coded as 0) or curriculum-based measures of reading skills (coded as 1). We expected that curriculum-based measures would be associated with larger effect sizes given that they are study-created to match the intervention content, whereas standard scores from norm-referenced tests would be associated with smaller effect sizes given that they were not developed to assess change and may be particularly insensitive to detecting intervention effects for children with neurodevelopmental disorders (see Farmer et al., 2020). We had intended to also code whether studies reported outcomes based on item response theory-based growth scale values or person ability scores (which are based on the same norming samples as standard scores but were developed explicitly to assess response to intervention; Farmer et al., 2020; Kaufman & Kaufman, 2014), but no studies used this preferred metric.

Delivery format (individual=0 vs. group or combined group + individual=1) was also coded initially, but was multicollinear with exceptional student education (ESE) status (i.e., studies with higher proportions of children with ESE status tended to deliver the intervention individually, presumably due to the more impaired nature of the sample; $r=.80$, $p<.001$). ESE status was retained given prior evidence that ESE status may be associated with smaller academic gains (Bussing et al., 2012).

Computation of Effect Sizes

Means, *SD*s, and sample sizes for each group were used to compute Hedges' *g* effect sizes using the R statistical program "Metafor" package (RStudio Team, 2020; Viechtbauer, 2010). For one study (Richardson et al., 1987), means and *SD*s were unavailable and thus effect sizes were estimated using the reported sample size and *p*-value (Alderson et al., 2007). Hedges' *g* effect sizes are Cohen's *d* effect sizes corrected for study sample size due to the upward bias in effect size magnitude of small *N* studies (2013). An effect size of 0.20 is considered small, 0.50 is medium, and 0.80 is large (Cohen, 2013). A random effects model was chosen due to the wide variance in reading outcomes and intervention types. The random effects model weights each effect size by its inverse variance weight ($1/SE^2$) to correct for study-level sampling error as recommended (Hunter et al., 2004; Lipsey & Wilson, 2001).

Missingness. For all studies, Hedges' *g* values were either computed given the data presented within the article ($k=51$) or were provided directly from the article author ($k=6$). For two studies, estimates of variance were not reported, but were instead estimated using multiple imputation as laid out by van Van Buuren (2018) so that these effect sizes could be used for subsequent analyses. Multiple imputation was done using the "MICE" package in R (Van Buuren & Groothuis-Oudshoorn, 2011), which has been effectively used to account for missing data in other meta-analytic projects (e.g., Jolani, 2012). Missingness was also an issue for several moderators within this project, and as such multiple imputation using the same process and package was used to impute missing values on a sample-by-sample basis. To limit bias from multiple imputation, a 50% cutoff of missingness was used to determine which variables were eligible for multiple imputation as recommended (Lee & Carlin, 2012; Mishra & Khare, 2014). As such, variables with missingness of 50% of independent samples or greater were excluded from use as noted above (i.e., IQ, SES, ethnicity, and percentage of the sample with co-occurring reading disability); all other potential moderators were reported in at least 94% of independent samples and therefore missingness was handled via multiple imputation as described above.

Publication bias: The file drawer problem. To assess for publication bias, three separate analyses were conducted. First, results were visualized using a funnel plot. Next, Egger et al.'s (1997) test was computed for detection of publication bias. Finally, the fail-safe N was computed (Rosenthal, 1979). The fail-safe N represents the number of studies with null effects that would have to be included in the analysis to significantly reduce the obtained effect size enough that its corresponding *p*-value becomes non-significant.

Moderator analyses. Potential moderators were examined via the R package "Metafor" (RStudio Team, 2020; Viechtbauer, 2010). Several studies produced multiple effect sizes, and thus a three-level (multi-level) modeling approach was selected to account for nesting of effect sizes as recommended (Assink & Wibbelink, 2016). The three-level model separates the proportion of variance attributable to the study, sample, and individual level for each effect size. The three-level model was selected over traditional meta-regression, as (a) the former allows for the inclusion of multiple effect sizes from a single study while controlling for dependency between effect sizes and (b) importantly avoids the artificial decrease in heterogeneity that occurs when averaging effect sizes within studies (Cheung, 2014; Hox, 2010).

Results

Overview

Publication bias was assessed, followed by heterogeneity tests and then tests of moderators when warranted. Next, "best case" estimates were computed based on the results of the moderator meta-regression (Lipsey & Wilson, 2001).

Table 3. Histogram of Hedges' *g* Effect Sizes.

Stem	Effect size							
4	*4.60*							
. . .								
2	*2.03*	*2.14*	**2.19**	**2.54**	**2.61**	**2.74**		
. . .								
1	1.24	1.50	**1.55**	1.76	1.79			
0.9	*0.97*	0.98						
0.8	*0.80*	*0.83*						
0.7	0.70	0.78						
0.6	*0.61*	0.69						
0.5	*0.51*	0.52	*0.53*	0.58				
0.4	0.40	0.42	0.43	0.44	0.45	0.46	0.47	
0.3	*0.32*	0.33	0.34	0.34	*0.36*	0.38	*0.38*	*0.39*
0.2	*0.21*	0.23	0.23	0.23	*0.24*	0.25	0.28	
0.1	*0.11*	0.15	*0.15*	*0.16*				
0								
−0	−0.02	−0.03	−0.10					
−0.1								
−0.2	−0.27							
−0.3								
−0.4	−0.43	−0.47						
−0.5								
−0.6	−0.61							
−0.7								
−0.8	−0.86							
−0.9								

Note. Histogram of effect sizes across studies. The left column reflects the ones and tenths digits, and the right columns reflect each obtained effect size. Each entry indicates one unique effect size. **Bold** font represents curriculum-based measures and un-bolded font represents standardized measures. *Italic* font indicates that the treatment targeted decoding skills. Positive values indicate positive treatment gains.

Finally, the included studies were scrutinized using established criteria to determine whether reading interventions could be considered evidence-based practice for at-risk readers with ADHD (Cook et al., 2015; Evans et al., 2018).

Publication Bias

Visual inspection of the stem-and-leaf histogram of obtained effect sizes (Table 3) indicated clear differences between studies based on outcome test selection (i.e., standardized/norm-referenced academic achievement tests versus study-developed/curriculum-based academic skill measures). Overall, however, the funnel plot indicated a fairly symmetrical distribution of effect sizes across studies indicating no evidence of publication bias. Similarly, neither the Egger's test ($z = 1.94$, $p > .05$) nor the fail-safe $N = 2{,}418$ provided significant evidence of publication bias.

Tier I: Overall (moderator independent) estimate of reading intervention efficacy. Reading interventions produced overall large magnitude benefits for children with ADHD ($g = 0.96$, 95% CI [0.56, 1.36]). However, the overall test of heterogeneity was significant ($Q[56] = 152.46$, $p < .0001$), with a large proportion of this variability (40.9%) attributable to between-study differences. This heterogeneity indicated that the overall effect size was likely not the best estimate of reading intervention efficacy for children with ADHD and supported the examination of potential moderators.

Tier II: Study, treatment, and demographic moderators of between-study differences

Outcome test selection. Outcome test selection (i.e., standardized/norm-referenced tests vs. study-developed/curriculum-based measures) was tested first as a potential moderator because visual inspection of the data indicated clear differences in effect sizes based on this study characteristic as noted above (Table 2). As expected, test selection was a significant moderator ($p = .001$), with curriculum-based measures ($g = 1.91$, 95% CI [0.92, 2.89]) associated with significantly larger effect sizes than norm-referenced achievement tests ($g = 0.66$, 95% CI [0.37, 0.95]).

Table 4. Overall Effects and Moderators of Effects.

	k	Hedges' g	95% CI
Overall Hedges' g effect size	57	0.96	0.56–1.36
CBM measures	7	1.91	0.92–2.89
Standardized measures			
Overall (moderator independent) effect	50	0.66	0.37–0.95
Moderated (best case) estimate	50	1.11	–
		B-weights	**95% CI**
Moderated Hedges' g effect sizes for standardized outcome tests			
Intercept		3.54	
Sample characteristics			
Age		−0.09	−0.72 to 0.53
Medication status (%)		0.03	−0.41 to 0.47
ESE status (%)		−0.30	−1.32 to 0.71
Self-contained classroom (N/Y)		−2.03	−3.43 to −0.63**
Study characteristics			
Diagnostic rigor		−0.89	−1.15 to −0.62***
Intervention target *(Dummy Coded)*			
Reading Comprehension vs. decoding		−1.26	−2.20 to −0.32*
Multicomponent vs. decoding		0.15	−0.29 to 0.58
Outcome characteristics			
Treatment outcome		0.13	−0.13 to 0.39
Treatment target matches treatment outcome (N/Y)		0.29	−0.12 to 0.69
Intervention characteristics			
Added behavioral treatment (N/Y)		0.16	−0.33 to 0.65
Treatment duration (minutes)		−0.35	−0.57 to −0.13**

Note. Effect sizes are considered significantly different from 0.0 (statistically significant at $p < .05$) if their 95% confidence interval does not include 0.0. The best-case estimate reflects expected benefits of reading treatments with 30+ intervention hours targeting decoding skills for children with rigorously diagnosed ADHD placed in regular education classrooms. k = number of included studies.
**$p < .01$. ***$p < .001$.

The overall test of heterogeneity was not significant for studies that used curriculum-based measures ($k = 7$; $n = 30$; $Q[6] = 7.87$, $p = .25$), indicating that $g = 1.91$ is the best estimate for reading improvements as assessed by study-created/curriculum based measures of the specific skills trained during the interventions (i.e., additional moderator analyses for these studies were not warranted). In contrast, significant heterogeneity remained among studies using standardized/norm-referenced outcome tests ($k = 50$; $n = 538$; $Q[49] = 123.04$, $p < .0001$), suggesting that $g = 0.66$ is likely not the best estimate of reading intervention efficacy for these outcomes and supporting the examination of additional moderators to provide a more accurate estimate of the extent to which the skills gained from reading interventions produce 'downstream' improvements on reading achievement.

Moderators of reading intervention efficacy as assessed by standardized/norm-referenced outcome tests. As shown in Table 4, results of the 3-level meta-regression where all moderators were entered into a single regression indicated

that better diagnostic methods (and better study methodology given the multicollinearity described above; $p < .001$), inclusion of students in self-contained classrooms ($p = .006$), and higher intervention dosage (i.e., more minutes; $p = .003$) were each associated with significantly *smaller* estimates of reading intervention efficacy. In contrast, interventions that targeted decoding/phonemic awareness yielded significantly *larger* benefits relative to interventions that targeted reading comprehension (decoding vs. reading comprehension: $p < .01$), whereas training additional skills beyond decoding failed to produce incremental benefits relative to just training decoding skills (decoding vs. multicomponent: $p = .50$, *ns*).

No other significant moderators were found, suggesting that reading intervention benefits for at-risk readers with ADHD do not vary according to child age/grade (range 6–18 years/1st–12th grade; $p = .77$), assessed reading skill outcome ($p = .31$), or whether the skill(s) targeted by the treatment matched the skill(s) assessed by the outcome test ($p = .16$). In addition, adding ADHD treatment failed to produce incremental gains in reading as evidenced by the

nonsignificant effects for both medication ($p = .88$) and behavioral treatment ($p = .50$). After accounting for all tested moderators, no significant variance remained to be explained at the study, sample, and individual level ($p = .07$).

Tier III: Best case estimation. Next, "best case" estimates (Lipsey & Wilson, 2001) were calculated based on the Tier II moderator results. For studies using curriculum-based outcome tests, there was no significant heterogeneity, indicating that the very large effect size of $g = 1.91$ is the best estimate for improvements in the specific reading skills directly trained by these interventions. For studies using standardized/norm-referenced outcome tests, the best case estimate was obtained by solving the Tier II meta-regression equation using values for each significant moderator corresponding to empirically-supported best practice (e.g., Kofler et al., 2008). The best case metric is value neutral—it does not imply whether lower or higher values are desirable—and facilitates conclusions concerning overall improvements on standardized tests of reading skills for children with ADHD (Lipsey & Wilson, 2001).

As described above, the current findings indicate that reading intervention efficacy varies according to diagnostic rigor/study quality, intervention target, minutes of intervention, and whether participants were placed in a self-contained classroom. For diagnostic rigor, we selected the value corresponding to gold-standard/multi-informant methods given that this reflects higher study quality and confidence that children in the ADHD group in fact have ADHD (e.g., Sciutto & Eisenberg, 2007). For intervention target, we selected studies targeting decoding/phonemic awareness rather than reading comprehension given that the former is associated with better outcomes and reflects a recommended treatment target based on the influential "simple view of reading" model (e.g., Lonigan et al., 2018). For intervention duration, we selected the mean for this variable across included studies (1,800 minutes/30 hours). For classroom type, we selected the value corresponding to general education classroom placement given that most children with ADHD receive services in general education classrooms (Fuchs & Fuchs, 2006).

Solving the regression equation using these values and their corresponding *B*-weights (Table 4) indicates that, on average, reading interventions are likely to produce large magnitude improvements on standardized/norm-referenced tests of reading achievement for groups of children with rigorously diagnosed ADHD in regular education classrooms who receive at least 30 hours of reading intervention that targets decoding/phonemic awareness skills ($g = 1.11$).

Tier V: Critical evaluation of the extent to which reading interventions meet established criteria for evidence-based practice for children with ADHD. Finally, we evaluated each study based on its intervention targets and the established criteria for evidence-based practice in (a) Clinical Psychology and (b) Special Education. In the clinical psychology literature, the Chambless and Hollon (1998) criteria are used to define evidence-based treatments (Evans et al., 2018). Based on these criteria, of the 18 studies examined in this review only three reading interventions (Denton et al., 2020; Tamm et al., 2017; Tannock et al., 2018) met all five of the Methods criteria, were randomized controlled trials, were collectively conducted by at least two independent investigatory teams in independent settings (Denton/Tamm, Tannnock), and all demonstrated greater effects as compared to already well-established treatments for ADHD. Consistent with our best case findings above, all three trials targeted decoding skills and included at least 30 hours of intervention. Thus, based on the Chambless criteria, decoding interventions to improve reading outcomes meet all criteria to be considered a Level 1: Well-Established Treatment for school-aged, at-risk readers with ADHD.

In the special education literature, the well-established Council for Exceptional Children Evidence-based Practices in Special Education two-step criteria for evidence-based practice is used (CEC EBP; Cook et al., 2015). Based on these criteria, the same three studies (Denton et al., 2020; Tamm et al., 2017; Tannock et al., 2018) were the only interventions that met the necessary Step 1 quality indicators (QIs) for all assessed areas including Context and Setting, Participants, Intervention Agents, Description of Practice, Implementation Fidelity, Internal Validity, Outcome Measures/Dependent Variables, and Data Analysis. In Step 2, these studies also collectively satisfied all three evidence-based practice criteria, including (a) at least two methodologically sound randomized controlled trials with positive effects, with (b) minimum 60 total participants across studies, and (c) no negative effects. As noted above, all three trials (Denton et al., 2020; Tamm et al., 2017; Tannock et al., 2018) targeted decoding and consisted of at least 30 hours of intervention. As such, based on CEC EBP criteria, decoding interventions to improve reading outcomes meet all criteria to be considered an Evidence-Based Practice with Strong Research Support for school-aged, at-risk readers with ADHD.

Discussion

The present study was the first to systematically quantify the benefits of reading interventions for at-risk readers with ADHD and co-occurring reading difficulties. Utilizing a multi-level meta-analytic approach (Assink & Wibbelink, 2016), we obtained a large overall effect size of $g = 0.96$. However, significant heterogeneity was detected, such that study-developed/curriculum-based measures ($g = 1.91$) yielded larger treatment effects than studies that utilized standardized outcome measures, rigorous ADHD diagnostic standards, and interventions that consisted of at least

30 hours of reading intervention that targets decoding/phonemic awareness ($g=1.11$). In contrast to our hypothesis, the overall effect of reading interventions for children with ADHD was generally *greater* than the overall effect found for reading interventions in studies that did not specifically recruit for children with ADHD (e.g., $g=0.21$–0.95; Gersten et al., 2020; Hall & Burns, 2018; Scammacca et al., 2007, 2015; Swanson, 1999), and instead more comparable to the large magnitude benefits of reading interventions previously reported in meta-analyses of children with behavioral/emotional difficulties more generally ($g=0.90$–1.02; Benner et al., 2010; Roberts et al., 2020). These results excitingly suggest that children with ADHD may benefit as much, or potentially even more, from extant reading interventions as their non-ADHD peers despite the well-known neurocognitive and behavioral risks associated with the disorder. Furthermore, critical evaluation of the included studies based on established criteria from influential professional societies indicates that decoding interventions meet all established criteria to be considered well-established, evidence-based practices based on both clinical and special education criteria (Cook et al., 2015; Evans et al., 2018). Collectively, our findings suggest the provision of at least 30 hours of targeted decoding/phonemic awareness intervention can be highly effective for treating ADHD-related reading difficulties.

In terms of factors that affect the efficacy of reading interventions for children with ADHD, aligned with prior meta-analytic evidence (e.g., Scammacca et al., 2015; Swanson, 1999) our best case estimates indicated that study-developed/curriculum-based measures ($g=1.91$) produced larger estimates of reading intervention efficacy than standardized/norm-referenced tests ($g=1.11$). Our current view is that the true magnitude of reading intervention benefits for children with ADHD likely lies somewhere in between these two estimates. That is, the curriculum-based estimate is likely inflated by the use of study-created (often psychometrically unvalidated) measures that may be too similar to the training content to accurately assess the transfer of training gains to reading skills more generally (e.g., Simons et al., 2016). In turn, the standardized test estimate is likely deflated because norm-referenced test scores are relatively insensitive to change—particularly for children with neurodevelopmental disorders such as ADHD who do not make gains at the same rate as the neurotypical children who comprise the test standardization sample (e.g., Farmer et al., 2020). In contrast, person-ability scores (e.g., growth scale values; Kaufman & Kaufman, 2014) are based on the same normative sample used to construct standard scores, but are developed using item response theory specifically to assess response to intervention. Unfortunately, none of the included studies used this preferred metric, despite frequent calls for increased scrutiny regarding the reliability, validity, and sensitivity of standardized/norm-referenced measures for assessing reading intervention gains (National Reading Panel (US), National Institute of Child Health, Human Development (US), National Reading Excellence Initiative, National Institute for Literacy (US), & United States Department of Health, 2000; Paris & Stahl, 2005). We strongly recommend that future studies report and analyze the preferred growth scale/person-ability scores when assessing change over time/response to intervention.

Whereas studies using curriculum-based measures produced highly consistent results (six of seven effect sizes fell between $g=1.5$ and 2.7), studies using standardized tests differed widely in terms of the reading intervention efficacy (Table 3). The current meta-analysis identified several participant, treatment, and study characteristics that accounted for this between-study variability. First, higher quality studies that used gold-standard, multi-informant assessment procedures and randomization/active control conditions produced significantly smaller effect sizes than studies using less rigorous diagnostic/control methods. These results are consistent with prior ADHD meta-analyses showing that higher study quality and diagnostic rigor are typically associated with smaller effects (Alderson et al., 2007; Kofler et al., 2008), and also consistent with the intervention literature more broadly (e.g., Simons et al., 2016). Previous meta-analyses have hypothesized that the larger effect sizes associated with less rigorous ADHD diagnostic procedures likely reflect the inadvertent inclusion of children without ADHD in the "ADHD" groups in these studies (Kofler et al., 2008). However, this explanation seems unlikely in the current study given that our overall effect suggests reading interventions tend to be *more* effective for children with ADHD than children without ADHD. A more likely explanation reflects the multicollinearity between diagnostic rigor and study quality identified in our preliminary analyses. That is, it is well known that effect sizes are inflated in studies with designs that are unable to control for expectancies, maturation, and other threats to validity, and as such it is unsurprising that smaller effects are obtained when these confounds are removed.

Next, we found that interventions targeting decoding/phonemic awareness yielded significantly *larger* benefits than interventions directly targeting reading comprehension, whereas training additional skills beyond decoding failed to produce incremental benefits relative to training decoding skills only. This pattern of results was consistent regardless of whether the skill targeted in treatment matched the skill assessed by the outcome test, and was consistent with meta-analytic evidence for school-aged children broadly indicating that adding reading comprehension instruction does not produce incremental benefits relative to interventions that specifically target foundational reading skills (e.g., decoding/phonological awareness; Denton et al., 2022). Collectively, our results suggest that interventions targeting phonemic decoding are likely to produce

larger reading gains for at-risk readers with ADHD than interventions targeting reading comprehension, irrespective of whether the treatment goal is to improve decoding or reading comprehension skills. These findings are consistent with predictions from the influential "simple view of reading" model (e.g., Lonigan et al., 2018), which posits that children's reading comprehension skills can be entirely explained by their decoding and language comprehension skills. Notably, however, no intervention in the current study directly targeted language comprehension (i.e., non-print-based understanding of language). Future study on reading interventions that target *both* components of the simple view of reading may lead to treatments that provide optimal reading outcomes for children with ADHD.

Unexpectedly, higher treatment dosages were associated with significantly smaller treatment effects. Interestingly, this counterintuitive association has also been found in prior studies, and has been hypothesized to be a product of higher quality studies delivering larger dosages of instruction (e.g., Hall & Burns, 2018; Scammacca et al., 2015; Tran et al., 2011; Wanzek et al., 2010). However, this hypothesis was not supported in the current meta-analysis because the negative effect of higher treatment dosage was significant over and above the effect of diagnostic rigor/study quality. Instead, we hypothesize that higher intervention dosage may be a marker for the inclusion of more severely reading disabled children (Daniel et al., 2022; Vaughn et al., 2003). Unfortunately, however, we were unable to analyze reading disability status or baseline reading level as moderators due to insufficient reporting across included studies, as noted above. With that said, it is important to note that the reading interventions included in the present review can be considered highly intensive, with treatments including on average 30 hours of intervention (range=3.5–100 hours) within the average span of 10 to 11 weeks (range=1–20 weeks). It will be important for future work to specifically assess optimal treatment duration (which may be dependent on comorbidities or other prognostic indicators) for at-risk readers with ADHD and whether more vs. less intensive treatments are needed.

Finally, adding ADHD-specific behavioral treatment or medication to reading interventions failed to provide incremental reading benefits for children with ADHD. These results differed from Roberts et al. (2020), who found that adding behavioral supports to reading interventions produced significantly *smaller* improvements in reading for children with emotional/behavioral disorders broadly. In contrast, our findings are firmly aligned with the breadth of research demonstrating that reductions in the hypothesized core domains of ADHD (i.e., inattention, hyperactivity-impulsivity) via behavioral management or psychostimulant medication do not lead to, or at best provide minimal, downstream effects on academic achievement (Rapport et al., 2000; Van der Oord et al., 2008). Of course, most if not all reading interventions likely include some behavioral components (e.g., redirecting attention back to the training materials, reinforcing reading skill practice). In that context, these findings suggest that adding ADHD-specific treatments to target interfering behaviors does not provide adjunctive benefits on reading outcomes for children with ADHD (Froehlich et al., 2018) over and above the typical behavioral management strategies used when working with children. Importantly, however, none of the included studies directly targeted the academic enablers or skills (e.g., organization, task engagement, and study skills) that support academic functioning for children with ADHD (DuPaul et al., 2004; Evans et al., 2016; Gallagher et al., 2014). Future reading intervention research may benefit from evaluating the potential additive effects of contingency/behavior modification plans that provide immediate corrective feedback, individualized attention, and frequent reinforcement of these academic enablers during reading intervention given their association with better academic achievement broadly (DuPaul et al., 2004; Evans et al., 2016; Gallagher et al., 2014).

Limitations

The results of this meta-analysis demonstrate that reading interventions produce robust improvements in reading skills for at-risk readers with ADHD. Strengths of our study include the use of three-level meta-analytic modeling to deal with dependencies across effect sizes (Assink & Wibbelink, 2016), use of best case estimation, as well as critical evaluation of included studies relative to established clinical and special education evidence-based practice criteria. The present study was also the first to examine participant, study, and treatment moderators of reading intervention effects for children with ADHD. Despite these methodological refinements, the following limitations must be considered when interpreting results. First, due to inconsistent reporting across included studies, we were unable to examine several potentially relevant moderators, including SES, race/ethnicity, IQ, and the extent to which reading interventions produce similar gains for children with ADHD with and without a diagnosed reading disability. Children with comorbid ADHD and reading disability may present with a unique set of neurocognitive, affective, and behavioral profiles compared to children with either diagnosis alone (DuPaul et al., 2013; Ek et al., 2011; Miranda et al., 2014). As such, it will be important to examine whether these children experience significantly different responses to reading intervention than children with ADHD who have less severe reading difficulties. With that said, meta-analyses of reading interventions for struggling readers more broadly indicate that demographic factors including SES and IQ are not significant predictors of reading intervention outcomes (Bus et al., 1995; Stuebing et al., 2009; Swanson, 1999;

Wanzek et al., 2016). Nevertheless, continued investigation of child, family, and social-community factors (Chan et al., 2022) that promote optimal reading outcomes will be important for determining "what works for whom in what context."

Finally, several planned moderators were excluded due to multicollinearity that confounded/qualified our interpretation of other moderators (e.g., diagnostic method/study quality; ESE status/individual vs. group treatment format). Studies examining each of these factors alone and in combination are needed to disentangle the effects of these factors on reading outcomes for children with ADHD. Moreover, standardized/norm-referenced tests are important for documenting disability but also possess several undesirable characteristics that limit their ability to detect change—particularly for children with neurodevelopmental populations such as ADHD as discussed above (for review, please see Farmer et al., 2020). In contrast, the content of study-created/ curriculum-based measures is often psychometrically unvalidated and potentially "too close" in content to the training curriculum, limiting conclusions that can be drawn regarding transfer of the trained skills to children's reading skills in general. Therefore, we encourage researchers to analyze and report person-ability scores/growth scale values that are developed based on item response theory specifically for assessing change and increasingly provided by standardized academic test publishers (e.g., Kaufman & Kaufman, 2014).

Clinical Implications

Based on the results of the present study, decoding interventions meet the most rigorous criteria to be considered well-established, evidence-based treatments with strong research support for reading difficulties in children with ADHD based on established benchmarks in both clinical psychology and special education (Cook et al., 2015; Evans et al., 2018). Decoding interventions should therefore be the first-line treatment for reading difficulties among at-risk readers with ADHD. In particular, our results revealed that reading interventions that provide at least 30 hours of intensive instruction in phonemic decoding are likely to produce large magnitude improvements in reading for children with ADHD despite the affective, behavioral, and neurocognitive risks associated with the disorder. In contrast, prior meta-analytic evidence indicates clearly that extant evidence-based ADHD treatments produce minimal to no improvements in reading or other academic domains (Boland et al., 2020; Van der Oord et al., 2008), and the current findings suggest that adding ADHD treatment to reading intervention is not likely to produce incremental benefits on reading outcomes. Together, the current review presents systematic and robust evidence for decoding interventions as a highly effective, evidence-based intervention for the pervasive reading difficulties associated with ADHD.

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Note

1. Seven of the 16 studies included in Stewart and Austin (2020) were included in the present meta-analysis, whereas the other nine studies included in Stewart and Austin (2020) were excluded from the current review due to focusing on second-language English learners (1), single-case design studies (6), or not directly targeting reading skills (i.e., spelling, effects of color on reading; 2).

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