Tier 2 Interventions Within the RtI-model for Developing Students' Word decoding – a Systematic Review and Meta-analysis

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Abstract

Background

Objectives: This pre-registered systematic review and meta-analysis aimed to answer if K-2 students at risk (**P**opulation) for reading impairment benefited from a response to tier 2 reading intervention (**I**ntervention) compared to teaching as usual, (**C**omparator) on word decoding outcomes (**O**utcome), based on randomized controlled trials (**S**tudy type).

Methods

Eligibility criteria were adequately sized ($n > 30$ per group) randomized controlled trials of tier 2 reading interventions within response to intervention targeting K-2 at risk students (percentile 40) compared with teaching as usual (TAU). Reading interventions had to be at least 20 sessions and conducted in a school setting with at least 30 students in each group and containing reading activities. TAU could not be another intervention. Only decoding tests from WRMT and TOWRE were included.

Information sources: Database search was conducted 190520 in ERIC, PsycINFO, LLBA, WOS and additionally in Google Scholar as well as a hand search in previous reviews and meta-analyses. The searches were updated in 2021-03-21.

Risk of bias: Studies were assessed with Cochrane's Risk of Bias 2, R-index and funnel plots.

Synthesis of results: A random-effects model was used to analyze the effect sizes (Hedges' g)

Results

Included studies: Seven studies met the eligibility criteria but only four had sufficient data to extract for the meta-analysis.

Synthesis of results: The weighted mean effect size across the four included studies was Hedges' $g = 0.31$ 95% CI [0.12, 0.50] which means that the intervention group improved their decoding ability more than students receiving TAU. A Leave-one-out analysis showed that the weighted effect did not depend on a single study. Students at risk of reading difficulties benefit from tier 2 reading intervention conducted within response to intervention regarding a small effect on the students decoding ability.

Discussion

Limitations of evidence: Only four studies met inclusion criteria and all studies had at least some risk of bias.

Interpretation: Tier 2 reading interventions, conducted in small groups within RtI, can to some extent support decoding development as a part of reading factors.

Other:

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Introduction

One of the most important responsibilities of educators in primary schools is to ensure that all students become competent readers. High-quality reading instruction in primary grades is essential (National Reading Panel, 2000). Ehri (2005) suggests that early reading instruction should focus on phonic decoding. Such a synthetic phonics approach builds upon the theory of ‘simple view of reading’ where reading comprehension is dependent on a sufficient word decoding ability (Gough & Tunmer, 1986; Hoover & Gough, 1990). Students who do not develop robust reading skills in the primary grades will most likely continue to struggle with their reading throughout school years (Francis & et al., 1996; Stanovich, 1986). However, it has been stated that various reading difficulties can be prevented if students are offered early reading interventions (Fuchs et al., 2008; Partanen & Siegel, 2014). Numerous efficient reading intervention programs have been examined in the last decades (Catts et al., 2015; Lovett et al., 2017; Torgesen et al., 1999). In the United States, reading interventions are often conducted as part of a multi-tiered systems of support (MTSS). Indeed, MTSS have become routine in many elementary schools (Gersten et al., 2020). One common approach is the Response to Intervention (RTI) framework, which is the focus of the present review.

Response to Intervention (RtI)

Response to intervention (RtI) is an educational approach designed to provide effective interventions for struggling students in reading and mathematics (Fuchs & Fuchs, 2006). It originated in the US through the ‘No Child Left Behind’ Act, which was introduced in the early 2000s (No Child Left Behind [NCLB], 2002). Many other countries have followed and implemented models inspired by RtI, for example in the Netherlands (Scheltinga et al., 2010) and the UK. A similar model, ‘Assess, Plan, Do, Review’ (APDR), was introduced in England in 2014 by the ‘Special Educational Needs and Disability Code of Practice’ (Greenwood &

Kelly, 2017). Theoretical comparisons have been made between the structure of special education in Finland since 2010 and RtI (Björn et al., 2018). As RtI or RtI-inspired models are widely implemented across the US and Europe, scientific evaluation of their effectiveness is important.

The basic premise of RTI is prevention mirrored from a medical model in the field of education. The aim is to prevent academic failure and misidentification for special education delivery services as a resource allocation framework (Denton, 2012). Struggling students are identified early and support can be offered before failure can occur, this in contrast with education where students fail before measures and support are put in place – a ‘wait-to-fail’ approach. RtI is often referred to as a three-tiered model of support. It is characterized by systematic recurring assessment and monitoring data that determines students’ response to interventions in tiers (Stecker et al., 2017).

Tier 1 consists of evidence-based teaching for all pupils in classroom-based activities. Students receive the core curriculum and differentiated instruction. Universal screenings are used to identify students at-risk. The students who do not develop adequate skills receive more intensive and individualized support through teaching in smaller groups. This corresponds to tier 2 in the model. Tier 2 entails supplemental support and is often delivered to small groups of students for a limited duration (Denton, 2012; Gilbert et al., 2012). The teacher can be a reading specialist, general educator, or paraprofessional who delivers a small-group lesson within or outside the regular classroom setting (Denton, 2012). The intention of tier 2 is to close the gap between current and age-expected performance (Denton, 2012). The third tier consists of even more individualized and intensive efforts. Intervention is provided in even smaller groups or through one-to-one tutoring, and intervention time is increased (45–60 minutes daily). Tier 3 teaching is provided by even more specialized teachers and progress is monitored weekly or biweekly (Fletcher & Vaughn, 2009). Throughout the model, instruction is intensified by

manipulating certain variables (i.e., group size, dosage, content, training of interventionist, and use of data) (Vaughn, Wanzek, Murray, & Roberts, 2012).

There are areas within RtI that are criticized such as the lack of specificity in assessment, the quality and implementation of interventions, selection of research-based practices and fidelity (Berkeley et al., 2009). In addition, RtI interventions have been criticized for lack of validity and comprising students who do not respond to interventions (Kavale, 2005). The many challenges of implementing the RtI-model have been discussed by Reynolds and Shaywitz (2009). Although implemented as a preventative model for students ‘at-risk’ as a contrast to ‘wait-to-fail,’ Reynolds and Shaywitz (2009) criticize the RtI-model for being implemented in practice without the support and adequate research. They also discuss the neglect of possible negative long-term impact on students with disabilities. RtI has historically also been used as a diagnostic method for learning disabilities, a highly controversial practice not discussed in this paper (Batsche et al., 2006).

Objectives

The aim of the present systematic review was to investigate the evidence for tier 2 reading interventions, conducted within the RtI-framework, concerning word decoding skills for at-risk students in primary school (Year K–2). Tier 2 interventions deliver supplementary instruction for students who fall behind their peers in tier 1 core instruction. In primary years these less intensive interventions might be preventative, with early identification and intervention of students at-risk for reading failure. Implementation of efficient tier 2 interventions could allow students to get back on track with their reading improvement. This is an argument for continuous evaluation of the efficiency of reading interventions within tier 2. Specifically, we asked:

- What are the effects of tier 2 interventions on at-risk students' word decoding skills compared to teaching as usual (TaU)?

At-risk refers to risk for developing reading impairments and was defined as word decoding skills on or below the 40th percentile. We focused only on studies with a randomized control trial design (RCT) with children in kindergarten to Grade 2 (K–2) using tier 2 reading interventions for struggling readers below the 40th percentile on decoding tests.

Previous reviews of the efficacy of RtI

Synthesis, reviews, and meta-analyses have examined the efficiency of interventions within the RtI-model since the beginning of the twenty-first century. Burns et al. (2005) conducted a simplified form of quantitative synthesis where they averaged all the effects available (i.e., there was no meta-analytical weighting of sample size), without taking the quality of the studies or the type of designs into account. They concluded that interventions within the RtI-model improved student outcomes regarding reading skills as well as demonstrating systemic improvement.

Wanzek and Vaughn (2007) conducted a synthesis of research on extensive early reading interventions for students with reading difficulties and disabilities. Their synthesis, comprising 18 studies published between 1995 and 2005, revealed positive outcomes for students participating in extensive interventions – in other words, interventions equivalent to tier 3. Their results indicated greater effects for studies providing intervention early (in Grades K–1) and in small groups. Wanzek et al. (2016) investigated tier 2 interventions by examining the effects of less extensive reading interventions (less than 100 sessions) from 72 studies for students with, or at-risk for, reading difficulties in Grades K–3. They examined the overall effects of the interventions on students' foundational skills, language, and comprehension. Effects were divided into (1) standardized foundational skill measures, (2) non-standardized

foundational skill measures, (3) standardized language/comprehension measures, and (4) non-standardized language/comprehension measures. Wanzek et al. (2016) also examined whether the overall effects were moderated by intervention type, instructional group size, grade level, intervention implementer, or the number of intervention hours. Adding further to these new findings, Wanzek et al. (2018) provided an updated review of this literature.

Taken together, these three exploratory meta-analyses provide an overview of the effect of RtI interventions. However, as they did not distinguish between different designs or assess the quality or risk of bias for the included studies, their effects cannot directly be used to answer our research question.

Although not technically a review, a large-scale US national report by Balu et al. (2015) also deserves consideration. The report included 146 elementary schools in 13 states that had implemented RtI processes for the previous three years. The researchers concluded that children in Grade 1, assigned to tier 2 or 3, performed significantly worse on reading comprehension measures than children not designated to interventions. This finding caused much discussion among reading researchers; however, the design of the study has been criticized elsewhere (see Fuchs & Fuchs, 2017).

Of highest relevance for the present research is a recent meta-analysis by Gersten et al. (2020) who reviewed the effectiveness of reading interventions on measures of word and pseudoword reading, reading comprehension, and passage fluency. Results from a total of 33 experimental and quasi-experimental studies conducted between 2002 and 2017 revealed a significant positive effect for reading interventions on reading outcomes. They assessed the quality of the included studies through the '*What Works Clearinghouse*' standard. Moderator analyses demonstrated that mean effects varied across outcome domains and areas of instruction. Gersten et al. found an effect size of 0.41 (Hedges' *g*) for word decoding (word and pseudoword reading).

The present study: A gold standard systematic review

It may seem that our research question could have already been answered in the previous reviews and meta-analyses outlined. Indeed, they had quite broad scopes. However, although these reviews are useful to explore a field, the gold standard to obtain specific answers from the literature is a systematic review. According to the '*Cochrane handbook for systematic reviews of interventions*' (Higgins et al., 2020), a systematic review is characterized by clearly pre-defined objectives and eligibility criteria for including studies that are pre-registered in a review protocol. The search for studies should also be systematic to identify all studies that meet the eligibility criteria. Further, the eligibility criteria should be set to the appropriate quality, or quality should be assessed among the included studies. Studies should be checked for risk of bias both individually (e.g., limitations in randomization) and across studies (e.g., publication bias). The entire workflow from searches to synthesis should be reproducible. Reporting of systematic reviews should be conducted in a standardized way, with the current gold standard being PRISMA (Page et al., 2021).

Systematic reviews have gained prominence in medicine (e.g., Cochrane) and health (e.g., PROSPERO), and are cornerstones of evidence-based medicine and health interventions. In educational research, systematic reviews remain more novel. We believe that the approach is an important development for the field. Indeed, the traditional meta-analysis approach has been criticized for combining low-quality with high-quality studies that yield effect sizes that lack inherent meaning and have little connection to real-life when results are averaged (Snook et al., 2009). In contrast, systematic reviews can directly inform practical work.

The only previous review that can be considered a systematic review is the recent one by Gersten et al. (2020). We applaud their novel and rigorous effort. Still, we are convinced that there are some limitations in Gersten et al. (2020) that the present study will address or complement, and thus further advance the field.

First, unlike the present research, the focus of Gersten et al. (2020) is not specifically on evaluating RtI tier 2 interventions, but on interventions that are, or could have, been used in a RtI framework. From our perspective of examining RtI, this distinction is important.

Second, we rely on another type of quality assessment that is more rigorous and restrictive. Although it could be argued that our criteria were *too* strict, they certainly serve as a nice complement to Gersten et al.'s (2020) and interested readers and policymakers can decide for themselves what level of evidence standard they prefer. For example, whereas Gersten et al. (2020) inclusion criteria allowed for varying levels of quality and evidence (e.g., quasi-experimental studies), we will only consider full-scale (i.e., not pilots) randomized control trials with outcomes that have previously established validity and reliability.

Third, Gersten et al. (2020) provided a limited examination of risk of bias across studies. We undertook more extensive investigation not only across studies but also for individual studies (Cochrane's Risk of Bias 2; Sterne et al., 2019). Importantly, we took advantage of new tools developed in the wake of the replication crisis (see Renkewitz & Keiner, 2019 for an overview), ultimately aiming to ascertain if the effects reported by Gersten et al. (2020) are robust.

A fourth benefit of the present research is that we pre-registered our protocol, which Gersten et al. (2020) did not. This ensures transparency throughout the process and reduces the risk of researcher bias when estimating the size of effects.

The fifth benefit of the present research is that our systematic review can be considered a replication of Gersten et al. (2020). It's unfortunately not uncommon that meta-analyses and systematic reviews cannot be reproduced by a third party (Gøtzsche et al., 2007; Lakens et al., 2019). This is also true for Gersten et al. (2020) who do not share their data with enough detail that we could reproduce it. This does, of course, not suggest any errors in their work, but it highlights the importance of our research as independent replication of their

findings. This is especially important given Balu et al.'s (2015) suggestion that RtI practices is not effective. We have ensured that our systematic review can be reproduced by a third party through sharing the pre-registration, the coding of the articles (i.e., our data) and the R code for all analysis. This means that anyone can build on and update our work.

A final benefit of the present review is that the literature search is updated. Gersten et al. (2020) included studies from 2002 - 2017, whereas we included studies published from 2001 to 2021-03-21.

Method

Protocol and registration

Pre-registration of the study and its protocol was published in March 2019 and can be found at the Open Science Framework: <https://osf.io/6y4wr>. All the materials, including search strings, screened items, coding of articles, extracted data, risk of bias assessments, and the data analysis can be accessed at <https://osf.io/dpgu4/>.

This method section presents our protocol (copied verbatim as far as possible), followed by deviations from the protocol that were made during the process.

Eligibility criteria

To examine the effectiveness of tier 2 reading interventions within the RtI framework regarding students at-risk in K-2, the following eligibility criteria were set.

Participants: Year K-2 students in regular school settings with assessed word decoding skills on or below the 40th percentile. Year K-2 were chosen because we wanted to focus on early intervention, rather than to focus on RtI being evaluated relatively late in the learning-to-read process. We think year two is a reasonably upper limit for that goal. The 40th percentile can be

perceived as high but was set in order to find all studies examining students at-risk”, which possibly could have a rank set on or just above the 30th percentile.

Interventions: The intervention within the RtI framework had to be a reading intervention, as defined by the authors. It needed to consist of at least 20 sessions during a limited period with duration and frequency reported. It has been stated in previous research that more longer-term and intensive literacy interventions are preferable in order to achieve more substantial differences among young students (Malmgren & Leone, 2000). The intervention needed to be conducted in a regular school setting. Other than that, there were no limitations on the type of interventions. For example, the reading interventions could explicitly teach phonological awareness and letter-sound skills along with decoding and sight word instruction. The interventions could also include fluency training, meaning-focused instruction, dialogic reading techniques, and reading comprehension activities, however, instructional activities needed to be reported. Tier 2 interventions in other similar frameworks that are not RtI, such as Multi-Tiered System of Support (MTSS) or other forms of tier/data-driven teaching, are not included.

Comparator: The control groups had to contain students of the same population (at-risk), allocated randomly to the schools’ regular remedial procedures, which is denoted as TaU (i.e., teaching as usual) or denoted as an active control group. The control group was, however, not referred to another research intervention.

Outcome: Descriptive statistics as M, SD and/or reported effect sizes at pre-and post-test of decoding with the tests TOWRE and/or WRMT had to be reported as the key outcome in the studies. We argue data obtained from these instruments has evidence of reliability and validity of the construct decoding. (i.e., studies must have included WRMT and TOWRE). Besides the eligibility criterias above the study had to be published in a peer-reviewed journal and written in English.

Study type: Only RCT:s were included. The sample size had to be at least 30 participants in each group. We choose this cut-off to avoid including pilots and severely underpowered trials, as $n < 30$ per group means less than 50 % power for a realistic moderate effect size ($d = 0.5$). Such small studies have a higher risk of publication bias and will mainly add heterogeneity to the overall estimate.

Information sources

A systematic search was conducted using the online databases of ERIC, PsycINFO, LLBA, Web of Science, and Google Scholar. We complemented the main search process with a hand search of references in previously conducted reviews, meta-analyses and syntheses regarding reading intervention within the RtI-model.

Search

Word decoding skills are one of several potential outcomes in typical RtI research. We aimed to include all possible search terms to identify relevant articles that include decoding skills, including the population of interest and type of study (i.e., at-risk students and RCT). Combinations of keywords, such as ‘reading,’ ‘decoding’, ‘K–2’ OR ‘Grade K’ OR ‘Grade 1’ OR ‘Grade 2’, ‘RtI OR Response to intervention,’ and ‘randomized OR RCT’ were used to identify studies (key phrases for the different searches are available at OSF). The search was restricted to a specified date range from 2001–2019, up to the search day 20th of May 2019. In 2001 RtI was recognized in legislation and then rolled out in school settings. Before 2001, intervention studies were conducted but not integrated into schools’ prevention models.

The searches were conducted as planned, with the one exception that parenthesis in a search string in the protocol had to be corrected because of a typo. On completion of our systematic review, we repeated the searches with the same criteria on the 9th of September 2020

to control the search strings. On 2021-03-21 we updated the searches once more, and this time we also added the search term “Multi-tiered system of support and MTSS”, because it was possible that some RtI studies were framed as such instead. New date range was thus 2001-01-01 to 2021-03-21. There were only minor differences compared to the previous search and no additional studies met the inclusion criteria. The final search strings can be found at OSF and should be used rather than the ones given in the protocol.

Study selection

Articles were downloaded from the databases, combined with those identified by the hand searches, and checked for duplicates. Next, two reviewers independently screened the abstracts of all these articles against the eligibility criteria. Based on the review of abstracts, we excluded dissertations, book chapters, and documents that did not involve children in K–2 or that were not relevant to our topic. Case studies or microanalyses, multiple studies as reviewees or policy reports were also excluded. The remaining articles moved forward into the full-text reading phase. In this phase, we also extracted information about the studies that related to our eligibility criteria. Only articles that met all these criteria were then analyzed further for extraction of statistical data.

Data collection process and data items

In the full-text screening phase, the following categories were coded: (A) author, (B) design, (C) participants, (D) grade, (E) inclusion criteria: at-risk / on or below 40th percentile, (F) intervention, (G) time in intervention and (H) outcome. Any occurring discrepancies or difficulties were solved by discussions with the fourth reviewer (RC) who was not part of the reading process at that point. We deviated from protocol in that the coders stopped their coding when it was apparent that a study would not meet eligibility criteria. When studies did not meet

a code's criteria no further coding was conducted, e.g., when the design (code B) was other than RCT, categories C–H were not coded as the studies were excluded. In other words, this full-text coding phase also involved some full-text screening.

Statistical data for the analysis were extracted independently by TN and RC, and disagreements were resolved by discussion. Data extracted were Mean and *SD* of pre- and post-intervention for each group. For each study where Mean and *SD* have been extracted, we also extracted the main inferential test used by the authors to draw a conclusion (i.e. a focal test). For example, the t-test for the difference between control and intervention group, or the *F*-test for the interaction effect in an ANOVA. The logic behind extracting the focal test is that these are the ones that authors may have tried to get significance for. The focal test was extracted by RC and TN, and disagreement was resolved by discussion.

The only deviation regarding the statistical extraction was that we could not extract pre-test information for one of the articles. We had also not specified that we should contact authors in case of missing data for the outcomes, but we decided to do so.

Risk of bias in individual studies

We decided to deviate from the protocol and use the new Cochrane risk of bias resource, RoB 2 (Sterne et al., 2019) instead of the stated SBU protocol because RoB 2 had updated and SBU was planning to revise their tool. Studies were assessed using the ‘individually randomized parallel-group trial’ template. The assessments followed the RoB 2 manual and were undertaken by two independent reviewers. Assessments were compared using the Excel program provided by Cochrane. Disagreements and consensus were solved through discussions.

We also assessed statistical risk of bias using R-index (Schimmack, 2016). R-index is a method that estimates the statistical replicability of a study by taking the observed power of the

test statistics and penalizing it relative to the observed inflation of statistically significant findings. For example, if 10 studies have 50 % average power, only 50 % of them should be significant (on average). If 100 % of studies are significant, R-index adjusts for this inflation that is plausibly due to publication bias, selective reporting etc. It should be interpreted as a *rough* estimate of the chance of a result replicating if repeated exactly and ranges from 0 to 100 %. Importantly, the adjustment can be used on a study-by-study basis to assess the *risk* of bias. Of course, it does not mean that the specific study is biased, but a study with just significant value (e.g., $p = 0.04$ with $\alpha = .05$) will be flagged as having a high risk of bias, whereas a study that passed the significant threshold with a large margin (e.g., $p = 0.001$) will not be flagged. Studies that present non-significant findings have no risk of bias on this test.

For each study where Mean and *SD* were extracted, we calculated the R-index based on the focal test (i.e., main inference) reported in the article. We used a tool called p-checker (Schönbrodt, 2018) for this. The focal test was extracted by RC and TN and disagreement was resolved by discussion. An R-index $< .50$ for a statistically significant focal test will be considered as high risk for statistical reporting bias. We deviated from our protocol by adding the level ‘some concerns’ for studies that would be over or below the threshold depending on how their focal test was interpreted. This additional level made this statistical bias check more in line with the Rob 2 terminology.

Summary measures

The outcome of interest was the standardized mean difference between the intervention group and the TAU group. We calculated the standardized mean differences in the form of Hedges’ *g*. Hedges’ *g* is the bias-corrected version of the more commonly used Cohen’s *d*. It can be interpreted in the same way but is more suitable for meta-analysis that combines studies of

different sizes. We used the R package Metafor (Viechtbauer, 2010) to calculate effect sizes and their standard errors.

We deviated from the protocol in the following ways: First, although not entirely clear in our protocol, the plan was to calculate the effect size based on the pre-post data for the two groups. Both were initial requirements for inclusion. For this to work, the articles also needed to report the correlation between pre and post. We realized that this would mean exclusion of articles that met all other criteria. Accordingly, we switched to post-comparisons only for the effect size. This does not change the interpretation, since the designs were always pre-post randomized trials, but simply means that when studies did not report the pre-test in sufficient detail for extraction, they could still be included. It has recently been argued that this practice is preferred for pre-post designs (Cuijpers et al., 2017).

We had not specified how to handle nested data in our protocol. Data collected from schools are naturally multi-level: students are nested within classes that are nested within schools. For the designs of the present review, there were two more types of clustering to consider. First, the trial might randomly assign clusters of students (schools or classes) instead of individuals to TAU or RtI (Hedges, 2007). Second, even if students are individually randomized, they may form clusters within the treatment group because tier 2 intervention is done in small groups. Another way to think of this is that there are interventionists (e.g., special teachers) specific effects (Hedges, 2007; Walwyn & Roberts, 2015). These could arise not only because of individual differences among the special teachers but also because the designs do not always call for random assignment to teachers but may depend on various things such as scheduling, preference and needs, etc.

When data are clustered, the effective sample size is reduced, and this will inflate standard errors (Hedges, 2007). The effect of this can range from negligible to substantial, depending on correlation within clusters (Intraclass correlation: ICC) and the size and number

of clusters. Proper modeling of this would require the full datasets from all trials, as well as information that is often not even collected. The difficulty of adjusting for clustering is likely the reason why it is often partially or completely ignored (Hedges, 2007; Walwyn & Roberts, 2015).

We decided to adjust for clustering of random assignment using design effect adjustment for sample size described in the Cochrane manual (Higgins et al., 2020). For studies with an individual assignment, we adjusted for clustering in the treatment group using the approach described in Hedges and Citkowitz (2015) if sufficient details were reported for it to be possible. We did not adjust for the multiple levels of clustering (i.e., everything is nested within schools) but focused on the most substantial clustering.

Synthesis of results

A random-effects model was used to analyze the effect sizes (Hedges' g) and compute estimates of mean effects and standard errors, as well as estimates of heterogeneity (tau, τ). We used the R package Metafor (Viechtbauer, 2010) for this purpose, and the R-script and extracted data are available on OSF.

Risk of bias across studies

Because we expected few studies (< 20), publication bias was mainly assessed through visual inspection using funnel plots. We also undertook sensitivity analysis for the random-effects meta-analysis using leave-one-out analysis and excluding studies that have a high risk of bias. When the tau (τ) estimate and inspection of funnel plots suggested low heterogeneity, we used p-curve to examine the evidential value (Simonsohn et al., 2014) and p-uniform (to provide a biased adjusted estimate (van Assen et al., 2015)). We used z-curve (Brunner & Schimmack,

2020) to estimate overall power, and PET PEESE (Stanley & Doucouliagos, 2014) to examine publication bias, only if we obtained a sufficient number of studies (> 20).

We deviated from our protocol in that we decided to only conduct p-uniform or p-curve if we had a large share of statistically significant studies. These two methods remove the non-significant studies and estimate only based on the significant studies. This is based on the assumption that there are more significant than non-significant studies because of publication bias. This assumption was not met for the current literature.

Additional analysis

As an additional exploratory analysis, we planned to examine effect sizes as a function of time, to investigate if effect sizes have decreased since 2001. However, having extracted the final dataset, we realized that we did not have enough data to examine the effect of time.

Results

Figure 1

Figure 1. Flow chart providing overview of systematic review process

Study selection

The flow chart (Figure 1) details the search and screening process. In the full-text reading phase three articles were assessed differently by the two reviewers; the differences were then resolved by the third reviewer. Seven studies met the qualitative inclusion criteria. However, only four of them were included because the other three did not report necessary statistics such as means and standard deviations for tests conducted after the interventions (Cho et al., 2014; Gilbert et al., 2013; Linan-Thompson et al., 2006). Authors were contacted by two consecutive emails and either they did not respond or could not provide the data. We decided to continue with the review with the four articles that contained the descriptive statistics, which can be found in Table 1. It can be noted that no study was excluded due to our choice of only including WRMT or TOWRE.

Study characteristics

Four studies were included in the review (Case et al., 2014; Denton et al., 2014; Fien et al., 2015; Simmons et al., 2011). All four were conducted in the US where RtI is implemented to a greater extent than in other countries. Table 1 provides an overview of the key features of the four included studies. A total of 339 students were represented in intervention groups and 341 children in control groups. The samples were detected as at-risk children scoring at least $< 40^{\text{th}}$ percentile but most commonly below the 30^{th} percentile. None of the studies included samples of students identified with learning disabilities. The intervention sessions lasted between 11–26 weeks. Intervention content was similar across the interventions in the different studies focusing on phonemic awareness, sound-letter relationships, decoding, fluency training and comprehension. In this section, no findings were reported from the studies because we analyzed the effects in accordance with our analysis plan. The focal tests for each study were analyzed

for the risk of statistical bias and can be found under the heading ‘Risk of bias’ within individual studies.

Case et al. (2014) investigated the immediate and long-term effects of a tier 2 intervention for beginning readers. Students were identified as having a high probability of reading failure. First-grade participants (n = 123) were randomly assigned either to a 25-session intervention targeting key reading components, including decoding, spelling, word recognition, fluency, and comprehension, or were admitted to a no-treatment control condition.

Denton et al. (2014) evaluated the two different approaches in the context of supplemental intervention for at-risk readers at the end of Grade 1. Students (n = 218) were randomly assigned to receive Guided reading intervention, Explicit intervention, or Typical school instruction. The Explicit instruction group received daily sessions of instructional activities on word study, fluency and comprehension.

Fien et al. (2015) examined the effect of a multi-tiered instruction and intervention model on first-grade at-risk students’ reading outcomes. Schools (N = 16) were randomly assigned to control or treatment conditions. Grade 1 students identified as at-risk (10th to 30th percentile) were referred to tier 2 instruction (n = 267). Students performing on the 31st percentile or above had tier 1 instruction. In the treatment condition, teachers were trained to enhance core reading instruction by making instruction more explicit and increasing practice opportunities for students in tier 1.

Simmons et al. (2011) investigated the effects of two supplemental interventions on at-risk students’ reading performance of kindergarteners. Students (n = 206) were randomly assigned to either an explicit and systematic commercial program or to the school’s own designed practice intervention. Interventions took place for 30 min per day in small groups, for approximately 100 sessions. Characteristics of the interventions are summarized in Table 1.

Table 1. *Features of intervention studies included in the systematic review*

Study	Desi gn	N	Grade	Group size	Difficulty	Intervention	Time	RoB 2 Risk of bias	R-index
Case et al. (2014)	RCT	Study N=123 Tier 2 N= E 61+ TAU 62	2	2–4	At-risk	PA, WA, sight word, fluency, spelling, comprehension	12 w. 3x40min	Some concerns	Low risk
Denton et al. (2014)	RCT	Study N=218 tier 2 N=	1–2	2–4	At-risk < 30 th percentile	Explicit reading instruction phonologically based, phonemic decoding and	23–25 w. 4x45 min.	Some concerns	Low risk

		E 70 + TAU 66 + (70 GR not examined)				spelling, text reading for fluency, comprehension instruction			
Fien et. al. (2015)	RCT	Study N=239 E 107+ TAU 132	1	3–5	At-risk < 30 th percentile	Irregular word reading, PA, sound spelling, blending and word reading, accuracy and fluency reading decodable text, encoding practice, reteaching of challenging words	26 w. 30 min.	High risk	Some concerns
Simmons et al. (2011)	RCT	Study N=206 tier 2 N= E 112 + TAU 94	K		At-risk <30 th percentile	Systematic ERI intervention PA, phonological and, alphabetic understanding, integrating writing and	25 w. 5x30 min.	Some concerns	Some concerns

spelling with phonemic and
alphabetic skills

Note. E=Experimental, N=sample size in included studies, NR=Not reported, ES=Effect size, W=weeks, M=Medium, GR= Guided Reading, PA=Phonological Awareness, WA=Word Attack, ERI=Early reading intervention. The full risk of bias assessment can be found on the OSF repository.

Risk of bias within individual studies

Using RoB 2, three studies were assessed as having some concerns of risk of bias (Case et al., 2014; Denton et al., 2014; Simmons et al., 2011) because no pre-registered trial protocol or statistical analysis plan existed (although Denton et al., 2014 mentioned one we could not find it) and assessors were not blinded to which group participants belonged. Fien et al. (2015) were assessed to be of high risk of bias because of a combination of no pre-registered trial protocol and with a composite score of the outcome which was marginally significant. However, this analysis is not the focus here as we calculated the effect in accordance with our research plan. The R-index risk of bias check resulted in two studies with no risk of bias and two studies about which there was some concern (see Table 1). Overall, there were no alarming levels of risk to be found.

Results of individual studies

Figure 2 shows the forest plots for the studies based on a random-effects meta-analysis. It shows the effect sizes and 95% CI of the individual studies and the random-effects weighted total. Because the forest plot is based on data adjusted for clustering, the width of the 95% CI does not directly match the sample sizes. Table 2 shows the Mean and *SD* extracted for the WRMT test. Note that one of the studies used a composite score and thus these scores are not based on exactly the same metric.

Figure 2

Figure 2. Forest plot of the studies in the meta-analysis

Table 2. *Descriptive data of the included studies*

Study	TAU	TAU	TAU	Tier 2	Tier 2	Tier 2
	M	SD	N	M	SD	N
Case et al. (2014)	104.72	11.05	62	107.07	9.57	61
Denton et al. (2014)	90.37	11.93	66	94.71	9.95	70
Fien et al. (2015)	106.7	7.7	132	109.2	7.4	107
Simmons et al. (2011)	105.16	10.99	94	108.09	9.20	112

Note. Output data post-test is WRMT=*The Word Identification subtest* (Woodcock, 1998). It should be noted that the N in this table is not consistent with the forest plot because of the cluster-designs used in two of the studies. *M*=mean; *TAU*=teaching as usual.

Synthesis of results – the meta-analytic findings

The random-effects model was estimated with the default restricted maximum-likelihood estimator. Tau (τ) was estimated to be zero (0). Although this is consistent with low heterogeneity, it should not be interpreted as exactly zero since small values of tau are hard to

estimate with a low number of studies. With a τ of zero, the results from a random-effects model are numerically identical to a fixed effects model.

The random-effects model on the four studies found an overall effect (Hedges' g) of .31, 95 % CI [0.12, 0.50], that was significantly different from zero $z = 3.18, p = .0015$. Relying on Cohen's convention for interpreting effect sizes, this interval ranges from trivial to just below moderate. Taken together we reject that the effect is negative, zero, moderate or large, and interpret it as a statistically trivial to small effect.

To summarize, interventions including decoding in tier 2 within RtI give a point estimate of a small positive effect of 0.31 with a 95% CI that ranges from 0.12 (a trivial effect) to 0.50 (just below moderate). Although we can exclude zero, the effect is not much more effective than teaching as usual in the comparison groups. This information is relevant to researchers and practitioners and discussed in the implications below.

Risk of bias across studies

Looking at the funnel plot with enhanced contours (indicated 90 %, 95 % and 99 % CI) we see no signs of publication bias (Figure 3). A conventional funnel plot suggests the same (Figure 4). However, with so few data points it is very hard to tell.

A leave-one-out analysis showed that the effect was robust as it ranged from 0.34 to 0.27, and the lower CI ranged from .04 to 0.17. Because of the little bias found in the individual studies, this leave-one-out analysis is enough to examine that as well. It is worth noting that the results were robust with the exclusion of Fien et al. (2015), the only study with high risk.

It was not possible to use p-curve or p-uniform because only one study was significant for the extracted outcome. It would have been possible to conduct these on the extracted focal tests, but we had already performed R-index on that, based on our analysis plan. Furthermore,

as we had detailed in our protocol, we would not conduct Z-curve or PET PEESE across studies if there were fewer than 20 studies.

Figure 3

Figure 3. Funnel plot with contour enhanced funnel plot centered at 0.

Figure 4

Figure 4. Conventional funnel plot centered at the standardized mean difference

Discussion and summary of evidence

This PRISMA-compliant systematic review examining RtI tier 2 reading interventions shows positive effects on at-risk students' decoding outcomes. The significant weighted mean effect size across the four included studies was Hedges' g 0.31 95% CI [0.12, 0.50]. Students at-risk

for reading difficulties benefit from the tier 2 interventions provided in the included studies, although the CI indicates the effect can be anything from trivial to just below moderate. Gersten et al.'s (2020) high-quality meta-analysis of reading interventions found effects of Hedges' $g=0.41$ in the area of word or pseudoword reading. Our present study, with both stricter criteria and only including studies conducted within the RtI-framework, suggests that the findings from Gersten et al. (2020) are robust. As the US national evaluation of RtI (Balu et al., 2015) found negative effects on reading outcomes for tier 2 reading interventions our result must be regarded as important to the field. Gersten et al. (2020) point out that intervention programs with specific encoding components might reinforce phonics rules and increase decoding ability. This is consistent with research in the field of interventions targeting reading acquisition and reading development that show that systematic and explicit training in phonemic awareness, phonics instruction, and sight word reading implemented in small groups can help students who are likely to fall behind when they receive more traditional teaching. The studies in this review included such intervention programs that also involved explicit instruction. The efficiency of explicit instruction is aligned with earlier extensive reading research highlighting explicit instruction as efficient for students at-risk of reading difficulties (Lovett et al., 2017). These findings are of importance for practitioners teaching children to read. However, the effect found in this review is not large compared to TAU, which was the comparator used. One way to interpret the relatively low effect of tier 2 intervention vs. TAU is that it could not be expected to be large. By definition, RtI is proactive instead of reactive (like 'wait-to-fail'). Indeed, the students who are given treatment are *at-risk*. By definition, with a cut-off at 40 %, some would not need this kind of focused intervention and thus the effect vs. TAU in such cases is expected to be low or close to zero. To use TAU in studies that investigate the relative effect of RtI interventions also points to the need to be able to carefully examine the TAU components and the quality of classroom reading instruction. However, TAU is rarely described in the same

detail as the experimental intervention, which makes conclusions about the effectiveness of RtI a bit difficult to determine. We would expect a larger contrasting effect in schools with lower overall classroom quality, and conversely, a smaller contrasting effect in schools of higher quality. As these details are not available to review, the generalization becomes more restricted. We believe that generalizing beyond the US school system would not be appropriate. More high-quality studies of RtI are needed outside of the American school setting to determine the effectiveness in a more global sense.

Some of the previous reviews and meta-analyses (Burns et al., 2005; Wanzek et al., 2016; Wanzek et al., 2018; Wanzek & Vaughn, 2007) have examined RtI reading interventions with several general questions studying overall effects on reading outcome measures. This approach has benefits and weaknesses. The benefit would be examining overall effects in reading, but at the same time, the results can be too broad and unspecified. This study narrowed both the research question asked, compared to previous studies, as well as the eligibility criteria (i.e, PICOS) for inclusion of studies. This was done to obtain studies with high quality but also studies that are comparable in terms of design and tests used. Previous reviews included not only RCTs but also quasi-experimental studies. Including quasi-experimental studies makes it harder to establish the effect of the intervention because individual differences at baseline can confound intervention effects. It is also common but unfortunate practice in meta-analyses to merge studies together for within-group and between-group designs, with within-group studies artificially inflating the overall effect size.

In this review, we assessed the studies at risk of bias in addition to only including high-quality studies. These approaches did not result in any alarming levels of risk of bias assessment. The interpretation of the reported effect size in the present study can therefore be regarded as a more confident meta-analytic finding compared to earlier findings as it only includes quality-checked RCT-studies that met our rigorous standards.

Limitations

Outcome data were missing in three studies with eligibility for inclusion in the review. This reduces the value of the evidence somewhat compared to if all studies had been included because it is not clear if the weighted effect obtained would be somewhat lower, the same, or higher.

Future research

For future research, we have several recommendations. First, we recommend pre-registered studies with sufficient power (several hundreds of schools) as the hitherto reported effects are small-to-medium and RtI should be studied at the organizational level. A large-scale longitudinal research study of fully implemented RtI-models has not been conducted yet. Such a study would be expensive and difficult to accomplish but it must be weighed against the cost of continued failure to teach a large percentage of school children to read, as has already been argued by Denton (2012). Second, we suggest that RtI-intervention studies use randomization at school or other appropriate cluster levels. This is important as RtI interventions are not pure reading interventions but occur in school contexts. To this end, we recommend using a cluster-randomized approach with proper modeling of nested effects that can occur at the school level as well as in intervention groups. Third, reading interventions published as scientific articles must report and provide necessary data for others to analyze, for example, accessible via a repository such as the OSF. The disadvantage of not doing so became apparent for this review and can seriously harm any field. Three more studies could have been examined in this meta-analysis if authors had reported descriptive data or made it accessible in other ways.

Fourth, we recommend studies included in future systematic reviews be assessed for evidential value (e.g., with GRADE) and risk of bias (e.g., using Rob 2). When applying Rob

2, risk of bias was indicated as few studies had blinded assessors and none had pre-registration protocols. We also found no systematic review that has been pre-registered or had a statistical analysis plan provided or made accessible for others to scrutinize. To increase quality and ensure the highest standards, pre-registrations, trial protocols, and transparency should be the standard, especially as RtI is widespread and impacts academic studies for many young students. Gersten et al. (2020) also ask for high-quality studies in the field of reading interventions that provide a robust basis for further examinations. They suggest it will take some time for the field to reach that goal. We hope for quicker progress.

Conclusions

The findings of this systematic review support previous findings that tier 2 reading interventions, conducted in small groups within RtI, to some extent support decoding as a part of reading development. Thus, teachers may continue to deliver tier 2 reading interventions that target basic reading skills such as decoding.

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Author contributions (CRediT)

Order of the authors is Nilvius, Camilla as lead author. In alphabetical order Carlsson, Rickard, Fälth, Linda and Nordström, Thomas as senior author.

Authors has contributed with following:

Camilla Nilvius: Conceptualization, Investigation, Writing original draft, Writing, reviewing and editing, Visualization, Funding acquisition.

Rickard Carlsson: Conceptualization, Methodology, Validation, Data curation, Resources, Formal Analysis, Writing, reviewing and editing, Funding acquisition.

Linda Fälth: Conceptualization, Investigation, Writing, reviewing and editing.

Thomas Nordström: Conceptualization, Methodology, Validation, Investigation, Data curation, Writing, reviewing and editing, Project administration, Funding acquisition.

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