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What's working in working memory training? An educational perspective

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Abstract

Working memory training programs have generated great interest, with claims that the training interventions can have profound beneficial effects on children's academic and intellectual attainment. We describe the criteria by which to evaluate evidence for or against the benefit of working memory training. Despite the promising results of initial research studies, the current review of all of the available evidence of working memory training efficacy is less optimistic. Our conclusion is that working memory training produces limited benefits in terms of specific gains on short-term and working memory tasks that are very similar to the training programs, but no advantage for academic and achievement-based reading and arithmetic outcomes.

Keywords

working memory; training; academic attainment; intervention

Background

Working memory refers to the system responsible for storage of information over short periods of time, whereby the information is used to fulfill some goal-directed activity (for reviews, see Baddeley, 2012; Conway, Jarrold, Kane, Miyake, & Towse, 2007). Working memory research has become increasingly important in educational and developmental

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[1]Other studies considered but not included, and the reasons why – no academic outcomes (Goldin et al., 2013; Holmes & Gathercole, 2014, Experiment 1; Horvat, 2014; Jaeggi, Buschkuehl, Jonides, & Shah, 2011; Pugin et al., 2014; Roughan & Hadwin, 2011; Shavelson, Yuan, Alonzo, Klingberg, & Andersson, 2008; Thorell, Lindqvist, Bergman-Nutley, Bohlin, & Klingberg, 2009; Wang, Zhou, & Shah, 2014; Zhao, Wang, Liu, & Zhou, 2011); combination of working memory training and another type of training (Aries, Groot, & van der Brink, 2014, Experiment 1; Soderqvist, Nutley, Ottersen, Grill, & Klingberg, 2012); no working memory transfer assessed in same sample as achievement tests assessed (Dahlin, 2011, 2013; Holmes & Gathercole, 2014, Experiment 2; Loosli, Bushkuehl, Perrig, & Jaeggi, 2012; Mansur-Alves, Flores-Mendoza, & Tierra-Criollo, 2013).

contexts. During typical child development, working memory functioning has consistently been shown to be correlated with academic outcomes (Alloway & Alloway, 2010). In addition, research suggests that children with reading disabilities (Swanson, Zheng, & Jerman, 2009) and children with arithmetic disabilities (Swanson & Jerman, 2006) frequently have working memory impairments. Working memory and attention control are strongly related (Redick, Heitz, & Engle, 2007), and children with impairments of attention, such as attention-deficit/hyperactivity disorder (ADHD), have also been observed to have working memory deficits (Alderson, Rapport, Hudec, Sarver, & Kofler, 2010; Martinussen, Hayden, Hogg-Johnson, & Tannock, 2005).

Given the numerous demonstrations that low working memory capacity is associated with poor outcomes in higher-order cognition, interventions have been developed to increase working memory. The idea is that if one could simply increase an individual's working memory capacity, then performance on other cognitive abilities that are strongly related to working memory should also increase (Shipstead, Redick, & Engle, 2010, 2012). Producing improvements on the repeatedly practiced working memory tasks is insufficient for demonstrating that the training "works". The key for working memory training proponents is to show that the improvement on the practiced working memory tasks leads to improved educational and behavioral outcomes. This *transfer* is typically assessed via comparisons of the pretest-to-posttest change for a working memory training group and a control group. A further distinction is also often made between pretest-to-posttest improvements on short-term and working memory tasks that are similar to the training materials (*near transfer*), and improvements on academic and behavioral outcomes that differ from the working memory training programs (*far transfer*).

The idea that working memory training should lead to transfer to academic outcomes depends foremost on an individual's working memory being modifiable by such training. Previous work has shown that manipulations can increase a person's score on a working memory measure (e.g., re-taking a test, motivation, strategy instruction), but this improvement in the individual's working memory score may not reflect a true change in underlying working memory ability. For example, Ericsson et al. (1980) demonstrated a subject who, through mnemonic strategies, was able to increase his serial recall of digits to 79 in a row, though when tested on memory span measures that did not include digits, his scores were in the normal range (7 ± 2). Biological determinants of working memory may also place limitations on the potential malleability of working memory – for example, research suggests that executive functions, including memory updating, are highly heritable traits (Friedman et al., 2008).

It is also critical to acknowledge that many of the demonstrations linking working memory to academic outcomes are correlational in nature, and not necessarily causal (for further discussion, see Jacob & Parkinson, in press). For example, numerous studies have shown that children with language and reading problems have poor working memory (for review, see Hulme & Snowling, 2009). However, these findings can be interpreted in at least two ways: working memory problems can be a *cause* of their language problems, but it may also be true that they exhibit poorer working memory performance as a *consequence* of their language disorder. Thus, causal theories are lacking about *how* working memory training would improve academic outcomes for children with learning disorders.

Many commercially available working memory training programs have been developed to target educational outcomes. One example is the *Cogmed* working memory training program, which is owned and distributed by the Pearson publishing group. Cogmed working memory training includes multiple computerized verbal and visuospatial memory span tasks that trainees practice several times per week for several weeks. These span tasks are variations of memory span tasks used in many neuropsychological and intelligence batteries, where the subject is presented with a sequence of stimuli and must report it back, either in the order it was presented, or after performing some manipulation on the sequence (e.g., recall backwards). The Cogmed program is videogame-like, and uses an adaptive formula to constantly adjust the sequence length to calibrate the difficulty to the performance of the individual. Other similar adaptive working memory training programs present sequences of items for subjects to recall in order, but subjects must selectively recall only the last few items on the list (running span) or remember the sequence of stimuli while also performing interleaved distracting tasks (complex span). Different working memory training programs include computerized variations of the *n*-back task, a working memory paradigm frequently used in the cognitive neuroscience literature (Redick & Lindsey, 2013). In the *n*-back task, individuals are asked to report whether or not the currently presented stimulus matches the stimulus that was presented *n* stimuli back. Again, in working memory training versions of the *n*-back task (*BrainTwister*, *Lumosity*), the task is often adapted to the performance of the trainee by varying the *n*, which is thought to affect the working memory load imposed.

Websites for various commercial working memory training programs tout the success of working memory training programs such as these to improve important academic outcomes including arithmetic, spelling, and reading comprehension. In addition, popular media coverage (e.g., Hurley, 2012) and books (e.g., Alloway & Alloway, 2013) have claimed that working memory training can improve functioning on a wide range of tasks. In particular, working memory training has often targeted various diagnostic groups (children with ADHD, reading disorders, or poor working memory) based on the idea that working memory "training could be used as a remediating intervention for individuals for whom low [working memory] capacity is a limiting factor for academic performance" (Klingberg, 2010, p. 322).

Here, we ask whether the available evidence supports such claims. A couple of early empirical studies with children argued that working memory training had caused improvements in vocabulary and arithmetic (Alloway, 2012) and nonverbal intelligence and ADHD symptoms (Klingberg et al., 2005). Although these studies generated substantial interest, their results are largely atypical compared to the rest of the published working memory training literature with children. A meta-analysis of the working memory training literature (Melby-Lervåg & Hulme, 2013) concluded that published studies involving children indicated that working memory training often produced near transfer to working memory measures, but did not cause far transfer to nonverbal intelligence, verbal intelligence, reading, or arithmetic outcomes. Similarly, a meta-analysis by Rapport, Orban, Kofler, and Freidman (2013) indicated nonsignificant effects of working memory training on academic achievement in children with ADHD.

Criteria for Evaluating Educationally-Relevant Working Memory Training Research

Unfortunately, certain methodological limitations are common in the working memory training literature and severely hamper the ability to discern the ‘true’ efficacy of working memory training, especially in studies using educationally-relevant and academic achievement outcomes. We begin with a consideration of what the criteria should be for evaluating the evidence of a particular study either for or against its efficacy in achievement-related tests and measures. These criteria (Table 1) are a sort of “best practices” guide in terms of study design and data interpretation. Our specific criteria for a review focused on working memory training and transfer to educationally-relevant achievement include: (a) samples comprised of children, not adults or older adults; (b) training on working memory specifically, instead of training on other tasks (e.g., inhibitory-control, task-switching, videogame) or a combination of working memory tasks and other tasks; and (c) pre-training and post-training assessment of both working memory near transfer and educationally-relevant achievement measures and academic tests (the focus here is on math and reading, instead of nonverbal or verbal IQ/intelligence tests, ADHD ratings, etc. that have been the focus of most other reviews). Less obvious criteria that we (and others) consider important for evaluating the strength of the evidence for or against educationally-relevant transfer include: (a) use of an active-control group; (b) use of sufficiently large sample sizes in each training and control group; (c) use of objective tests and measures instead of subjective measures such as rating scales or questionnaires; (d) evidence for positive near transfer to working memory measures; and (e) transfer results that follow a sensible pattern (e.g., significant group x time interactions are not driven by decreases from pretest to posttest by control group).

We consider the rationale for these general criteria below (for further discussion, see Buschkuehl & Jaeggi, 2010; Melby-Lervåg & Hulme, 2013; Redick, 2015; Redick et al., 2013; Shipstead et al., 2012). First, the nature of the control group within training studies affects causal interpretations. A *passive* control group is one in which the subjects are not receiving working memory training and have no contact with the experimenters during the pretest-to-posttest interval. In contrast, an *active* control group is one in which subjects practice an alternative type of task that is unrelated to working memory (examples include visual search, trivia questions, or variations of the working memory training tasks to eliminate/reduce working memory involvement) during the pretest-posttest interval. Although a researcher may want to make a causal claim about working memory training efficacy, if the comparison is with a passive control group, there are many other variables that differ between the training and control groups. These may include: (a) amount of experimenter and computer contact; (b) familiarity and level of comfort with the research team and setting; (c) expectancy effects; and (d) motivation. The use of active control groups helps control for (although may not eliminate; see Boot, Simons, Stohart, & Stutts, 2013) alternative explanations for observed transfer in working memory training studies. As evidence for the importance of the type of control group, Melby-Lervåg and Hulme (2013), in their meta-analysis of all age groups, observed that transfer to nonverbal intelligence was significant when compared to a passive control group; however, the working memory training effect on nonverbal intelligence was zero when compared to active-control groups.

Second, studies using small sample sizes are unfortunately common in the working memory training literature, perhaps because of the financial cost and time necessary to conduct such studies. Using a small sample biases the study to produce an inflated effect size (Button et al., 2013), which misrepresents the strength of the working memory training program. Studies using larger sample sizes provide stronger and more accurate evidence about working memory training's efficacy. Based on recommendations that a minimum of 20 observations per cell should be included in a research design (Simmons, Nelson, & Simonsohn, 2011), we have categorized studies as having a sufficient sample size depending on whether each training and control group in the study contains pretest and posttest data for at least 20 subjects – and note that we consider this an absolute minimum number of subjects to include.

Third, while there may be valuable information in either self-report or informant-report (e.g., teacher, parent) questionnaires about children's behavior before and after working memory training, these data are less compelling than performance on more objective academic measures and achievement tests. In particular, one concern is that in many studies, those doing the rating are aware of whether or not the person being rated is in the training or the control condition, and therefore their responses on questionnaires may reflect expectancy or placebo effects instead of actual cognitive changes (Shipstead, Hicks, & Engle, 2012). As shown in the meta-analysis by Rapport et al. (2013), working memory training studies that used behavioral ratings as outcomes are highly influenced by who is doing the rating: in studies in which the rater was aware of whether or not the pupil received training, there was a large improvement in the behavioral rating of the training-group students; in studies in which the rater was blind to the group assignment, there was no change in behavior ratings. This pattern suggests that raters are influenced by their knowledge of which students received the working memory training intervention, and thus claims about the efficacy of working memory training based on unblinded behavioral ratings should be viewed skeptically.

Next, if one wants to argue that working memory training is the cause of far transfer to academic and achievement outcomes, one must also demonstrate within the same subjects that the working memory training produced near transfer to working memory tasks. Unfortunately, most studies tests this using working memory tasks that are identical or very similar to the training programs (Melby-Lervåg & Hulme, 2013), so it's not clear if the working memory construct is changed, or simply working memory tasks sharing content or methodological features with the training.

Finally, an often overlooked aspect of interpreting the strength of evidence for the efficacy of working memory training is whether or not the pattern of transfer results is sensible (Redick, 2015). That is, there are numerous ways that a significant group x session interaction (or significant effect of group on posttest if pretest is used as covariate) can occur. The sensible or expected pattern is that the training and control groups obtain a similar test score at pretest, and their performance diverges at posttest because the training group has improved significantly more than the control group (see Redick, 2015, Figure 1). Notably, issues typically arise in these studies because (a) the training and control groups do not have similar pretest scores; and/or (b) the control group decreases in performance from pretest to posttest. While the critical statistical test is significant, if the effect is driven primarily by the control group's declining performance instead of the training group's improving performance, the data provide limited evidence for the training's effectiveness.

Before discussing the specifics of the relevant studies, we briefly mention two additional study characteristics that we think are important, but did not require for inclusion in the current review. First, some working memory training studies have included follow-up assessments months after the completion of training to investigate the duration of transfer, if observed. There are reasons to think that transfer will not persist after training has ceased (e.g., Jaeggi, Buschkuehl, Jonides, & Shah, 2014), drawing a comparison to aerobic training, where benefits quickly dissipate when an individual no longer regularly engages in exercise. Based on this logic, working memory training benefits to academic and achievement tests might be observed immediately following training (viz., at posttest), but no longer be present months later (viz., at follow-up). However, others (Holmes, Gathercole, & Dunning, 2009; St Clair-Thompson, Stevens, Hunt, & Bolder, 2010) have suggested that transfer to achievement-based measures would not show up immediately after training, but would take some amount of time after training has finished to manifest in improved reading or math performance. Given these conflicting predictions, we did not require that studies have a long-term follow-up. Indeed, since relatively few of the working memory training studies that met our other criteria also included follow-up assessments.

Finally, many working memory training studies have measured transfer outcomes based on one test only, instead of using multiple measures of the intended construct or ability. Performance on any test reflects variance specific to that test and its administration method, in addition to the theoretical construct it is intended to measure. A more persuasive demonstration that the training leads to improvement in the transfer ability is accomplished by using multiple measures of each intended construct, which can be used to either form a composite or latent variable for analyses (Shipstead et al., 2012).

Empirical Evidence for and against Working Memory Training

The meta-analyses by Melby-Lervåg and Hulme (2013) and Rapport et al. (2013) indicated no evidence for the efficacy of working memory training for academic outcomes, even including studies with the shortcomings outlined above. For the current review, Table 2 provides information about each study, and Table 3 evaluates each study in relation to our criteria, along with a brief description of the main academic transfer results. In each table, the studies are organized alphabetically within active- and passive-control groupings. In general, the bulk of the studies in Table 2 involve children with poor working memory, meaning that the samples being trained provide perhaps the strongest opportunity to observe benefits from working memory interventions, given that they have the most room to improve from training. In addition, Table 2 shows that although Cogmed is the training program that has been used most often, there are a variety of other training programs and control conditions used across studies. Of note, although the majority of training programs were administered individually via computer, two studies conducted face-to-face, interactive training (Henry, Messer, & Nash, 2014; Kroesbergen et al., 2014). Two studies (St Clair-Thompson et al., 2010; Witt, 2011) explicitly trained subjects on mnemonic strategies such as rehearsal and imagery, as compared to the typical unstructured training studies that provide no explicit guidance or strategies for subjects to use during training. There is also a wide variety in the ages of the subjects used across studies. Below, we highlight individual studies, focusing on those using active controls, and make an evidence-based decision about the efficacy of working memory training in educational contexts.

Alloway, Bibile, and Lau (2013)

Alloway and colleagues investigated the efficacy of training with the *Jungle Memory* program for 8 weeks, with the training group completing training sessions 4 times per week compared to an active control group that completed 1 training session per week and a passive control group. Despite the authors' optimistic conclusion that "the present study offers supporting evidence that computerized working memory training can lead to transfer gains in untrained cognitive tests of ability and attainment" (p. 637), the training group did not show larger pretest-posttest improvement compared to either control group on the two administered measures of academic attainment (spelling from the Wechsler Objective Reading Dimensions, *WORD*, and math from the Wechsler Objective Numerical Dimensions, *WOND*). The same nonsignificant results were observed in a subset of subjects that completed the academic tests at an 8-month follow-up. Notably, these nonsignificant results were obtained although both control groups showed a numerical decrease from pretest to posttest, and on the spelling pretest, the training group scored significantly lower ($p < .001$) than both control groups.

Ang, Lee, Cheam, Poon, and Koh (2015)

Ang and colleagues conducted a rigorous study, examining near and far transfer with a number of tests for each outcome in two training groups (Cogmed and updating groups) in comparison to active- and passive-control groups. Each group had a minimum of 25 subjects that completed pretest, posttest, and follow-up sessions 6 months after the end of training. Academic outcomes were assessed via three standardized mathematics tests (Numerical Operations, addition fluency, subtraction fluency from the Wechsler Individual Achievement Test; *WIAT*). Although near transfer to working memory tasks was observed, the authors concluded that "improvement is limited to a task similar to that used in training and did not transfer to better mathematics performance" (p. 7). Of note, inspection of Table 1 in Ang et al. (2015) indicates that the Cogmed training group showed mostly numerically *smaller* changes from pretest to posttest and pretest to follow-up on all three math tests, compared to either the active- or passive-control group.

Chacko et al (2014)

Chacko and colleagues compared an adaptive Cogmed training group and non-adaptive Cogmed control group on multiple outcomes, including four tests from the Wide Range

Achievement Test (*WRAT*; Word Reading, Sentence Comprehension, Spelling, Mathematical Computation). Each group contained over 40 children with ADHD. Again, although near transfer to working memory was observed for the training group, no significant transfer to any of the four academic tests was observed. The authors concluded that the results indicate Cogmed "may not have specific effects on measures of academic achievement, at least in the short term" and "should not be used as a treatment for ADHD in children" (p. 254).

Dunning, Holmes, and Gathercole (2013)

Dunning and colleagues conducted a study using Cogmed training with 94 elementary students who had been previously identified as having working memory impairments. Dunning et al. (2013) randomly assigned students across multiple schools to either training, active-control, or passive-control groups, and assessed academically-relevant far transfer via multiple reading (Basic Reading from the WORD, reading accuracy, rate, and comprehension from the Neale Analysis of Reading Ability, Written Expression from the Kaufman Test of Educational Attainment) and math outcome tests (Number Operations and Mathematical Reasoning from the WOND). Dunning et al. (2013) observed near transfer to short-term and working memory tasks, but no transfer to any achievement outcomes, in contrast to previous work by the same authors (Holmes et al., 2009) that did not randomly assign students or include a control group comparison for specific analyses. Transfer was assessed 12 months after completion of training in a subset of subjects, with no transfer on the five academic tests also administered at follow-up. As noted by Dunning et al. (2013), their previous findings in Holmes et al. (2009) are called into question given the lack of academic transfer using "a more robust methodology" (p. 916).

Gray et al. (2012)

In Gray et al. (2012), children with coexisting learning disabilities and ADHD completed 5 weeks of either Cogmed working memory training or Academy of Math active comparison tasks. Academic achievement was assessed 3 weeks later via the four aspects of the WRAT (Word Reading, Sentence Comprehension, Spelling, Mathematical Computation), as in Chacko et al. (2014). The authors concluded that the "study did not, however, find any improvement in behavior or academic measures" (p. 1283).

Henry et al. (2014)

Henry et al. (2014) randomly assigned 18 children each to adaptive, face-to-face, one-to-one training and active control conditions. Children in each group were presented with the same stimuli in sequential order, but whereas the training group was instructed to both complete a processing task (e.g., judge veracity of sentence) and remember the item for later recall (e.g., the final word in the sentence), the control group completed only the processing task without the additional storage requirement. Word reading and Number skills (British Ability Scales-II) achievement tests were administered at pretest, posttest, 6-month follow-up, and 12-month follow-up sessions. Again, despite evidence for near transfer to working memory tests for the training group, "there was no difference between the groups in their gains on single word reading and mathematics over 12 months" (p. 84). That is, no transfer to academic outcomes was seen at posttest, 6 months later, or 12 months later.

Holmes et al. (2009)

Holmes et al. (2009) tested children with poor working memory on Cogmed adaptive training or Cogmed non-adaptive, low-working memory control interventions. Reading (Basic Reading, WORD) and math (Mathematical Reasoning, WOND) transfer was assessed at pretest and posttest for both groups – unfortunately, only the training group received these tests at the 6-month follow-up session, so there is no way to disentangle follow-up performance due to training versus maturational effects. However, compared to the training group, the control group showed numerically larger gains from pretest to posttest on both reading and math, leading the authors to state "adaptive training had little detectable impact on measures of the children's academic skills immediately following completion of training" (p. 13). As noted earlier, Dunning et al. (2013) was an attempt to replicate Holmes et al. (2009) with a more rigorous experimental design and also found no beneficial effects of working memory training on measures of academic achievement.

Karbach, Strobach, and Schubert (in press)

Karbach and colleagues compared two groups each of 14 typically-developing children on adaptive versions of complex span tasks from Braintwister training battery and non-adaptive, low-load active control versions of the same tasks. Standardized tests of reading (Knuspels Reading Tasks) and math (German Mathematics Test) were administered at pretest, posttest, and 3-month follow-up sessions. Karbach et al. reported significantly greater reading pretest-to-posttest gains for the training group versus the control group, although the training effect was no longer significant at follow-up. The math test showed no effects of working memory training. The authors conclude that the study "provides strong new evidence for the effectiveness of WM training", although because of the sample size, "the findings will have to be replicated in larger samples" (p. 12). As noted above, given the small sample size used, the results should be interpreted cautiously.

Van der Molen, Van Luit, Van der Molen, Klugkist, and Jongmans (2010)

Van der Molen et al. (2010) tested 95 adolescents from schools in which a requirement for entrance was an IQ in the range of 55–85. The training group trained three times per week, in 6 minute sessions, on an adaptive complex span task (click on the shape that differs from the others, remember the location of the yellow shape for later recall). One active control group saw the same stimuli as the training group, but only made the processing decision without the additional storage requirement, whereas the other active control group received a non-adaptive, low-load version of the task (note that the authors referred to this latter group as another training group, but we consider it an active control group). Academic outcomes were assessed via arithmetic (Tempo Test Arithmetic) and reading (One-minute reading test) tasks administered at pretest, posttest, and 10-week follow-up sessions. Using one-tailed significance tests, the authors observed no significant training effects on the reading test, at either posttest or follow-up, in comparison to either control group. For the arithmetic outcome, no significant effects were observed at posttest in comparison to either control group. However, at follow-up, although there was no significant difference between the adaptive and non-adaptive groups, the authors reported that the adaptive training group exhibited transfer (one-tailed) relative to the processing-task only control group. Using information presented in Van der Molen et al. (2010) Table 1b, we calculated an effect size of Cohen's $d = .10$ for this lone significant (one-tailed) effect. Thus, we are skeptical of their conclusion that their study shows that working memory "can be trained effectively with a fanning out effect on scholastic and other everyday tasks" (p. 445).

To summarize the results of the nine studies just reviewed, one study (Van der Molen et al., 2010) provided very weak evidence for delayed transfer to arithmetic but not reading outcomes, one study (Karbach et al., in press) provided evidence for immediate transfer to reading but not math outcomes (nor delayed transfer), and seven studies provided no evidence for immediate or follow-up transfer to a variety of reading, spelling, and arithmetic outcomes (Alloway et al., 2013; Ang et al., 2015; Chacko et al., 2014; Dunning et al., 2013; Gray et al., 2012; Henry et al., 2014; Holmes et al., 2009). Note that all nine studies produced evidence of near transfer to short-term and working memory tasks similar to those trained.

Although there are substantial limitations in studies that only employ a passive control group, we highlight two such studies from the remaining nine studies listed in Tables 2 and 3. These two studies (Bergman-Nutley & Klingberg, 2014; Rode, Robson, Purviance, Geary, & Mayr, 2014) are notable because of their extremely large sample sizes relative to the other studies in the literature.

Bergman-Nutley & Klingberg (2014)

Bergman-Nutley and Klingberg (2014) administered Cogmed to subjects recruited through clinicians via self-reported memory and attention problems. The subjects in training group (n = 162 at pretest) were diverse in age (7 to 15 years old) and the nature of their cognitive deficit, and completed the training at home or in the clinic. The passive-control group (n = 268 at pretest) was composed of typically developing students recruited through classroom newsletters who completed the transfer assessments (including a speeded arithmetic test) at school in a group setting. Transfer sessions were administered five times during the course of the study for both groups. The authors observed a significant effect of training on the math outcome at posttest. Notably, the training and passive-control groups did not differ in their math performance on the first 4 transfer sessions (cf. Bergman-Nutley & Klingberg, Figure 1), and only diverged at the final, fifth session. Because of the way the training and control groups were formed, the training group started with significantly lower working memory scores at pretest ($p < .001$), further complicating interpretation of the results. While the authors conclude the results are "encouraging regarding the potential role of cognitive training for education" (p. 869), the lack of random assignment and the large differences between the training and passive-control groups' composition, baseline working memory, and testing situations make interpretations of data from this study very difficult.

Rode et al. (2014)

Rode et al. (2014) conducted a working memory training study with nearly 300 third-graders across multiple schools in the state of Oregon. The training consisted of four weeks of practice each school day on an adaptive version of the operation span task (Turner & Engle, 1989), which displays individual digits for later recall interleaved with math operations for the subject to mentally compute. After a series of operation and digit presentations, the participant must recall the digits in the correct serial order. Rode et al. (2014) examined transfer to reading and math outcomes based on state standardized tests (Easy Curriculum-Based Measurement) and Reading Comprehension and Mathematical Reasoning subtests of the WIAT. Despite 156 students in the training group and 126 students in a passive-control group, Rode et al. (2014) observed a small positive effect on only 1 (CBM-Math) of the 4 possible reading and math outcomes. Rode et al. (2014) concluded the "results do not provide strong evidence in favor of substantial and educationally meaningful transfer gains that go beyond the benefits achieved through regular classroom instruction" (p. 7).

Discussion

The bulk of the evidence from studies with rigorous methodology provide little evidence for the efficacy of working memory training in improving academic and achievement outcomes such as reading, spelling, and math. The observation of positive near transfer to working memory and lack of academic or achievement test far transfer corresponds with previous meta-analyses (Melby-Lervåg & Hulme, 2013; Rapport et al., 2013), and indicates that contrary to popular belief, the evidence for the educational benefit of working memory training is lacking. Of course, there are a number of differences among studies, including the nature of the training and the composition of the sample, as mentioned previously. Other aspects of the training programs that vary across studies include the frequency (number of times per week), duration (length of each session and number of overall sessions), and location (in school, at home, in lab) of training. Another variable that differed among the studies reviewed here is one that typically appears in educational interventions: whether or not the training and control groups were randomized between classes/schools versus within the same classes/schools. Additionally, the studies varied in whether or not they assessed follow-up transfer, and the duration of time that had passed since posttest in the studies that did include follow-up assessments. Despite the studies differing in these various qualities, the results were relatively consistent – working memory training did not improve educational outcomes.

There are limitations to our review. Although we focus on academic and achievement tests, other outcomes are typically assessed in the literature (e.g., IQ, behavioral ratings). Certainly, one could argue that these outcomes are also educationally relevant, but there have already been numerous meta-analyses and reviews examining these outcomes in children (Chacko et al., 2013; Melby-Lervåg, & Hulme, 2013; Rapport et al., 2013; Shipstead, Hicks, & Engle, 2012), and the results from studies with active-control groups and/or blinded raters indicate no benefits for working memory training. In addition, we focused specifically on working memory training, in contrast to other 'general' interventions such as other forms of cognitive training (attention, task-switching, inhibition) and videogame training (for review, see Jacob & Parkinson, in press). We recommend that in contrast to unstructured, unguided, general interventions such as cognitive training and videogame training, more research should be focused on training specific skills and abilities that are likely to exhibit near transfer to very similar academically relevant outcomes – for example, training specific language skills in children with text comprehension difficulties (Clarke, Snowling, Truelove, & Hulme, 2010), or computer-assisted instruction of reading and math skills (Rabiner, Murray, Skinner, & Malone, 2010).

Despite optimistic reports from early working memory training studies, and claims advertised on various commercial websites touting the benefits of working memory training, the evidence indicates that working memory training does not reliably improve academic or other educational outcomes. We conclude with two notes about the utility and cost of working memory training. First, one of the intents of working memory training is to help individuals who have lower cognitive functioning (e.g., poor working memory, low IQ, children with ADHD, students with dyslexia), as seen in most of the studies in Table 2. The logic is that students with working memory impairments would benefit most from working memory training, and although the training may not equate them with their high-functioning or typically-developing peers, at least the training would improve their academic level to age-appropriate levels. However, in the few studies that have examined such aptitude-by-treatment interactions, the opposite pattern has emerged. Specifically, as measured by scores at pre-test, high-ability children are more likely than low-ability children to show larger training gains on the training tasks (Jaeggi, Buschkuehl, Jonides, & Shah, 2011; Rode et al., 2014). Thus, following the assumption by some researchers that more improvement on the training tasks would be associated with more transfer (Jaeggi et al., 2011), even if working memory training worked, it might serve to further exacerbate pre-existing differences between high- and low-ability children (the so-called "Matthew effect"; Walberg & Tsai, 1983). Second, in terms of the cost of working memory training, one may ask if there's a downside to using it. Even if working memory training has limited transfer, is there any harm in using a program, especially if it makes the child believe his or her working memory has improved? We would argue that the problem with using working memory training is that it most likely represents an opportunity cost. Commercial working memory training programs are typically not free, and in some cases cost thousands of dollars. What would that individual or school corporation have otherwise spent that money on? In the case of school corporations, what other curricula or programs could have been implemented in the time that was spent having students do working memory training (Diamond & Lee, 2011)? For the child with ADHD who suspends effective cognitive behavioral therapy or pharmaceutical treatment in favor of Cogmed, what impact does this have on the child's development? Based on the empirical evidence, we agree with the conclusion of Rode et al. (2014): "attempts to use working memory training programs in a pedagogical/clinical context should be considered with great caution" (p. 7).

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Table 1

Criteria for Strong Evidence of Working Memory Training Efficacy in Educationally-Relevant Context

General	
1	Use of active-control group
2	Use of large sample sizes in each training and control group
3	Use of objective measures
4	Evidence for positive transfer results to working memory
5	Transfer results follow a sensible pattern
6	(Follow-up transfer assessment)
7	(Multiple measures of each construct)
Specific to current review	
1	Studies with children (not adults)
2	Working memory training (not task-switching, inhibitory-control, videogame)
3	Working memory and education/achievement-related transfer (not nonverbal or verbal IQ/intelligence, ADHD ratings)

Table 2

Study	Age	Sample	Training (n)	Control (n)	Duration
Alloway 2013	10–11	Learning difficulties	Jungle Memory 4x/wk (23)	Jungle Memory 1x/wk (32) Passive (39)	8 weeks
Ang 2015	6–7	Low WM	Cogmed (25) Updating (32)	Updating w/o memory (28) Passive (26)	8 weeks
Chacko 2014	7–11	ADHD	Cogmed (44)	Cogmed low (41)	5 weeks
Dunning 2013	7–9	Low WM	Cogmed (34)	Cogmed low (30) Passive (30)	6 weeks
Gray 2012	12–17	LD+ADHD	Cogmed (20)	Academy of Math (32)	5 weeks
Henry 2014	5–8	Typically developing	Complex span (18)	Span w/o memory (18)	6 weeks
Holmes 2009	8–11	Low WM	Cogmed (22)	Cogmed low (20)	5–7 weeks
Karbach 2014	7–9	Typically developing	Complex span (14)	Complex span low (14)	9 weeks
Van der Molen 2010	13–16	Low IQ	Complex span (41)	Complex span low (26) Span w/o memory (26)	5 weeks
Alloway 2012	11–13	Learning difficulties	Jungle Memory (8)	Extra class lessons (7)	8 weeks
Aries 2014 E2	14–16	Preparatory school	History-based WM (26)	Passive (17)	12 weeks
Bergman-Nutley 2014	7–15	WM deficits	Cogmed (162)	Passive (268)	5 weeks
Egeland 2013/Hovik 2013	10–12	ADHD	Cogmed (33)	Passive (34)	5–7 weeks
Foy 2014	4–6	Low income	Cogmed (23)	Passive (28)	5 weeks
Kroesbergen et al 2014	5–6	Low math	Verbal WM (15) Verbal/Numeric WM (15)	Passive (21)	4 weeks
Rode 2014	8–9	Typically developing	Complex span (156)	Passive (126)	4 weeks
St Clair-Thompson 2010	5–8	Typically developing	Memory Booster (46)	Passive (77)	6–8 weeks
Witt 2011	9–10	Typically developing	Strategy WM (19)	Passive (19)	6 weeks

Note. Age in years. Sample sizes were not consistent across all transfer measures in all studies, so where possible the number of subjects completing the academic transfer measures is reported.

Table 3

Study	**Active**	**Sample**	**Near**	**Sensible**	**Follow**	**Results**
Alloway 2013	**Y**	**Y**	**Y**	**N**	**Y**	**No transfer to spelling or math**
Ang 2015	**Y**	**Y**	**Y**	**Y**	**Y**	**No transfer to math**
Chacko 2014	**Y**	**Y**	**Y**	**Y**	**N**	**No transfer to reading, spelling, or math**
Dunning 2013	**Y**	**Y**	**Y**	**Y**	**Y**	**No transfer to reading and math**
Gray 2012	**Y**	**Y**	**Y**	**Y**	**N**	**No transfer to reading, spelling, or math**
Henry 2014	**Y**	**N**	**Y**	**Y**	**Y**	**No transfer to reading or number skills**
Holmes 2009	**Y**	**Y**	**Y**	**Y**	**N**	**No transfer to reading or math**
Karbach 2014	**Y**	**N**	**Y**	**Y**	**Y**	**Transfer to reading, no transfer to math**
Van der Molen 2010	**Y**	**Y**	**Y**	**Y**	**Y**	**No transfer to reading or arithmetic**
Alloway 2012	?	N	Y	N	N	Transfer to math, no transfer to spelling
Aries 2014 E2	N	N	Y	Y	N	No transfer to history reasoning school exam
Bergman-Nutley 2014	N	Y	Y	Y	N	Transfer to math; impaired WM training vs TD control
Egeland 2013/Hovik 2013	N	Y	Y	N	Y	Transfer to reading, no transfer to math
Foy 2014	N	Y	Y	Y	N	No transfer to pre-reading skills
Kroesbergen et al 2014	N	N	Y	Y	N	Transfer to early numeracy skills
Rode 2014	N	Y	Y	Y	Y	Transfer to reading fluency, no transfer to reading comprehension or math
St Clair-Thompson 2010	N	Y	Y	?	Y	Transfer to mental math, no transfer to reading and 2 other arithmetic tests
Witt 2011	N	N	Y	Y	N	Transfer to addition

Note. Studies in **bold** considered strongest evidence given their use of an active-control group. Active: Did study include active control group? Sample: Did each group in study have sample size with minimum of 20 subjects? Near: Did results indicate near transfer to working memory? Sensible: Did transfer results exhibit a sensible pattern? Follow: Did study include follow-up assessment?