Critical Review:
The efficacy of working memory intervention in children

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This critical review examines the efficacy of working memory intervention in children. A literature search using computerized databases was completed resulting in four articles meeting the inclusion criteria. Study designs included randomized controlled trials and nonrandomized mixed designs, one with partial randomization. Overall, research suggests that systematic training that taxes working memory is effective in significantly augmenting working memory skills directly related to a trained task. However, demonstration of a carry-over effect to untrained related tasks was limited.

Introduction

Working memory (WM) allows one the capacity to store, maintain, and transform information over brief periods of time. It is a multifocal system rather than a single storage unit, which compartmentalizes several interacting components, and is associated with the frontal lobes (Alloway et al. 2006, Holmes et al. 2009 & Klingberg et al. 2005). WM plays an integral role in supporting leaning and maintaining focused behaviour (Holmes et al. 2009). Various WM models have been proposed to illustrate its structure and function. However, what has remained a constant research finding is that WM is linked to the ability to learn.

Working memory has also been coupled with and closely associated with scholastic achievement, as many academic skills, such as reading and mathematics, require the employment of a functioning WM system (Alloway & Archibald, 2008 & Holmes et al. 2009). Rudimentary forms of WM are present early in life, but show rapid development in the preschool and early-school age years (Thorell et al. 2009). Thus, it should come as no surprise that children with a compromised WM system will likely encounter challenges in academia.

According to Gathercole and Alloway (2008), low WM is not a rarity in children. Of those whose WM abilities fall below the tenth percentile, 80% will encounter significant challenges in reading or mathematics, or more commonly both. The paramount importance of a functioning WM system has led researchers to investigate its 'trainability' and transfer effects. Recent attention to the area has led researchers to question the functionality of WM, the potential of boosting it through systematic WM training, and whether it will have lasting carry-over effects.

The implications of boosting one's WM are thus tri-fold: (1) educationally, (2) professionally, and (3) socially. Therefore, it is necessary to examine the effectiveness of WM intervention in children in order for clinicians to apply the appropriate skills and tasks to augment WM to an age appropriate level.

Objectives

The primary objective of this paper is to critically evaluate existing literature regarding the efficacy of WM intervention for children. The secondary objective is to propose evidence-based practice recommendations for speech-language pathologists whose caseload may be comprised of certain children who have low working memory function.

Methods

<u>Search Strategy</u>
Computerized databases, including PubMed and PsychInfo were searched using the following search strategy: (WM) OR (WM intervention) OR (training WM) AND (children). Articles were also located by searching the authors' surnames.

The search was limited to articles written in or translated to English.

<u>Selection Criteria</u>
Studies were selected for this critical review that implemented WM training. Only articles that investigated WM intervention in children were considered for inclusion.

<u>Data Collection</u>
Results of the literature search produced the following types of articles congruent with the aforementioned selection criteria: two randomized controlled trials, and two nonrandomized mixed

designs (including one study with partial randomization).

Results

The following studies are presented in a hierarchical manner, which reflects the level of evidence provided.

Klingberg, et al. (2005) investigated the effects of WM training using a computer program for children with ADHD. A randomized control trial was employed for this study. Included in the trial were 53 children identified through referral sources, who were clinically diagnosed with ADHD and not taking stimulant medication. Participants were randomly assigned to a treatment group (*n*=18) and a comparison group (*n*=18). All children completed computer activities for 90 trials per day (around 40 minutes for 25 days over 5-6 weeks). The treatment group completed activities where the difficulty automatically increased in difficulty. The comparison group completed the same activities, but the difficulty did not increase and remained at the initial low level. Participants were assessed, using multiple standardized outcome measures by raters blinded to the treatment group status prior to commencing intervention, post intervention and at a 3-month follow-up.

An appropriate general linear model analysis was used to examine the effect of computerized WM training. Significant treatment effects were demonstrated in the treatment group on the trained tasks and related visuospatial and verbal tasks (untrained tasks) both at post intervention and at follow-up. The researchers noted that parent, but not teacher rating of ADHD symptoms, significantly declined.

Holmes et al. (2009) investigated whether poor progress in reading and mathematics due to low WM skills could be overcome by training WM. A nonrandomized mixed design was employed for this study. Children with poor WM (scored at or below 15th centile) were assigned to an adaptive program (*n*=22) and a non-adaptive version (*n*=20) via a routine screening of 345 children. The programs were implemented in two separate schools to control the environmental context. Both groups completed 115 trials every day (around 35 minutes for 20 days over 5-7 weeks). The task difficulty was matched to their current memory span for the adaptive version, but remained at the initial low level for the non-adaptive version. Participants were assessed using multiple standardized outcome measures (WM, following directions, and ability measures), prior to commencing intervention, post intervention and at a 6-month follow-up (only adaptive group). A passive control group (*n*=25) completed the pre and post-assessment. No significant differences were found in test and re-test scores of the passive control group, thus eliminating the chance of practice effects.

A series of MANOVAs established there was no significant difference between the adaptive and non-adaptive groups at pre-training on three of the four WM measures (except visuo-spatial STM). ANCOVAs were performed with the visuo-spatial STM measure as a covariant. Results of an ANOVA revealed that children who completed the adaptive training showed significant improvements in all aspects of WM compared to the non-adaptive training group; these effects maintained six months post training. No significant gains were found immediately following the training in ability measures (IQ, reading, and mathematics). The adaptive group did show significant gains in their 6-months post-training score in mathematics compared to pre-training baseline.

Holmes et al. (2009) attribute the limited amount of academic gains to the fact that these skills are expected to take longer to show significant advances in standardized measures. It is suggested that WM training may be beneficial to students who currently facing challenges academically, but do not qualify for special needs.

Thorell et al. (2009) examined the effects of WM and inhibition training in preschool children. A nonrandomized mixed design with partial randomization was employed for this study. All children between the ages of four and five at four preschools were asked to participate in the study. Participants at two of the preschools were randomly assigned to either the WM training (*n*=17) or inhibition training (*n*=18) group, a third preschool was the active control group (*n*=14), and a fourth preschool acted as a passive control group (*n*=16). The two training groups completed 15 minutes of training for five weeks; the control group played commercially available computer games that did not tax WM. All participants were assessed by raters blinded to the treatment group status using multiple standardized outcome measures, prior to commencing intervention, and at post intervention.

The results of an ANCOVA, controlled for age and gender, revealed significantly larger improvements over time for the WM group, when compared to the inhibition group and the combined control group (shown in separated analysis to not be significantly different).

Alloway and Alloway (2009) examined the effects of WM intervention on crystallized intelligence in children with learning disabilities in an unpublished randomized control trial. Participants were randomly assigned to a training group (n=8) that received WM training (cognitively based) or a control group (n=7) that received non-computer based targeted education support (knowledge based). Participants were assessed, using pre- and post-measures prior to commencing an eight-week intervention period that assessed crystallized intelligence, academic attainment, and WM. The training and control groups did not differ significantly in their pre-training score across all three measures. Both groups completed tasks three times a week for half-an-hour over an 8-week period. The training group completed activities where the difficulty automatically increased in difficulty. The comparison group completed the same activities, but these remained at the initial low level.

The gains of memory training were compared across the two groups by computing Mann-Whitney U. The results of the study demonstrated significant improvements in crystallized intelligence using WM training in the training group compared to the control group. Alloway and Alloway (2009), thus, reportedly demonstrated that improvement were not restricted to trained skills but transferred to acquired skills in crystallized intelligence and academic achievement. However, at present this data remains incomplete, as it has not been peer assessed, and thus difficult to critically analysis.

Discussion

<u>Subject Selection and Characteristics</u>
Children were involved in all of the studies, although there was some variance in the characteristics of the participants. All studies chose participants who where likely to have decreased WM (e.g. ADHD, learning disabilities, low working memory) except for Thorell et al. (2009) who based their study on preschoolers.

The distribution between males and females in the experimental groups of Klingberg at al. (2005) and Thorell et al. (2009) was similar. Alloway and Alloway (2009) and Holmes et al. (2009) employed more males compared to females. Identifying characteristics were provided in all studies, except for Alloway and Alloway (2009). This is likely due to the nature of being in an unpublished format. Without a sufficient amount of participant information it is difficult to delineate significant treatment effects.

Subjects were selected through referrals, large-scale screening results, and preschool enrolment. However, Alloway and Alloway (2009) did not outline the details of their subject selection. It is possible that selection bias could have occurred in the former mentioned study, thus potentially affecting the generalizability of results.

While studies varied in their sample size, Klingberg et al. (2005), Holmes et al. (2009) and Thorell et al. (2009) included a reasonable number of participants, thus holding a higher degree of confidence in the ability to detect differences between groups (78, 42, and 65 participants respectively). Alloway and Alloway (2009) presented only 15 participants, subsequently divided to a group of 8 and 7 participants. This low number of participants and small group distribution limits the generlizability of the findings and reduces the probability of detecting true existing differences.

<u>Methods</u>
All analyzed studies incorporated appropriate and valid outcome measures, implementing pre- and post-test assessments analyzing WM and other related ability measures. WM training periods ranged from five to eight weeks (mean length 6 weeks). Examiners were blinded in the administration of training and outcome measures in Klingberg et al. (2005), and Thorell et al. (2009). This aids in diminishing the potential bias of the examiners.

The exclusion of participants due to lack of compliance during training may result in a lack of randomization, as only the data of 'compliers' is being included. This was apparent in Klingberg et al. (2005), and thus potentially diminishes the strength of the results being generalizable. However, Klingberg et al. (2005) was the sole study that explicitly mapped out the entirety of the participants journey throughout their study. This is a key component as it reduces the possibility of selection bias by providing the reader with all of the methods that were taken in order to attain the reported results.

Follow-up testing was conducted by Klingberg et al. (2005): 3-months post assessment, and Holmes et al. (2009): 6-moths post assessment. Although it is appropriate to conduct a timely follow-up to assess the generlization of skills, one must be cautioned in the conclusions drawn. Holmes et al.'s (2009) attribution of mathematical gains would have held more merit if a 3-month and 9-month follow-up demonstrated the same gains.

Certain aspects of the studies lacked specific detail regarding the procedures used therein. For instance, Thorell et al. (2009) were not forthcoming with important methodological information, such as describing how the data were used to combine the two control groups (active and passive). Such information is pertinent to the replication of the study and to ensure the effect is not being manipulated.

<u>Statistical Analysis</u>
Appropriate statistical measures were computed for all analyzed studies.

<u>Level of Evidence</u>
When comparing the level of evidence, validity and clinical importance seen across all four studies, the findings of each can be considered suggestive on all levels.

Klingberg et al. (2005) employed sound methodological techniques, including randomized assignment in a level 1 experimental design. However, this study remains suggestive as participants improved on trained WM skills only, as opposed to an improvement in ability measures, for example, reading and/or mathematics, thus demonstrating a lack of a carry-over effect. Klingberg et al. (2005) did note a decrease in ADHD symptoms, conversely, this was based on parent report who were not blinded to the study design not teachers, thus the potential for bias is likely significant.

Holmes et al. (2009) demonstrated a significant gain in one ability measure 6-months post training in a level 2a experimental design. Nevertheless due to the aforementioned limitations their conclusions remain suggestive.

Thorell et al. (2009) utilized a moderate study design in a level 2a experimental design, although there were numerous limitations present with respect to methodology. The lack of a follow-up period and measurement of ability measures result in the study being deemed somewhat suggestive.

Although Alloway and Alloway's (2009) level 1 experimental design encompassed many limitations due to its unpublished format, the results demonstrated a crossover affect from WM training to improved ability measures. These results must be interpreted with caution, thus this study remains at the somewhat suggestive level.

Conclusion & Recommendations

There is growing and consistent body of evidence supporting the belief that WM can be trained and improve WM deficits. While the literature reviewed here demonstrated the significant effects of WM intervention, the generalizability of the newly acquired skills remains unclear.

These constraints, coupled with the various limitations of each study strongly suggest the need for future research in the area of WM intervention. It is recommended the future research take the following into account:

a) Adequate sample sizes and distributions of participants into experimental groups that will increase the power and facilitate appropriate conclusions for the populations as a whole.

b) Specific explanation of inclusion/exclusion criteria, including identification of any premorbid conditions that may impact upon the generlizability to the population.

c) Inclusion of specific populations that may benefit from WM training, in order to compare gains in skills, and be able to more appropriately target interventions.

d) Employment of appropriate ability measures, for example, reading, mathematical reasoning, and problem solving, to pre-/post-testing as well as a timely follow-up.

e) Integration of measuring baselines at numerous time points to increase the strength of significant findings, and ability to attribute significant improvements to training.

f) Blinding should be ensured during assessment and training by the experimenter in order to decrease the amount of bias present.

Clinical Implications

Despite the limitations of the abovementioned studies, the results and conclusions discussed have important clinical implications for speech-language pathologists.

- Evidence does illustrate a significant effect of WM intervention.

- There is symbiotic relationship between WM and learning, there are likely benefits that may translate from one to another. This is specifically relevant for children whose poor academic performance may be implicated by low WM.

- By boosting children's WM capabilities and augmenting them to their peer's levels, a decrease of other academic demands may begin to diminish or ameliorate.

- Clinicians are still cautioned when interpreting the immediate applicability of the results, as future research is currently required to determine which populations may benefit from WM intervention.

- Clinically, one may begin to observe and note a client's unique challenges, which may be linked to low WM. By amalgamating a series of clinical impressions, it in turn strengthens the knowledge basis in the area of WM.

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