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Abstract

The putative effectiveness of working memory (WM) training at enhancing cognitive and academic skills is still ardently debated. Several researchers have claimed that WM training fosters not only skills such as visuospatial WM and short-term memory (STM), but also abilities outside the domain of WM, such as fluid intelligence and mathematics. Other researchers, while acknowledging the positive effect of WM training on WM-related cognitive skills, are much more pessimistic about the ability of WM training to improve other cognitive and academic skills. In other words, the idea that far-transfer – i.e., the generalization of a set of skills across two domains only loosely related to each other – may take place in WM training is still controversial.

In this meta-analysis, we focused on the effects of WM training on cognitive and academic skills (e.g., fluid intelligence, attention/inhibition, mathematics, and literacy) in typically developing (TD) children (aged three to 16). While WM training exerted a significant effect on cognitive skills related to WM training ($\bar{g}$ = 0.46), little evidence was found regarding far-transfer effects ($\bar{g}$ = 0.12). Moreover, the size of the effects was inversely related to the quality of the design (i.e., random allocation to the groups and presence of an active control group).

The results suggest that WM training is ineffective at enhancing TD children's cognitive or academic skills and that, when positive effects are observed, they are modest at best. Thus, in line with other types of training, far-transfer rarely occurs and its effects are minimal.

Keywords: working memory; training; transfer; meta-analysis; intelligence; academic achievement.

Introduction

Transfer of learning occurs when a set of skills acquired in a particular domain generalizes to other domains. The occurrence of transfer is either a tacit assumption or a deliberate objective of most educational interventions: any learned skills are meant to be applied beyond the learning context (Perkins & Salomon, 1994). For example, one's ability in analytic geometry is supposed to generalize to calculus.

According to Thorndike and Woodworth's (1901) common element theory, transfer is a function of the extent to which two tasks share common features and cognitive elements. In accordance with this hypothesis, while *near*-transfer – i.e., the transfer of skills between strictly related domains (e.g., analytic geometry and calculus) – takes place frequently, *far*-transfer – i.e., the transfer occurring between source and target domains weakly related to each other (e.g., Latin and mathematics) – has rarely been observed (Donovan, Bransford, & Pellegrino, 1999). Examples of failed far-transfer include teaching the computer language LOGO to improve children's reasoning skills (De Corte & Verschaffel, 1986; Gurtner, Gex, Gobet, Nunez, & Restchitzki, 1990) and, as reported in a recent meta-analysis (Sala & Gobet, 2016), teaching chess to improve children's cognitive and academic skills.

The training investigated in those studies was highly specific (learning a programming language and chess, respectively). However, it is possible that boosting a domain-general cognitive mechanism is an effective way to improve other cognitive and real-life skills, such as academic achievement. This assumption is the key principle underlying the research on WM training.

Working Memory Training

WM is the cognitive system used to store and manipulate the information necessary to carry out cognitive tasks (Baddeley, 1992). Measures of WM capacity, such as the number of items WM can store and the ability to keep information in active memory during interfering tasks, correlate positively with fluid intelligence (Engle, Tuholski, Laughlin, & Conway, 1999) and measures of cognitive control such as the Stroop task (Kane & Engle, 2003), the go/no-go task (Redick, Calvo, Gay, & Engle, 2011), and the dichotic-listening task (Conway, Cowan, & Bunting, 2001). In addition, WM capacity is related to academic skills such as reading comprehension (Conway & Engle, 1996) and mathematical ability (Peng, Namkung, Barnes, & Sun, 2016). WM also seems to play a fundamental role in cognitive development. Deficits in WM capacity in children are associated with reading difficulties (Swanson, 2006), mathematical disorders (Passolunghi, 2006), attention deficit/hyperactivity disorder (ADHD; Klingberg et al., 2005), and language impairment (Archibald & Gathercole, 2006).

Several hypotheses have linked WM to intelligence and academic achievement. It has been proposed that WM and fluid intelligence share a common capacity constraint (Halford, Cowan, & Andrews, 2007). The amount of information (e.g., the number of items) that can be handled in WM is limited. Consequently, the number of interrelationships among elements that can be held and manipulated by WM in a reasoning task (e.g., Raven's progressive matrices) is bounded. If such limits are alleviated by training, then an improvement in fluid intelligence might occur (Au et al., 2015; Jaeggi, Buschkuehl, Jonides, & Perrig, 2008). Crucially, such an improvement is supposed to generalize to subject areas such as mathematics or literacy, because fluid intelligence is a key predictor of academic achievement (Deary, Strand, Smith, & Fernandes, 2007; Rohde & Thompson, 2007). Another related hypothesis concerns the role of attentional control processes in both working memory and fluid intelligence (Gray, Chabris, & Braver, 2003). Chein and Morrison (2010), for example, have suggested that WM training induces positive effects on measures of cognitive control (e.g., Go/no-go, Stroop task), which, in turn, boosts performance in other tasks outside the domain of WM. Finally, it has been hypothesized that WM training is especially beneficial for individuals with low WM capacity (e.g., children with ADHD or other learning disabilities). The idea is simple. If one's learning difficulties stem from reduced WM capacity, then training that specific skill might help to improve academic performance. The common assumption underlying these three hypotheses is that WM training boosts domain-general mechanisms (WM capacity, cognitive control, and attention), and hence enhances many other cognitive and academic skills.

However, in spite of a vast amount of research, no definite conclusion on the putative effectiveness of WM training at boosting cognitive skills and academic achievement has been reached yet. There is substantial agreement about the existence of near-transfer effects due to WM training – such as improvements in measures of verbal and non-verbal WM and short-term memory. However, while several reviews of the available experimental evidence have upheld the idea that WM training is a valuable cognitive enhancement tool (Au et al., 2015; Au, Buschkuehl, Duncan, & Jaeggi, 2016; Klingberg, 2010; Morrison & Chein, 2011), others have challenged the hypothesis according to which WM training effects substantially transfer to other cognitive skills outside the domain of WM (Dougherty, Hamovits, & Tidwell, 2016; Melby-Lervag & Hulme, 2013, 2016; Melby-Lervag, Redick, & Hulme, 2016; Redick, Shipstead, Wiemers, Melby-Lervag, & Hulme, 2015; Schwaighofer, Fischer, & Buhner, 2015; Shipstead, Redick, & Engle, 2010, 2012).

Working Memory Training in Children

Children represent an important population on which to test the ability of WM training to boost cognitive and academic skills. During childhood, cognitive ability and academic skills are still at the beginning of their development, and, thus, cognitive training is likely to be more efficient than in adulthood. In agreement with this idea, research into expertise has clearly established that the likelihood of far-transfer is inversely related to the level of expertise in a discipline, which needs several years to acquire (Ericsson & Charness, 1994; Gobet, 2015). That is, WM training is more likely to improve, for example, a child's basic arithmetic abilities than an undergraduate student's skill in solving differential equations. In fact, while the skill to develop is quite general and based to some extent on cognitive ability in the former case, it depends to a large extent on domain-specific knowledge in the latter case. Thus, from a theoretical point of view, children are an ideal population to test the occurrence of transfer.

Several recent reviews have addressed the issue of the putative benefits of WM training in children, without reaching any agreement. According to Klingberg (2010), WM training can be used as an effective remediating intervention. By contrast, Rapport, Orban, Kofler, and Friedman's (2013) meta-analysis reported little or no evidence of amelioration in academic achievement in children with ADHD after WM training. In line with Rapport et al.'s (2013) results, Redick et al.'s (2015) review showed that WM training did not provide any benefit to academic performance in children with ADHD (e.g., Chacko et al., 2014) and poor WM (e.g., Ang, Lee, Cheam, Poon, & Koh, 2015), or in typical developing children (e.g., Rode, Robson, Purviance, Geary, & Mayr, 2014).

Evaluating the effects of WM training on children with no learning disability has substantial practical and theoretical implications. If a brief training can improve overall cognitive ability and academic achievement, the impact of such an intervention on educational practices and policies would be profound. Any positive effect of WM training would provide an advantage for a vast cohort of individuals, not just for a relatively small sub-sample (children with ADHD or children with poor WM). However, it is yet to be established whether increasing WM capacity in typically developing (TD) children with no WM impairment can enhance academic achievement and cognitive abilities outside the domain of WM. The aim of the present study is to quantitatively evaluate the available evidence via meta-analysis.

The Present Meta-Analysis

The present meta-analysis focuses on the putative effectiveness of WM training at enhancing cognitive and academic skills in TD children. While several previous meta-analyses (e.g., Melby-Lervag & Hulme, 2013; Melby-Lervag et al., 2016; Schwaighofer et al., 2015) included studies dealing with the putative benefits of WM training in TD children, no meta-analysis has yet been specifically devoted to this issue.[1]

The main purpose of this meta-analysis is to estimate the overall effect sizes obtained with WM training with respect to near-transfer (i.e., WM-related outcomes) and far-transfer (i.e., outcomes outside the domain of WM). Also, we aimed to test the possible effects of several moderators, with particular attention to far-transfer measures (e.g., fluid intelligence, cognitive control, and academic achievement measures). Therefore, the meta-analysis followed five steps. First, to estimate the presence or absence of near-transfer and far-transfer at the end of the intervention, we calculated the overall standardized difference between WM training groups and control groups on (a) near-transfer measures (e.g., visuospatial working memory, short-term memory) and (b) measures related to abilities outside the domain of WM (e.g., fluid intelligence, cognitive control, mathematics).

Second, we carried out a moderator analysis. As noted in previous meta-analyses (e.g., Melby-Lervag & Hulme, 2013; Schwaighofer et al., 2015), two methodological features may be a major source of variability between intervention studies—random assignment to groups and the presence of an active control group to control for potential confounding effects (e.g., differences at baseline level between experimental and control groups,

[1] Weicker, Villringer, and Thöne-Otto's (2016) meta-analysis reported several overall effect sizes regarding the effect of WM training on TD children's cognitive abilities such as fluid intelligence and processing speed. However, the total sample included only nine studies.

Hawthorne effect). The absence of these features may result in an inflation of the positive effects of the training due to confounds such as differences at baseline level, self-selection of the treated sample, and placebos. Therefore, we evaluated the potential moderating effects of the type of control group (active or passive control group) and the presence of randomization for the assignment to the groups. We also investigated the potential moderating effects of the age of the participants and the total duration of the training. Third, we focused on the far-transfer effects and investigated whether WM training is more (or less) successful in boosting particular academic/cognitive skills. Fourth, we performed publication bias analyses. Finally, we calculated the follow-up overall effect sizes for near- and far-transfer measures.

Method

Literature Search

In accordance with the PRISMA statement (Moher, Liberati, Tetzlaff, & Altman, 2009), a systematic search strategy was used to find the pertinent studies. Using several combinations of the terms "working memory," "training," "cognitive," "intervention," and "children", we searched Scopus, ERIC, Psyc-Info, ProQuest Dissertation & Theses, and Google Scholar databases to identify all the potentially relevant studies. Also, earlier narrative reviews were examined, reference lists were scanned, and we e-mailed scholars in the field (n = 13) requesting unpublished studies and inaccessible data.

Inclusion/Exclusion Criteria

The studies were included according to the following six criteria:

1. The design of the study included an intervention aimed to train working memory skills (e.g., verbal working memory, visuospatial working memory); correlational and ex-post facto studies were excluded;
2. The study presented a comparison between a treated group and at least one control

group;

3. During the study, a measure of academic or cognitive skill other than working memory was collected; importantly, to assess a genuine near-transfer effect, all the measures of performance in the trained WM intervention task were excluded;
4. The participants in the study were aged three to sixteen;
5. The participants in the study were TD children without any specific learning disability (e.g., ADHD) or borderline cognitive ability (e.g., low IQ, poor working memory capacity);[2]
6. The data presented in the study (or provided by the author) were sufficient to calculate an effect size.

To identify studies meeting these criteria, we searched for relevant published and unpublished articles through April 1, 2016. We found 25 studies, conducted from 2007 to 2016, that met all the inclusion criteria. These studies included 26 independent samples and 104 effect sizes (30 for WM-related measures, see Table 1; 74 for non-WM-related measures, see Table 2), with a total of 1,601 participants. Finally, a subsample of the included studies (*n* = 6) reported follow-up effects. A total of 30 follow-up effect sizes were computed (6 for WM-related measures, see Table 3; 24 for non-WM-related measures, see Table 4), with a total of 249 participants.[3] The entire procedure is summarized in Figure 1.

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[2] In Shavelson, Yuan, Alonzo, Klingberg, and Andersson (2008), eight participants (out of 37) had ADHD or learning difficulties. Since separate results were not available, we calculated the effect sizes considering the whole sample of 37 participants.

[3] In Soderqvist and Bergman-Nutley (2015), no post-test assessment was administered immediately after the training, but only 24 months later. Thus, we included the effect sizes extracted from this study in both the main models and the follow-up models.

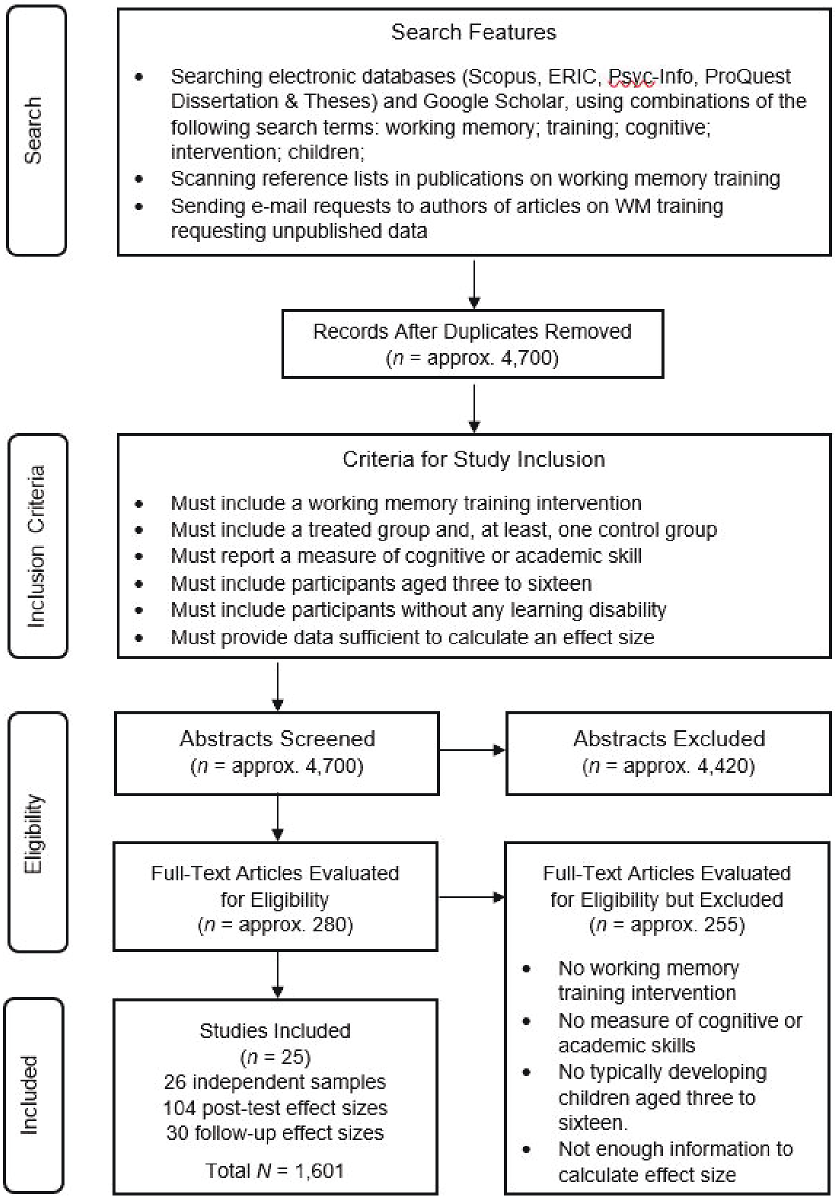

Figure 1. Flow diagram of the studies included in the meta-analytic review.

Moderators

We selected five potential moderators:

1. Random allocation (dichotomous variable): Whether the participants were randomly allocated to the groups;
2. Type of control group (active or passive; dichotomous variable): Whether the WM training-treated group was compared to another activity;

3. Duration of training (continuous variable): The total time of training in hours;
4. Age (continuous variable): The mean age (in years) of the participants; when the mean age was not provided (n = 3) we used either the median age (n = 1) or an age estimation based on the school grade (n = 2; e.g., third graders = 9-year-olds);
5. Domain (categorical variable): This variable, which was inserted only in the far-transfer model, includes literacy/word decoding, mathematics, science, fluid intelligence, crystallized intelligence, and cognitive control.[4]

The two authors coded each effect size for moderator variables independently. There was no disagreement with respect to Random allocation, Type of control group, and Age. Regarding the moderator Duration of training, 87% agreement was obtained. For the moderator Domain, the Cohen's kappa was κ = .95. The authors resolved every discrepancy.

Table 1

Studies and moderators of the 30 near-transfer effect sizes included in the meta-analysis

[4] These broad categories were built by aggregating different outcomes related to a particular domain (e.g., go/no-go task and Stroop task under the category of cognitive control). For all the details about the reviewed studies, see Tables S1.1 to S1.4 in the Supplemental material available online.

Study	**Age**	**Duration of training**	**Random allocation**	**Type of control group**
Bergman-Nutley et al. (2011) - M1	4.27	6.25	Yes	Active
Bergman-Nutley et al. (2011) - M2	4.27	6.25	Yes	Active
Henry, Messer, & Nash (2014)	7.00	3.00	Yes	Active
Karbach, Strobach, & Schubert (2015)	8.30	9.33	Yes	Active
Kroesbergen, Noordende, & Kolkman (2014) - M1	5.87	4.00	Yes	Passive
Kroesbergen, Noordende, & Kolkman (2014) - M2	5.87	4.00	Yes	Passive
Kuhn & Holling (2014) - S1	9.00	5.00	Yes	Passive
Kuhn & Holling (2014) - S2	9.00	5.00	Yes	Active
Kun (2007) - S1 - M1	12.84	8.00	Yes	Active

Kun (2007) - S1 - M2	12.84	8.00	Yes	Active
Kun (2007) - S2 - M1	13.52	14.58	Yes	Active
Kun (2007) - S2 - M2	13.52	14.58	Yes	Active
Kun (2007) - S2 - M3	13.52	14.58	Yes	Active
Lee (2014)	9.00	3.00	Yes	Active
Lindsay (2012)	5.49	3.00	Yes	Active
Passolunghi & Costa (2016) - S1 - M1	5.44	10.00	Yes	Active
Passolunghi & Costa (2016) - S1 - M2	5.44	10.00	Yes	Active
Passolunghi & Costa (2016) - S2 - M1	5.42	10.00	Yes	Passive
Passolunghi & Costa (2016) - S2 - M2	5.42	10.00	Yes	Passive
Pugin et al. (2015) - M1	13.00	8.05	No	Passive
Pugin et al.	13.00	8.05	No	Passive

(2015) - M2				
Rode, Robson, Purviance, Geary, & Mayr (2014)	9.00	7.14	Yes	Passive
Shavelson et al. (2008) - M1	13.50	14.58	Yes	Active
Shavelson et al. (2008) - M2	13.50	14.58	Yes	Active
St Clair-Thompson, Stevens, Huth, & Bolder (2010)	6.83	6.00	No	Passive
Studer-Luethi, Bauer, & Perrig (2016) - S1	8.25	4.50	Yes	Active
Studer-Luethi, Bauer, & Perrig (2016) - S2	8.25	4.50	Yes	Passive
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S1	4.67	6.25	No	Active
Thorell, Lindqvist,	4.67	6.25	No	Passive

Bergman, Bohlin, & Klingberg (2008) - S2				
Witt (2011)	9.68	7.50	No	Passive

Table 2

Studies and moderators of the 74 far-transfer effect sizes included in the meta-analysis

Study	Age	Duration of training	Random allocation	Type of control group	Domain
Bergman-Nutley et al. (2011)	4.27	6.25	Yes	Active	Fluid intelligence
Henry, Messer, & Nash (2014) - M1	7.00	3.00	Yes	Active	Literacy/WD
Henry, Messer, & Nash (2014) - M2	7.00	3.00	Yes	Active	Mathematics
Horvat (2014)	not given	not given	No	Passive	Fluid intelligence
Jaeggi, Buschkuehl, Jonides, & Shah (2011) - M1	8.98	5.00	No	Active	Fluid intelligence
Jaeggi, Buschkuehl, Jonides, & Shah (2011) - M2	8.98	5.00	No	Active	Fluid intelligence

Karbach, Strobach, & Schubert (2015) - M1	8.30	9.33	Yes	Active	Literacy/WD
Karbach, Strobach, & Schubert (2015) - M2	8.30	9.33	Yes	Active	Mathematics
Karbach, Strobach, & Schubert (2015) - M3	8.30	9.33	Yes	Active	Cognitive control
Karbach, Strobach, & Schubert (2015) - M4	8.30	9.33	Yes	Active	Cognitive control
Kroensbergen, Noordende, & Kolkman (2014) - M1	5.87	4.00	Yes	Passive	Cognitive control
Kroensbergen, Noordende, & Kolkman (2014) - M2	5.87	4.00	Yes	Passive	Mathematics
Kuhn & Holling (2014) - S1	9.00	5.00	Yes	Passive	Mathematics

Kuhn & Holling (2014) - S2	9.00	5.00	Yes	Active	Mathematics
Kun (2007) - S1 - M1	12.84	8.00	Yes	Active	Fluid intelligence
Kun (2007) - S1 - M2	12.84	8.00	Yes	Active	Science
Kun (2007) - S2 - M2	13.52	14.58	Yes	Active	Science
Lee (2014) - M1	9.00	3.00	Yes	Active	Literacy/WD
Lee (2014) - M2	9.00	3.00	Yes	Active	Literacy/WD
Lindsay (2012) - M1	5.49	3.00	Yes	Active	Literacy/WD
Lindsay (2012) - M2	5.49	3.00	Yes	Active	Literacy/WD
Loosli, Buschkuehl, Perrig, & Jaeggi (2012) - M1	9.50	2.00	No	Passive	Fluid intelligence
Loosli, Buschkuehl, Perrig, & Jaeggi (2012) - M2	9.50	2.00	No	Passive	Literacy/WD
Mansur-Alves &	11.17	13.33	Yes	Passive	Fluid intelligence

Flores-Mendoza (2015) - M1					
Mansur-Alves & Flores-Mendoza (2015) - M2	11.17	13.33	Yes	Passive	Fluid intelligence
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013) - M1	9.19	10.00	Yes	Active	Fluid intelligence
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013) - M2	9.19	10.00	Yes	Active	Fluid intelligence
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013)	9.19	10.00	Yes	Active	Crystallized intelligence

- M3					
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013) - M4	9.19	10.00	Yes	Active	Literacy/WD
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013) - M5	9.19	10.00	Yes	Active	Mathematics
Mansur-Alves, Flores-Mendoza, & Tierra-Criollo (2013) - M6	9.19	10.00	Yes	Active	Literacy/WD
Nevo & Breznitz (2014) - M1	8.50	4.80	Yes	Active	Literacy/WD
Nevo & Breznitz	8.50	4.80	Yes	Active	Literacy/WD

(2014) - M2					
Passolunghi & Costa (2016) - S1	5.44	10.00	Yes	Active	Mathematics
Passolunghi & Costa (2016) - S2	5.42	10.00	Yes	Passive	Mathematics
Pugin et al. (2015) - M1	13.00	8.05	No	Passive	Fluid intelligence
Pugin et al. (2015) - M2	13.00	8.05	No	Passive	Cognitive control
Pugin et al. (2015) - M3	13.00	8.05	No	Passive	Cognitive control
Pugin et al. (2015) - M4	13.00	8.05	No	Passive	Cognitive control
Rode, Robson, Purviance, Geary, & Mayr (2014) - M1	9.00	7.14	Yes	Passive	Mathematics

Rode, Robson, Purviance, Geary, & Mayr (2014) - M2	9.00	7.14	Yes	Passive	Mathematics
Rode, Robson, Purviance, Geary, & Mayr (2014) - M3	9.00	7.14	Yes	Passive	Literacy/WD
Rode, Robson, Purviance, Geary, & Mayr (2014) - M4	9.00	7.14	Yes	Passive	Literacy/WD
Shavelson et al. (2008)	13.50	14.58	Yes	Active	Fluid intelligence
Soderqvist & Bergman-Nutley (2015) - M1	9.85	not given	No	Passive	Literacy/WD
Soderqvist & Bergman-Nutley	9.85	not given	No	Passive	Mathematics

(2015) - M2					
St Clair-Thompson, Stevens, Huth, & Bolder (2010) - M1	6.83	6.00	No	Passive	Literacy/WD
St Clair-Thompson, Stevens, Huth, & Bolder (2010) - M2	6.83	6.00	No	Passive	Mathematics
St Clair-Thompson, Stevens, Huth, & Bolder (2010) - M3	6.83	6.00	No	Passive	Mathematics
St Clair-Thompson, Stevens, Huth, & Bolder (2010) - M4	6.83	6.00	No	Passive	Mathematics
Studer-Luethi, Bauer, & Perrig (2016) - S1-M1	8.25	4.50	Yes	Active	Literacy/WD

Studer-Luethi, Bauer, & Perrig (2016) - S1-M2	8.25	4.50	Yes	Active	Mathematics
Studer-Luethi, Bauer, & Perrig (2016) - S1-M3	8.25	4.50	Yes	Active	Crystallized intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S1-M4	8.25	4.50	Yes	Active	Fluid intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S1-M5	8.25	4.50	Yes	Active	Cognitive control
Studer-Luethi, Bauer, & Perrig (2016) - S2-M1	8.25	4.50	Yes	Passive	Literacy/WD
Studer-Luethi, Bauer,	8.25	4.50	Yes	Passive	Mathematics

& Perrig (2016) - S2-M2					
Studer-Luethi, Bauer, & Perrig (2016) - S2-M3	8.25	4.50	Yes	Passive	Crystallized intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S2-M4	8.25	4.50	Yes	Passive	Fluid intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S2-M5	8.25	4.50	Yes	Passive	Cognitive control
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S1 - M1	4.67	6.25	No	Active	Cognitive control
Thorell, Lindqvist,	4.67	6.25	No	Active	Cognitive control

Bergman, Bohlin, & Klingberg (2008) - S1 - M2					
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S1 - M3	4.67	6.25	No	Active	Fluid intelligence
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S1 - M4	4.67	6.25	No	Active	Cognitive control
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S2 - M1	4.67	6.25	No	Passive	Cognitive control
Thorell, Lindqvist,	4.67	6.25	No	Passive	Cognitive control

Bergman, Bohlin, & Klingberg (2008) - S2 - M2					
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S2 - M3	4.67	6.25	No	Passive	Fluid intelligence
Thorell, Lindqvist, Bergman, Bohlin, & Klingberg (2008) - S2 - M4	4.67	6.25	No	Passive	Cognitive control
Wang, Zhou, & Shah (2014) - S1	10.50	6.67	Yes	Active	Fluid intelligence
Wang, Zhou, & Shah (2014) - S2	10.50	6.67	Yes	Active	Fluid intelligence
Wang, Zhou, & Shah	10.50	6.67	Yes	Active	Fluid intelligence

(2014) - S3					
Wang, Zhou, & Shah (2014) - S4	10.50	6.67	Yes	Active	Fluid intelligence
Witt (2011)	9.68	7.50	No	Passive	Mathematics
Zhao, Wang, Liu, & Zhou (2011)	9.76	not given	Yes	Passive	Fluid intelligence

Table 3

Studies and moderators of the 6 near-transfer follow-up effect sizes included in the meta-analysis

Study	**Age**	**Duration of training**	**Random allocation**	**Type of control group**
Henry, Messer, & Nash (2014)	7.00	3.00	Yes	Active
Karbach, Strobach, & Schubert (2015)	8.30	9.33	Yes	Active
Pugin et al. (2015) - M1	13.00	8.05	No	Passive

Pugin et al. (2015) - M2	13.00	8.05	No	Passive
Studer-Luethi, Bauer, & Perrig (2016) - S1	8.25	4.50	Yes	Active
Studer-Luethi, Bauer, & Perrig (2016) - S2	8.25	4.50	Yes	Passive

Table 4

Studies and moderators of the 24 near-transfer follow-up effect sizes included in the meta-analysis

Study	**Age**	**Duration of training**	**Random allocation**	**Type of control group**	**Domain**
Henry, Messer, & Nash (2014) - M1	7.00	3.00	Yes	Active	Literacy/WD
Henry, Messer, & Nash (2014) - M2	7.00	3.00	Yes	Active	Mathematics

Jaeggi, Buschkuehl, Jonides, & Shah (2011) - M1	8.98	5.00	No	Active	Fluid intelligence
Jaeggi, Buschkuehl, Jonides, & Shah (2011) - M2	8.98	5.00	No	Active	Fluid intelligence
Karbach, Strobach, & Schubert (2015) - M1	8.30	9.33	Yes	Active	Literacy/WD
Karbach, Strobach, & Schubert (2015) - M2	8.30	9.33	Yes	Active	Mathematics
Karbach, Strobach, & Schubert (2015) - M3	8.30	9.33	Yes	Active	Cognitive control
Karbach, Strobach, & Schubert (2015) - M4	8.30	9.33	Yes	Active	Cognitive control
Pugin et al. (2015) - M1	13.00	10.00	No	Passive	Fluid intelligence

Pugin et al. (2015) - M2	13.00	10.00	No	Passive	Cognitive control
Pugin et al. (2015) - M3	13.00	8.05	No	Passive	Cognitive control
Pugin et al. (2015) - M4	13.00	8.05	No	Passive	Cognitive control
Soderqvist & Bergman-Nutley (2015) - M1	9.85	not given	No	Passive	Literacy/WD
Soderqvist & Bergman-Nutley (2015) - M2	9.85	not given	No	Passive	Mathematics
Studer-Luethi, Bauer, & Perrig (2016) - S1-M1	8.25	4.50	Yes	Active	Literacy/WD
Studer-Luethi, Bauer,	8.25	4.50	Yes	Active	Mathematics

& Perrig (2016) - S1-M2					
Studer-Luethi, Bauer, & Perrig (2016) - S1-M3	8.25	4.50	Yes	Active	Crystallized intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S1-M4	8.25	4.50	Yes	Active	Fluid intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S1-M5	8.25	4.50	Yes	Active	Cognitive control
Studer-Luethi, Bauer, & Perrig (2016) - S2-M1	8.25	4.50	Yes	Passive	Literacy/WD
Studer-Luethi, Bauer, & Perrig (2016) - S2-	8.25	4.50	Yes	Passive	Mathematics

M2					
Studer-Luethi, Bauer, & Perrig (2016) - S2-M3	8.25	4.50	Yes	Passive	Crystallized intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S2-M4	8.25	4.50	Yes	Passive	Fluid intelligence
Studer-Luethi, Bauer, & Perrig (2016) - S2-M5	8.25	4.50	Yes	Passive	Cognitive control

Effect Size

The standardized means difference (Cohen's *d*) was calculated with the following formula:

$$d = (M_{g-e} - M_{g-c})/SD_{pooled-pre} \qquad (1)$$

where $SD_{pooled\text{-}pre}$ is the pooled standard deviation of the two pre-test standard deviations, and $M_{g\text{-}e}$ and $M_{g\text{-}c}$ are the gain of the experimental group and the control group, respectively (Schmidt & Hunter, 2015).[5] The follow-up effect sizes were calculated by using the standardized difference between the follow-up and the pre-test measures.

Finally, the Comprehensive Meta-Analysis (Version 3.0; Biostat, Englewood, NJ) software package was used for correcting the effect sizes for upward bias (Hedges' *g*; Hedges & Olkin, 1985), computing the overall effect sizes ($\bar{g}$s), and conducting statistical analyses.

Statistical Dependence of the Samples

The effect sizes were calculated for each relevant measure reported in the studies (Schmidt & Hunter, 2015). When several subscales of a test were used to measure the same construct (e.g. block recall and digit recall as measures of working memory), the measures were averaged, following Schmidt and Hunter's (2015) recommendation. Also, when the study presented a comparison between the treatment group and two control groups (passive and active), two effect sizes – one for each comparison with experimental and control groups – were calculated. As this procedure violates the principle of statistical independence of the samples, Cheung and Chan's (2004) method was applied to all the meta-analytic models. This method reduces the weight of dependent samples in the analysis by estimating an adjusted (i.e., smaller) *N* (for a list of the adjusted *N*s, see Tables 2.1 to 2.13 in the Supplemental material available online). Since the method

[5] When only the *t*-statistics were available, the *t*-values were converted into Cohen's *d*s (Lee, 2014; Witt, 2011).

of Cheung and Chan (2004) cannot be used for partially dependent samples,[6] we ran our analyses as if the comparisons between experimental samples and two different control groups were statistically independent. As shown by Bijmolt and Pieters (2001) and Tracz, Elmore, and Pohlmann (1992), the violation of statistical independence has little or no effect on means, standard deviations, and confidence intervals. Thus, the entire procedure is a reliable way to deal with the statistical dependence of part of the samples.

Results

Near-Transfer Effects

The random-effects meta-analytic overall effect size was $\bar{g} = 0.46$, 95% CI [0.35; 0.57], $k = 30$, $p < .001$. The forest plot is shown in Figure 2. The degree of heterogeneity between effect sizes was close to zero, $I^2 = 7.94$.[7]

[6] In addition, in three studies, a few participants did not take part in all the tests (i.e., attrition). In these cases, we used the mean number of participants as the number to be adjusted.

[7] The I^2 statistic refers to the percentage of between-study variance due to true heterogeneity and not to random error (Higgins, Thompson, Deeks, & Altman, 2003).

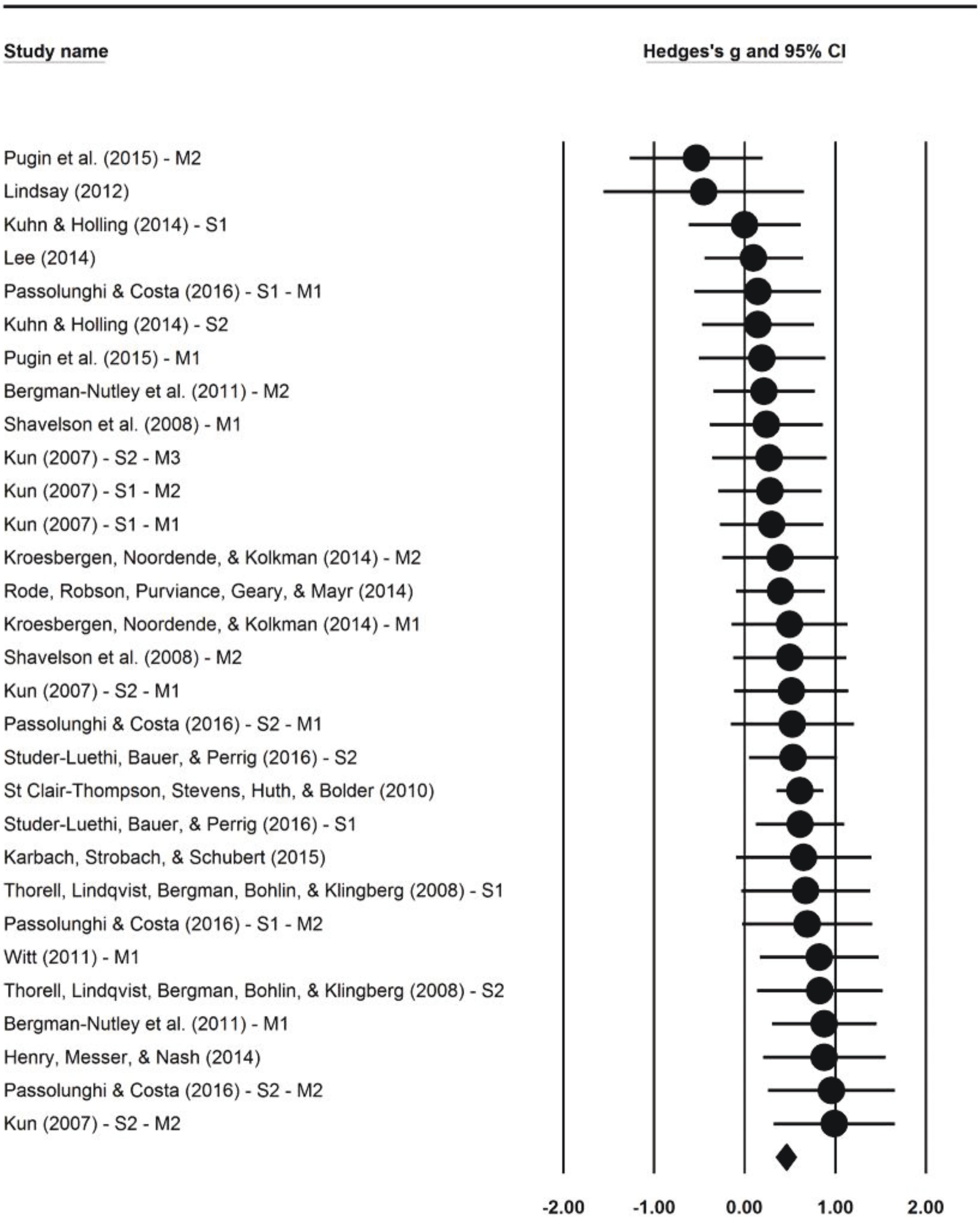

Figure 2. Forest plot of the near-transfer model. Hedges' *g*s (circles) and 95% CIs (lines) are shown for all the effects entered into the meta-analysis. The diamond at the bottom indicates the meta-analytically weighted mean $\bar{g}$. When studies had multiple samples, the table reports the result of each sample (S1, S2, etc.) separately. Similarly, when studies used multiple outcome measures, the table reports the result of each measure (M1, M2, etc.) separately.

Moderator analyses

Age was marginally significant, $Z(1) = –1.80$, $b = –0.03$, $p = .072$. None of the other three moderators were significant: Random allocation, $Z(1) = –0.58$, $b = –0.08$, $p = .562$; Type of control group, $Z(1) = –0.31$, $b = –0.04$, $p = .760$; and Duration of training, $Z(1) = 0.42$, $b = 0.01$, $p = .678$.

Publication bias analysis

To test whether our analysis was affected by publication bias, we examined a funnel plot representing the relation between effect sizes and standard errors. The contour-enhanced funnel plot (Peters, Sutton, Jones, Abrams, & Rushton, 2008) is shown in Figure 3.

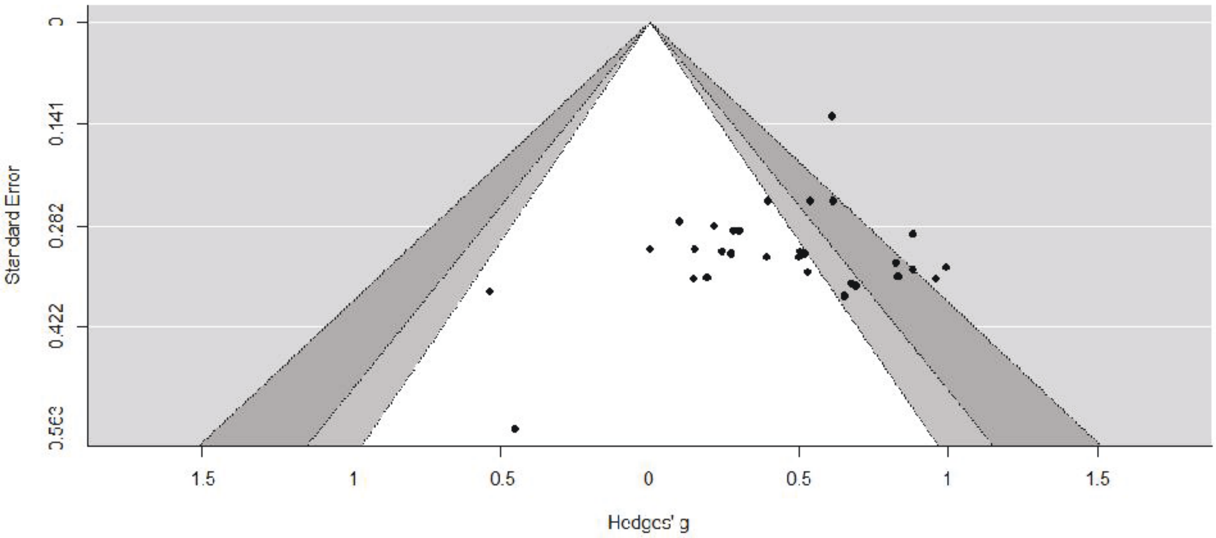

Figure 3. Contour-enhanced funnel plot of standard errors and effect sizes (Hedges' *g*s) in the near-transfer meta-analysis. The black circles represent the effect sizes included in the meta-analysis. Contour lines are at 1%, 5%, and 10% levels of statistical significance.

The symmetry of the funnel plot around the meta-analytic mean was tested by Egger's regression test (Egger, Smith, Schneider, & Minder, 1997). The test showed no evidence of publication bias ($p = .217$). In addition, the trim-and-fill analysis (Duval & Tweedie, 2000) estimated no weaker-than-average missing study (left of the mean). Finally, a *p*-curve analysis was run with all the *p*-values < .05 related to positive effect sizes (Simonsohn, Nelson, & Simmons, 2014). The results showed evidential values (i.e., no evidence of publication bias), $Z(9) = –3.39$, $p = .003$ (Figure 4).

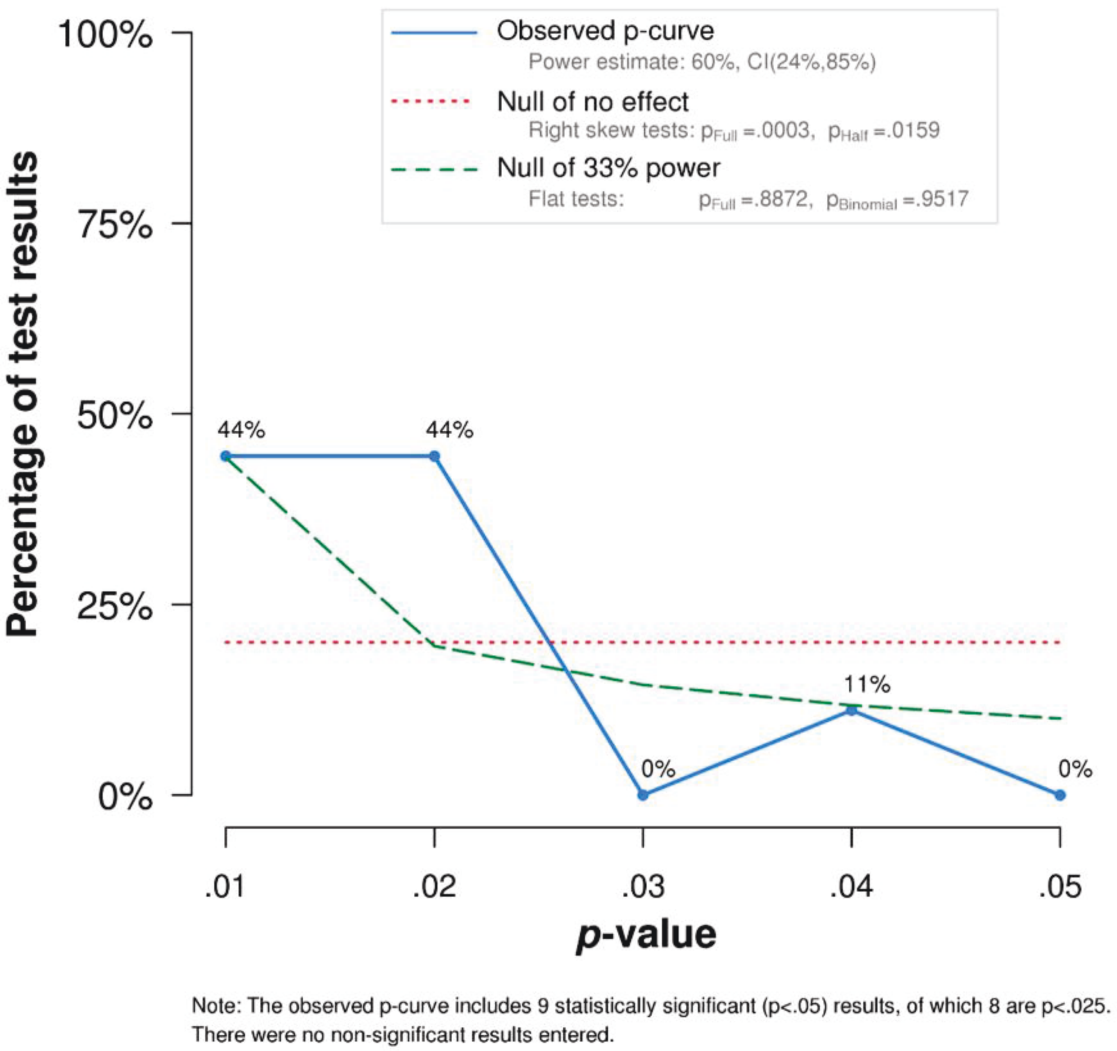

Figure 4. *p*-curve analysis. The blue line shows that most of the significant *p*-values are smaller than .025, suggesting evidential value.

Far-Transfer Effects

The random-effects meta-analytic overall effect size was $\bar{g} = 0.12$, 95% CI [0.06; 0.18], $k = 74$, $p < .001$. The forest plot is shown in Figure 5. The degree of heterogeneity between effect sizes was $I^2 = 0.00$.

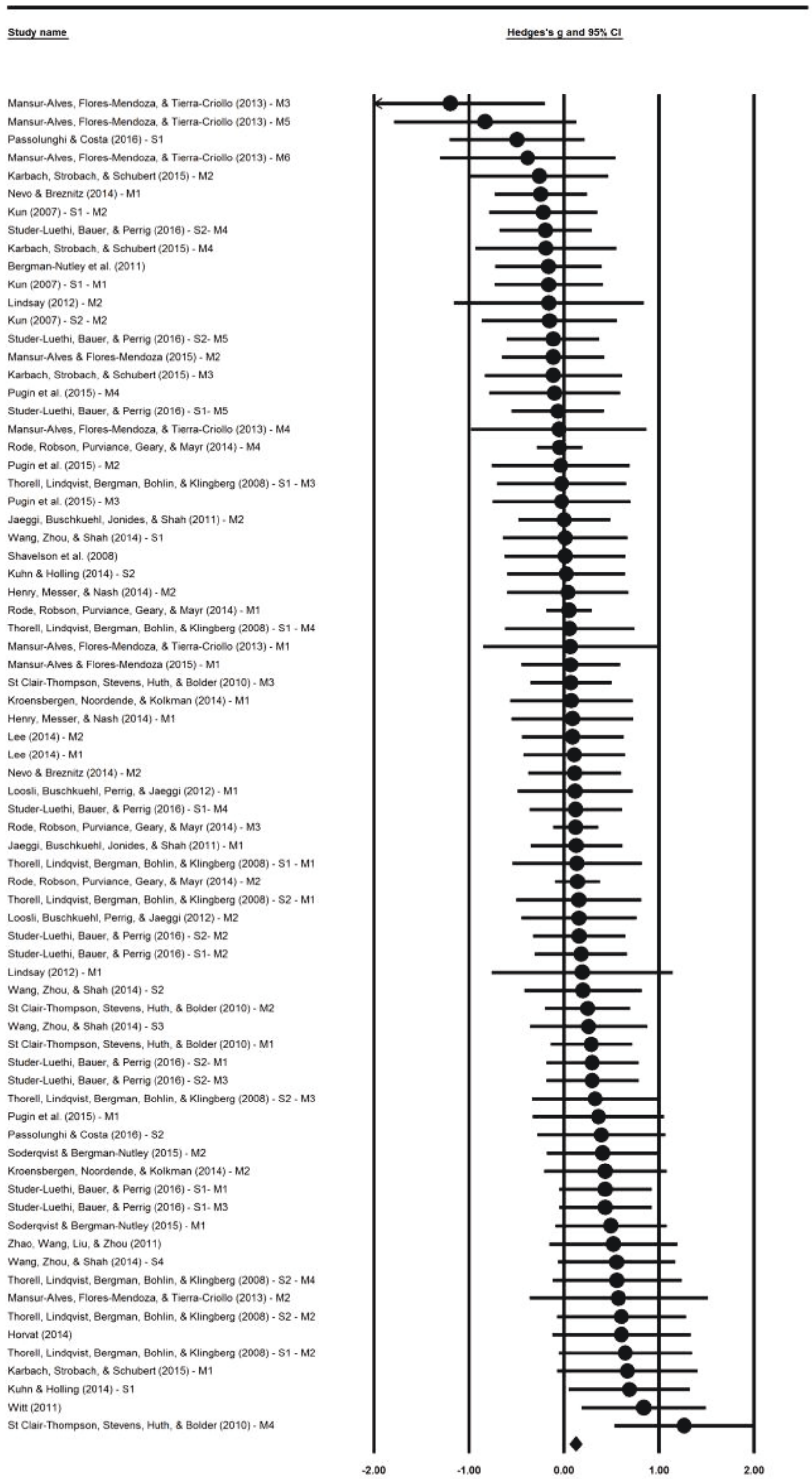

Figure 5. Forest plot of the far-transfer model. Hedges' *g*s (circles) and 95% CIs (lines) are shown for all the effects entered into the meta-analysis. The diamond at the bottom indicates the meta-analytically weighted mean $\bar{g}$. When studies had multiple samples, the table reports the result of each sample (S1, S2, etc.) separately. Similarly, when studies used multiple outcome measures, the table reports the result of each measure (M1, M2, etc.) separately.

Moderators analysis

Random Allocation was a significant moderator, $Z(1) = –2.76$, $b = –0.20$, $p = .006$. The overall effect sizes in randomized and non-randomized samples were $\bar{g} = 0.07$, 95% CI [0.00; 0.14], $k = 50$, $p = .046$, and $\bar{g} = 0.27$, 95% CI [0.15; 0.39], $k = 24$, $p < .001$, respectively. Type of control group was marginally significant, $Z(1) = –1.83$, $b = –0.12$, $p = .067$. The overall effect sizes when WM training was compared to active and passive control groups were $\bar{g} = 0.05$, 95% CI [–0.05; 0.15], $k = 40$, $p = .311$, and $\bar{g} = 0.18$, 95% CI [0.09; 0.26], $k = 34$, $p < .001$, respectively. Also, the overall effect size in randomized samples with active control groups was $\bar{g} = 0.03$, CI [–0.07; 0.14], $k = 34$, $p = .521$. Finally, Duration of training was marginally significant, $Z(1) = –1.81$, $b = –0.02$, $p = .070$. No other moderator was significant: Age, $Z(1) = –1.60$, $b = –0.03$, $p = .110$; and Domain, $p = .703$.

Additional meta-analytic models

We calculated the random-effects meta-analytic overall effect sizes of each of the six domains. The only significant overall effect size was $\bar{g} = 0.20$, 95% CI [0.03; 0.36], $k = 17$, $p = .018$, for mathematics. To test the robustness of the result, we ran two moderator analyses for this domain. Random Allocation was a significant moderator, $Z(1) = –2.01$, $b = –0.35$, $p = .045$. The overall effect sizes in randomized and non-randomized samples were $\bar{g} = 0.10$, 95% CI [–0.05; 0.25], $k = 12$, $p = .193$, and $\bar{g} = 0.49$, 95% CI [0.11; 0.88], $k = 5$, $p = .012$, respectively. Type of control group was significant, $Z(1) = –2.41$, $b = –0.43$, $p = .016$. The overall effect sizes when WM training was compared to active and passive control groups were $\bar{g} = –0.11$, 95% CI [–0.38; 0.16], $k = 6$, $p = .426$, and $\bar{g} = 0.31$, 95% CI [0.13; 0.49], $k = 11$, $p = .001$, respectively.

Literacy/WD overall effect size was marginally significant, $\bar{g} = 0.11$, 95% CI [–0.00; 0.22], $k = 17$, $p = .055$. None of the other overall effect sizes was significant: $\bar{g} = 0.11$, 95% CI [–0.02; 0.24], $k = 21$, $p = .101$ for fluid intelligence; $\bar{g} = 0.09$, 95% CI [–0.08; 0.26], $k = 14$, $p = .302$ for cognitive control; $\bar{g}$ = – 0.02, 95% CI [–0.75; 0.71], k = 3, p = .956 for crystallized intelligence; and $\bar{g}$ = –0.20, 95% CI [–0.65; 0.25], k = 2, p = .386 for science.

Publication bias analysis

The contour-enhanced funnel plot of the main model (k = 74) is shown in Figure 6.

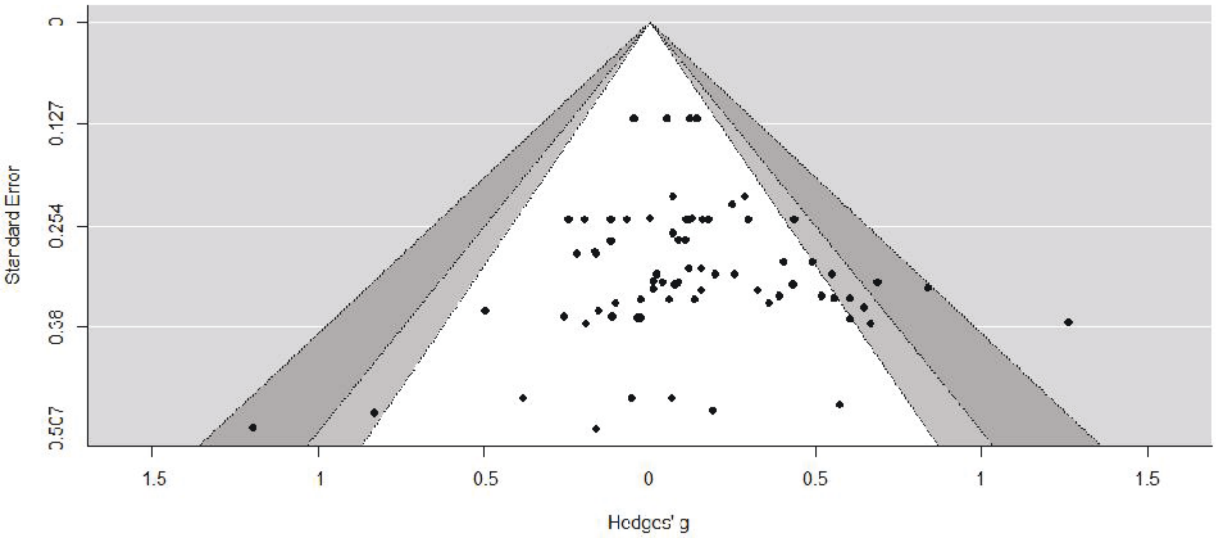

Figure 6. Contour-enhanced funnel plot of standard errors and effect sizes (*g*s) in the far-transfer meta-analysis. Contour lines are at 1%, 5%, and 10% levels of statistical significance.

Egger's regression test showed no evidence of publication bias (p = .511). In addition, the trim-and-fill analysis estimated no weaker-than-average missing studies (left of the mean). Finally, we performed a p-curve analysis. Both the full and half p-curve tests were right skewed with $p < .100$ ($Z(3) = –1.40$, p = .081 and $Z(3) = –1.38$, p = .084, respectively) suggesting evidential value (Simonsohn, Simmons, & Nelson, 2015; Figure 7).[8]

[8] Since only three values were inputted, the results of this p-curve analysis might be unreliable. However, it must be kept in mind that the occurrence of publication bias is quite unlikely when the overall effect size is close to zero.

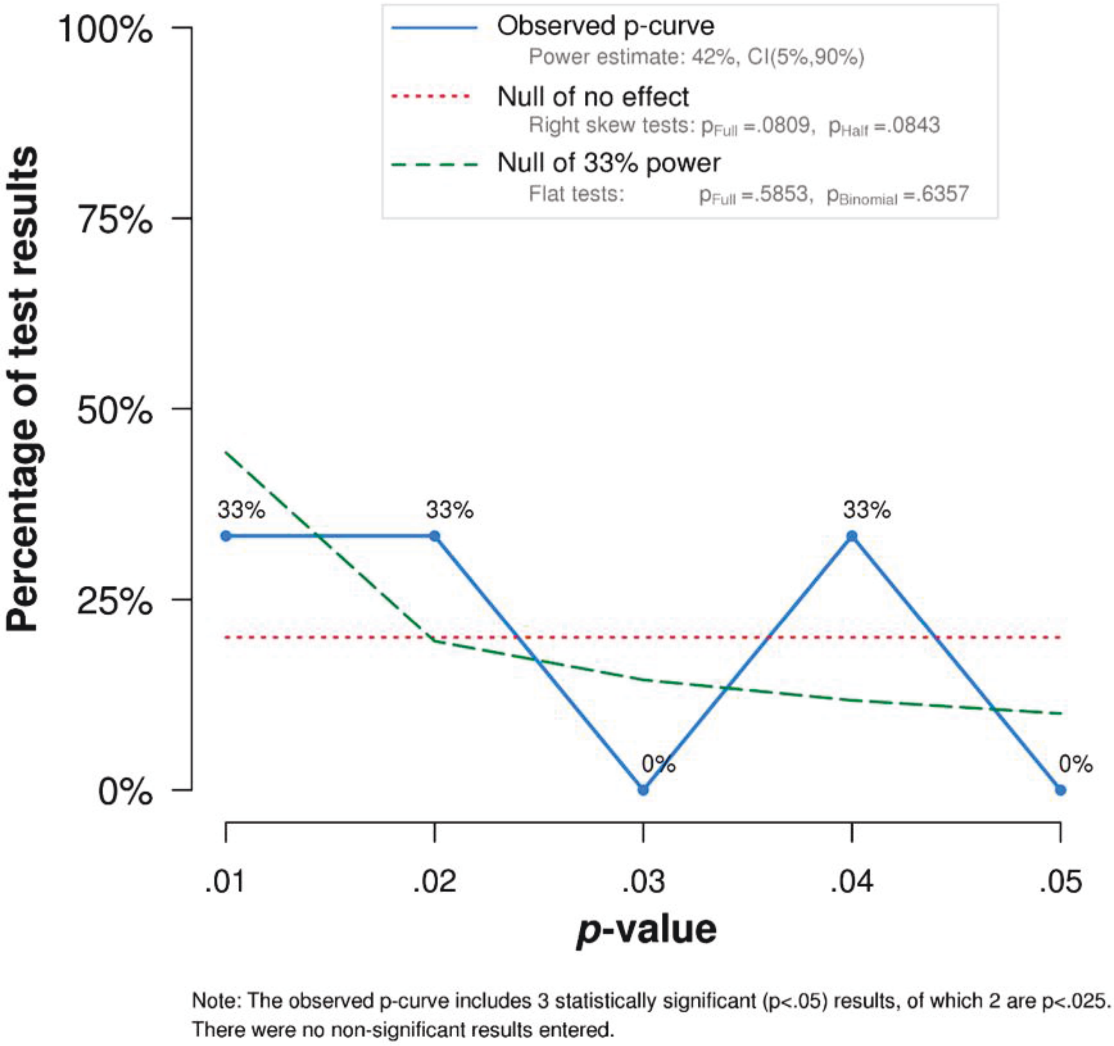

Figure 7. *p*-curve analysis. The blue line shows that most of the significant *p*-values are smaller than .025, suggesting evidential value.

A trim-and-fill analysis was performed for four additional meta-analytic models, (fluid intelligence, cognitive control, mathematics, and literacy/WD models). In the fluid intelligence model, five studies were filled in, and the point estimate was $\bar{g}$ = 0.03, 95% CI [–0.09; 0.15]. In the literacy/word decoding model, two studies were filled in, and the point estimate was $\bar{g}$ = 0.08, 95% CI [–0.03; 0.19]. No missing study was found in the other two models. Due to the scarcity of effect sizes, no publication bias analysis was run for the science and crystallized intelligence models.

Follow-Up Effects

For near-transfer follow-up effects, the random-effects meta-analytic overall effect size was $\bar{g}$ = 0.33, 95% CI [0.00; 0.65], k = 6, p = .049. The degree of heterogeneity between effect sizes was I^2 = 40.50.

For far-transfer follow-up effects, the random-effects meta-analytic overall effect size was $\bar{g}$ = 0.09, 95% CI [–0.02; 0.20], k = 24, p = .122. The degree of heterogeneity between effect sizes was I^2 = 0.00.

Moderator analyses

Due to the small number of effect sizes, no moderator analysis was run for the near-transfer effects model. (For the same reason, no publication bias analysis was carried out for this model.) Regarding the far-transfer effects model, no moderator was significant.

Publication bias analysis

In the far-transfer effect model, Egger's regression test showed no evidence of publication bias (p = .345). In addition, the trim-and-fill analysis estimated no weaker-than-average missing studies (left of the mean). No p-curve analysis was carried out because none of the effect sizes in the model reached statistical significance.

Discussion

The purpose of this meta-analysis was to evaluate the impact of WM training on TD children's cognitive and academic skills. The results showed a clear pattern. Similar to previous meta-analyses (e.g., Melby-Lervag & Hulme, 2013; Schwaighofer et al., 2015), WM training significantly affected WM-related skills (post-test overall effect size, $\bar{g}$ = 0.46, $p < .001$) and remained several months after the end of training (follow-up overall effect size, $\bar{g}$ = 0.33, p = .049). However, we found little or no evidence that WM training enhances fluid intelligence or domain-general processes such as cognitive control. The same applied to academic abilities such as literacy or science. Only the mathematics-related overall effect size was significant, albeit quite modest ($\bar{g}$ = 0.20, p = .018). However, methodological issues cast some doubts on the authenticity of the effect (we will take up this point below). Thus, the results of the meta-analysis do not support the hypothesis according to which WM training benefits cognitive or academic abilities in TD children.

Interestingly, WM training seems to produce approximately the same negligible effects on measures outside the domain of WM regardless of the age of participants and domain. The significant (or marginally significant) moderators in the far-transfer main model (k = 74) were the random allocation of the participants to the samples, the type of control group, and duration of training. The overall effect size was much smaller in randomized samples ($\bar{g}$ = 0.07, p = .046) than in non-randomized samples ($\bar{g}$ = 0.27, p < .001). This outcome suggests that episodes of self-selection in the experimental groups or differences at baseline level between experimental and control groups may have inflated the effect sizes in samples with no random allocation.[9] Analogously, the overall effect size was smaller when the experimental group was compared to an active control group ($\bar{g}$ = 0.05, p = .311) than a passive control group ($\bar{g}$ = 0.18, p < .001). This finding corroborates the idea that the positive effect sizes reported in some primary studies are due to placebos as well. Moreover, when only the effect sizes in randomized samples with active control groups were considered, the overall effect size was almost null ($\bar{g}$ = 0.03, p = .521). Finally, the duration of training seems to be slightly inversely related to the size of the effects (b = –0.02). This result is difficult to interpret. However, the null degree of heterogeneity suggests caution in

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[9] In the present case, the difference between groups at baseline level in some of the dependent variables seems to be the most likely explanation. In several studies (e.g., Thorell, Lindqvist, Bergman, Bohlin, & Klingberg, 2008), the control groups performed better than the experimental groups at the pre-test. The difference between the groups decreased at the post-test, suggesting that the positive effect size is probably due to some statistical artefact (e.g., regression to the mean, ceiling effect).

interpreting these outcomes. In fact, the moderator analyses may have detected effects due to random error rather than true heterogeneity between-effect sizes (see footnote 7). In any case, far transfer effects of WM training appear to be negligible or, at best, modest.

Theoretical and Practical Implications

The present meta-analysis reviewed the studies in which participants were TD children. For this reason, the results we reported do not apply to other populations – such as children with learning disabilities or adults. Nonetheless, the fact that, in the general population of children, WM training induces improvements in WM-related outcomes but not in other types of cognitive and academic measures suggests some theoretical and practical implications.

To begin with, if far-transfer is more likely to occur in children than adults when cognitive and academic skills are developing, then our findings cast serious doubts on the idea that training a domain-general mechanism such as WM improves fluid intelligence, cognitive control, or academic achievement.[10] Second, and linked to the first point, the lack of an effect of WM training on fluid intelligence supports the idea that WM and fluid intelligence are two different constructs (Ackerman, Beier, & Boyle, 2005; Hornung, Brunner, Reuter, & Martin, 2011; Kane, Hambrick, & Conway, 2005).

However, it must be noticed that the positive effects in near-transfer measures might reflect an improvement in WM tasks performance, rather than a genuine enhancement in WM capacity (Shipstead et al., 2012). In other words, participants learn how to do the task without improving their WM capacity. If this is the case, nothing can be inferred about the relationship between fluid intelligence (or any other far-transfer measure) and WM capacity. Moreover, following this line of

[10] It must be noticed that this argument does not apply to the population of older adults. In fact, the aim of WM training in the elderly is to slow down cognitive decline, not to extend developing cognitive abilities. For a review, see Karbach and Verhaeghen (2014).

reasoning, the absence of fluid intelligence enhancement could be interpreted as a failed improvement in WM capacity after the training (see also the discussion in Melby-Lervag & Hulme, 2013). Regrettably, the information provided in the primary studies is not sufficient to solve the issue.

The fact that the participants showed improvements in a large variety of tasks different from the WM trained tasks (see Table S1.1 in the Supplemental Material available online) might suggest that WM capacity was actually boosted. However, pervasive improvement in WM-related measures may stem from amelioration in some general skill at performing WM tasks rather than an increased WM capacity. Thus, testing whether WM training enhances WM capacity requires not only a set of multivariate measures of WM capacity, but also that task-related improvements occur through a common factor that is measurement invariant across treatment and control groups (i.e., training effects that are proportional to the factor loadings in a structural equation model). If such conditions can be met in a well-powered single study, then it can be convincingly claimed that WM capacity has been enhanced.

Beyond these theoretical aspects, the most obvious practical implication of our results is that WM training, at the moment, cannot be recommended as an educational tool. WM training seems to have little or no effect on far-transfer measures of cognitive abilities and academic achievement. More generally, this meta-analysis provides further evidence that the occurrence of far-transfer is too infrequent to offer solid educational advantages. For this reason, cognitive and academic enhancement interventions should be as close as possible to the skills that are meant to be trained.

Limitations of the Present Meta-Analysis

Near-transfer effects seem to remain even a few months after the end of the training. However, the limited number of studies (n = 4) and effect sizes (k = 6) does not allow to draw any reliable conclusion about this. The same limitation applies, to a lesser degree, to the far-transfer follow-up effects (n = 6, k = 24). In this case, however, the findings are consistent with the immediate post-test outcomes: modest or null effects in both the measures. In fact, it is hard to see why negligible effects immediately after training, such as those reported in this meta-analysis, should become significantly larger several months after the end of training.

Finally, other potential moderators – such as the type of training program – were not considered in the meta-analytic models because of the limited number of the effect sizes. However, the small degree of heterogeneity in both the near- and far-transfer models discourages us from thinking that other moderators could have affected the overall results.

Conclusions

The findings of the present meta-analysis do not invite optimism about the effectiveness of WM training at improving cognitive skills and academic achievement in TD children. WM training seems to enhance children's performance in WM- and STM-related measures. However, with regard to skills outside the domain of WM such as fluid intelligence, cognitive control, mathematics, and literacy, this training seems to have little or no effect. Consistent with Thorndike and Woodworth's (1901) common element theory, our findings show that the occurrence of far-transfer is, at best, sporadic.

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