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An Empirical Comparison of the Therapeutic Benefits of Physical Exercise and Cognitive Training on the Executive Functions of Older Adults: A Meta-Analysis of Controlled Trials

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A robust body of aging-related research has established benefits of both physical exercise (PE) and cognitive training (CT) on executive functions related to the activities of daily living of older adults; however, no meta-analysis has compared these treatments. ***Objective:*** The current quantitative review involved a comparison of the overall effect sizes of PE and CT interventions on executive functions (Morris, 2008; pre-post-controlled effect size: d_{ppc}), while also exploring contextual moderators of treatment outcomes. ***Method:*** A systematic review identified 46 studies (23 PE, 21 CT, and 2 both) meeting inclusion criteria (i.e., controlled interventions, executive-related outcomes, mean ages 65+, information to calculate d_{ppc}). ***Results:*** The weighted mean d_{ppc} values came to 0.12 ($p < .01$) for PE and 0.24 ($p < .01$) for CT. Treatment effects differed based on executive constructs for CT, with problem solving presenting the highest d_{ppc} (0.47, $p < .01$). Notably, PE produced similar effect sizes across distinct executive functions. Treatment characteristics (e.g., session length/frequency) did not predict effect sizes. CT had a significant benefit on healthy participants (0.26, $p < .01$), but cognitively impaired samples did not experience a significant effect. ***Conclusions:*** Both treatments improved executive functions, but CT presented a potential advantage at improving executive functions. Improvements in executive functions differed depending on construct for CT, whereas each construct produced similar, modest effect sizes for PE. Publication bias and study quality variability potentially bias these conclusions, as lower quality studies likely produced inflated effect sizes.

Keywords: physical exercise, cognitive training, executive function, aging, meta-analysis

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A growing body of research has demonstrated the vulnerability of executive functions to age-related changes in brain structures and functioning (e.g., Di, Rypma, & Biswal, 2014; Gunning-Dixon & Raz, 2003; Turner & Spreng, 2012). Specifically, higher-order cognitive functions seem to decline during healthy aging (Salthouse, Atkinson, & Berish, 2003) and appear highly susceptible to the cortical and subcortical degeneration observed in dementia (Vemuri et al., 2010). Some have labeled declines in executive control an important underlying issue in cognitive aging (Braver & Barch, 2002); however, this issue may have an array of solutions, as executive functions remain particularly malleable abilities into later age (Colcombe et al., 2004; Kramer & Erickson, 2007; Kramer et al., 1999).

Although involved in normative age-related declines (Burke & Barnes, 2006), executive functions arise from brain areas presenting high levels of neuroplasticity (e.g., prefrontal cortex; Miller & Cohen, 2001). Researchers have examined potential interventions that impact brain function in executive networks related to cognitive control, concluding that these networks show improved functional plasticity in response to cognitive and fitness-based experiences (Mozolic, Hayasaka, & Laurienti, 2010; Voss et al., 2010). These findings suggest that behavioral interventions have the potential to counterbalance the neurological changes associated with the executive deficits of the aging process (Park & Reuter-Lorenz, 2009). With functional changes in the brain occurring at later age (Greenwood & Parasuraman, 2010), improvement in executive functions and their respective neural networks may translate into maintained autonomy and improved quality of life among older adults.

Along these lines, growing evidence supports the relationship between executive functions and activities of daily living at later age and in dementia, a connection emphasizing the ecological importance of executive functions for the everyday lives of older adults (Boyle et al., 2003; Cahn-Weiner, Boyle, & Malloy, 2002; Grigsby, Kaye, Baxter, Shetterly, & Hamman, 1998; Johnson, Lui, & Yaffe, 2007; Martyr & Clare, 2012; Pereira, Yassuda, Oliveira, & Forlenza, 2008). As researchers clarify the importance of exec-

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utive functions in cognitive aging (Braver & Barch, 2002; Gunning-Dixon & Raz, 2003), various treatments have arisen to ameliorate higher-order cognitive declines, ranging from pharmacological to lifestyle-based therapies.

In parallel with advances in drug therapies for cognitive declines (Herrmann, Chau, Kircanski, & Lanctot, 2011), alternative lifestyle interventions (i.e., physical exercise, cognitive training) have become increasingly popular considering the growing evidence for their efficacy at improving executive functions and activities of daily living (Colcombe & Kramer, 2003; Kramer et al., 1999; Sitzer, Twamley, & Jeste, 2006). These interventions may offer cost-effective and person-centered alternatives to the pharmacological treatments still in development. In a meta-analysis on fitness-based interventions for cognition, Colcombe and Kramer (2003) summarized the neuropsychological benefits of physical exercise interventions for older adults and elucidated their value in modifying executive functions. Along these same lines, researchers have also examined cognitive training interventions for older adults, presenting marked benefits for the same higher-order constructs (Sitzer et al., 2006). These cognitive training strategies incorporate restorative and compensatory programs targeting specific cognitive abilities (i.e., problem solving, memory, etc.). Research on both styles of interventions has burgeoned within the last decade, resulting in many new studies examining the impact of both treatments on executive functions.

Overall a large body of research has confidently established the cognitive benefits of both behavioral treatments independently. Many published reviews have covered the cognitive effects of physical exercise (e.g., Colcombe & Kramer, 2003; Smith et al., 2010; van Uffelen, Chin A Paw, Hopman-Rock, & van Mechelen, 2008) and cognitive training (e.g., Clare, Woods, Moniz Cook, Orrell, & Spector, 2003; Gates & Valenzuela, 2010; Kurz, Leucht, & Lautenschlager, 2011; Sitzer et al., 2006; Woods, Aguirre, Spector, & Orrell, 2012). The current meta-analysis diverges from these former studies in two fundamental ways: first, through its comparative element, as it aims to distinguish the effects of physical exercise interventions from those of cognitive training; and second through its sole focus on research reports with executive-related outcomes, narrowing the survey of the literature to studies evaluating the effects of treatment on a neuropsychological construct highly sensitive to cognitive decline (i.e., executive functions; Braver & Barch, 2002; Gunning-Dixon & Raz, 2003). Although considered elusive, a large body of research has identified diverse executive abilities (Jurado & Roselli, 2007; Miyake et al., 2000), including five executive constructs included as outcome measures in the current meta-analysis: working memory, inhibition, executive attention, problem solving, and fluency.

These five constructs derive from a rich history of neuropsychological research detailing the diversity of executive-related abilities. The first construct, working memory, represents the ability to hold information in mind and update it moment to moment (Goldman-Rakic, 1996), playing a critical role in guiding everyday behaviors and serving as an interface between perception, attention, memory, and action (Baddeley, 1996). The second construct, inhibition, involves the ability to suppress or ignore irrelevant information currently interfering with an immediate goal (Nigg, 2000). The third construct, executive attention, was proposed originally by Posner and Rothbart (2007). This higher-order cognitive ability involves the overriding of automatic attention processes and the redirecting of behavior in accordance with goals, rather than the saliency of the stimulus (e.g., shifting attentional resources, Miyake et al., 2000). Two more executive constructs, problem solving and fluency, have been more broadly defined, as they may be representative of second-order executive abilities. Problem solving refers to the ability to reason through a nonroutine, novel event, involving several steps including problem representation (Zelazo & Müller, 2011), problem identification (Lezak, Howieson, Bigler, & Tranel, 2012), goal/task analysis (Borkowski & Burke, 1996), and initiation and planning (Anderson, 2002). Lastly, the final construct, fluency, involves the flexible production of a series of novel responses within a given set or category, while inhibiting those responses previously provided. Categories range from phonological sets (e.g., words beginning with "F") to semantic sets (e.g., types of furniture). Tests also vary in response modalities, from verbal fluency with oral responses to design fluency with graphomotor responses (Lezak et al., 2012). Altogether, these five constructs represent executive-related abilities commonly measured in both clinical and experimental neuropsychological research, with each frequently assessed as outcomes in physical exercise and cognitive training trials.

Evaluating the impact of physical and cognitive interventions on these five executive abilities, the current meta-analysis aimed to achieve three objectives: (a) to compare the established benefits of physical exercise and cognitive training to determine if either intervention presents an advantage at improving executive functions; (b) to identify participant characteristics (e.g., age, sex, cognitive impairment, etc.) that predict the efficacy of each intervention at improving executive functions; and (c) to determine which treatment characteristics (e.g., duration, session frequency, etc.) produce the best outcomes for higher-order cognitive abilities.

Method

The report of this meta-analysis closely followed the guidelines advocated by Moher, Liberati, Tetzlaff, Altman, and the PRISMA Group (2009), following the item reporting checklist provided within their Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) Statement.

Literature Search

The systematic review occurred in June 2013, involving online searches of the PsycInfo, MedLine, CINAHL, PsycArticles, and Cochrane Central Register of Controlled Trials databases. Pertaining to search filters, results were limited to English publications with clinical trial, controlled clinical trial, or randomized controlled trial methodologies involving human participants aged 65 years and older. Following the search terms of similar systematic reviews, the first search involved keywords related to exercise and cognition among older adults (Colcombe & Kramer, 2003; Heyn, Abreu, & Ottenbacher, 2004), and the second search used keywords focused on cognitive training among older adults (Sitzer, Twamley, & Jeste, 2006; Valenzuela & Sachdev, 2009). Additional terms related to executive functioning and clinical trial methodologies were included with each search. Reference lists from peer-reviewed articles were also examined to identify other pertinent studies (see Figure 1 for a summary of search terms and a flowchart of the systematic search method).

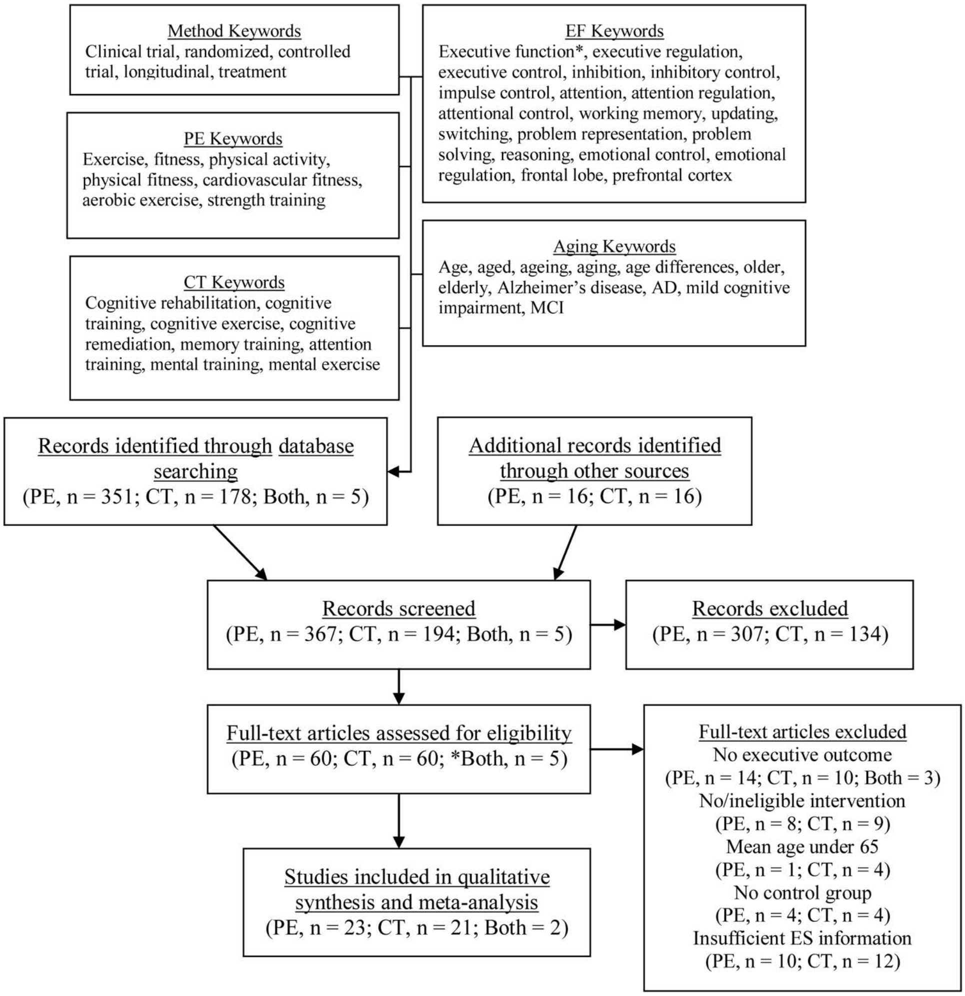

Figure 1. Flowchart of systematic review. EBSCOhost automatically removes duplicates from search results. PE = physical exercise; CT = cognitive training; EF = executive functioning; ES = effect size. * Five studies involving both PE and CT treatments were identified, with two meeting inclusion criteria.

Prior to the literature search, specific inclusion criteria were established by the authors. To be included in the study, articles needed to (a) have an executive function (or a related construct) as an outcome measure, (b) involve an intervention (either physical or cognitive) with baseline and immediate posttreatment assessments (i.e., posttest, not follow-up), (c) involve a mean age over 65 for both experimental and control groups, (d) involve either a healthy sample or a sample experiencing dementia or mild cognitive impairment, (e) include a waitlist or placebo control condition, and (f) report enough information to calculate an effect size for at least one executive-related outcome measure.

The physical exercise keyword search yielded 351 results. A manual search from reference lists and other known studies of the field resulted in an additional 16 articles for more extensive review. Abstracts were reviewed for all studies, and 60 of these studies were selected for more extensive review. Altogether, 23 articles on exercise and executive functions ultimately met inclusion criteria and were included in the meta-analysis. Notably, Madden, Blumenthal, Allen, and Emery (1989) used a subsample of participants from Blumenthal, Emery, Madden, and George (1989) and Liu-Ambrose, Nagamatsu, Voss, Khan, and Handy (2012) used a subsample of participants from Liu-Ambrose et al. (2010). With the cognitive training keywords, the search yielded 178 articles. Manual search from reference lists and other known studies resulted in an additional 16 articles for more extensive review. After the abstracts of these results were screened, 60 of these articles were more extensively reviewed. The equal number of retrieved articles across literature searches occurred entirely by coincidence, with no predetermined quota of articles decided beforehand. After further review, 21 articles on cognitive training and executive functions (with two of these articles representing a single study; Craik et al., 2007; Stuss et al., 2007) met inclusion criteria and were included in the meta-analysis. An additional five articles involved both physical exercise and cognitive training (either separate or combined), of which two ultimately met inclusion criteria. Appendix A, included in the online supplemental materials for this article, lists the excluded studies on both physical exercise and cognitive training interventions organized by their rationale for exclusion. The following list reports the number of studies excluded across each training modality (i.e., number of excluded physical exercise studies, number of excluded cognitive training studies) based on the aforementioned inclusion criteria: ineligible mean age (1, 4), insufficient information for effect size calculation (10, 12), ineligible intervention or no intervention (8, 9), no control condition (4, 4), and no executive function outcome (13, 10).

Data Extraction

The review was performed systematically using a modified data collection instrument (Zaza et al., 2000) and the PEDro scale for rating controlled trial study quality (Maher, Sherrington, Herbert, Moseley, & Elkins, 2003). The PEDro scale quantifies study quality through 11 items, attributing one point for each item satisfied by the rated study, except for the first item (range: 0–10). The items assess design attributes characteristic of a rigorous clinical trial, including randomization, blinding, dropout rates, and other methodological considerations. Each eligible article was read independently by two reviewers with extracted data compared to ensure interrater reliability. Reliability analyses indicated 93.42% congruence between summary reports across reviewers. Discussion over discrepancies between reviewers ultimately yielded 100% consensus regarding the extracted data.

Study Variable Coding

For each study a series of variables were recorded for inclusion in analysis, including year of publication, study quality, sample size (divided by experimental and control groups), average age, percent male, baseline Mini-Mental State Examination (MMSE) score, cognitive status (i.e., healthy vs. impaired), control condition (i.e., active vs. waitlist), and treatment style (i.e., physical exercise vs. cognitive training, individual vs. group, treatment length, frequency of sessions, session duration). For cognitive status, any degree of cognitive decline qualified as impairment, resulting in a heterogeneous group including multiple diagnoses, such as dementia and mild cognitive impairment. In addition, physical treatment style (i.e., strength, aerobic, combined) was recorded for exercise trials.

Effect Size Calculation

Treatment effect sizes. Considering the pretest-posttest-controlled (PPC) designs customary of the clinical trials included in this meta-analysis, effect size calculations followed the recommended formula of Morris (2008), with each effect size calculated from the mean treatment group change minus the mean control group change divided by the pooled baseline standard deviation of both the treatment and control groups (hereafter referred to as d_{ppc}):

$$d_{ppc} = c_p\left[\frac{(M_{post,T} - M_{pre,T}) - (M_{post,C} - M_{pre,C})}{SD_{pre}}\right]$$

With the pooled pretest standard deviation calculated as

$$SD_{pre} = \sqrt{\frac{(n_T - 1)SD^2_{pre,T} + (n_C - 1)SD^2_{pre,C}}{n_T + n_C - 2}}$$

Where n_T and n_C are the sample sizes of the treatment and control groups and the small sample size bias-correction is calculated as

$$C_P = 1 - \frac{3}{4(n_T + n_C - 2) - 1}.$$

For studies reporting more than one control condition (Clare et al., 2010; Powell, 1974; Quayhagen et al., 1995), the data for the control with the most limited activity (i.e., waitlist if available) was selected for computing the effect size. For studies with two experimental groups (Brown, Liu-Ambrose, Tate, & Lord, 2009; Buiza et al., 2008; Margrett & Willis, 2006; Moul et al., 1995; Oken et al., 2006; Liu-Ambrose et al., 2010, 2012; Scherder et al., 2005), the same control group was used in the calculation for two separate effect sizes. Two studies (Klusmann et al., 2010; Legault et al., 2011) included both cognitive and physical training conditions, but one control group, with the prepost data for this nontreatment condition used in the calculation of effect sizes for both intervention styles. As well, one study (Blumenthal, Emery, Madden, & George, 1989) divided effects by gender and the separate subgroup means and variances were averaged prior to the calculation of effect sizes.

Random effects model. Aligning with a study-effect meta-analysis methodology (Bangert-Drowns, 1986), effect sizes for each study were averaged into one effect size estimate per study to limit effect size interdependency and prevent a study reporting many executive-related effect sizes from biasing any findings. In turn, all studies were equally represented in each analysis, with one average effect size for each study regardless of how many executive-related outcomes the study reported. Although the Rosenthal and Rubin (1986) method for combining effects was preferred, a simple mean effect size was used as the next best alternative (Marín-Martínez & Sánchez-Meca, 1999), as the former was not designed for merging Morris (2008) PPC effect sizes and most published research did not report the information needed to use this methodology. In the random effects model, effect sizes across studies are assumed to share a common distribution with variance, τ^2, as well as possessing a unique error, σ_i^2, at the study level. In the analyses, effect sizes are weighted according to their total variance $\sigma_i^2 + \tau^2$ so that the influence of an effect size is proportional to the level of certainty as to its true value. Unfortunately, accurate estimation of the variance σ^2 of the d_{ppc} effect size requires the correlation between pre- and posttest scores, which was not available in the overwhelming majority of studies included in the analysis. As an approximation, we estimate the variance of the standardized mean difference, according to

$$s_i^2 = \frac{n_T + n_C}{n_T n_C} + \frac{d^2_{ppc_i}}{2(n_T + n_C)}$$

Where n_T and n_C are the sample sizes of the treatment and control groups (Cooper, Hedges, & Valentine, 2009). Estimation of the between study variance, τ^2, proceeds as follows. First, each effect size is weighted by the inverse variance

$$w_i = \frac{1}{s_i^2}$$

And τ^2 is then estimated according to DerSimonian and Laird (1986)

$$\tau^2 = \frac{Q_w - (k-1)}{\sum_i w_i - \sum_i w_i^2 / \sum_i w_i}$$

$$Q_w = \sum_i w_i (d_{ppc_i} - \bar{d}_{ppc})^2$$

Where k is the total number of effect sizes in the meta-analysis and $\bar{d}_{ppc}$ is the weighted mean effect size. The random effects weights are then given by

$$w_i^{RE} = \frac{1}{s_i^2 + \tau^2}.$$

Statistical Analyses

To confirm the heterogeneity of effect sizes for the random effects model, analyses were preceded by a Cochran's Q test and an I^2 index calculation for all effect sizes (Conover, 1999; Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006). A significant result indicated differences among treatment effects, as hypothesized by the a priori distinctions in treatment types.

As the systematic review gathered only published research articles, the file drawer problem remains a likely bias affecting the results of this meta-analysis (Rosenthal, 1979); however, in response to publication bias in the literature search, a fail-safe *N* was calculated to determine the number of hypothetical null results needed to reduce the significance of the overall effect size to a null result (Rosenberg, 2005).

Weighted means and correlations. Hypothesizing significant between-study heterogeneity, follow-up analyses involved weighted *t* test comparisons for categorical moderators and metaregression models for continuous moderators. All weighted means and correlations for moderator variables were calculated separately for physical and cognitive training effect sizes, with results identifying how the extracted variables impact outcomes for each treatment style. A first-extracted variable set involved categorical comparisons (i.e., physical exercise vs. cognitive training, healthy vs. impaired samples, individual vs. group therapy, active vs. waitlist control condition). Each category was also statistically compared to zero, evaluating whether the effect estimate for each category statistically differed from a null effect.

Executive construct also served as a categorical moderator variable, with effect size estimates for each construct compared with zero as well. Effect sizes for each study were averaged based on their latent constructs (i.e., working memory, inhibition, executive attention, problem solving, and fluency). The measurements of two executive-related outcomes were too global to be divided into the defined constructs (i.e., Frontal Assessment Battery, Nouchi et al., 2012; NexAde executive function, Peretz et al., 2011). In turn, these effect sizes were included in the overall treatment estimates and moderator analyses, but not the comparisons of executive constructs. The overall effect size for each construct involved its own τ^2 value in calculating the weights to avoid the biasing of the between-study variance estimate by studies reporting multiple constructs. Appendix B, included in the online supplemental materials, lists the tests assigned to each construct by study.

Metaregression models evaluated continuous moderators as predictors of treatment effects, with identical models predicting physical exercise and cognitive training effect sizes separately. For each treatment modality, two metaregression models examined sets of continuous moderator variables. The first model included participant features (i.e., percent male and average age), and the second model evaluated treatment characteristics (i.e., length in weeks, session duration in hours, frequency in sessions per week). A third model including study features (i.e., year of publication and study quality; Maher et al., 2003) presented poor fit. As well, too few studies reported MMSE values to include it in a model without substantial listwise deletion. In turn, the weighted correlations between all continuous moderators and effect sizes were independently compared to zero for both treatment modalities. For physical exercise effects only, an exercise style variable (e.g., aerobic, strength, and combined) was extracted; however, dividing the physical exercise sample three ways for categorical comparisons significantly reduced the sample size of effects for each group. In turn, these variables are reported qualitatively, but did not guide any analysis or interpretation. Due to the number of statistical comparisons, the threshold for significance was set at $p \leq .01$ to reduce the Type I error rate.

Results

Systematic Review

Physical exercise. Altogether, 25 eligible studies involved physical exercise interventions (i.e., 23 with only exercise and two with separate cognitive and physical treatments). These studies reported 134 effect sizes. The averaging of effect sizes within each study resulted in 32 effect sizes, with five studies involving two treatment conditions and a separate effect size calculated for each treatment condition. Across this sample of studies, 958 participants completed an experimental condition and 920 participants completed a control condition. The participants had an average age of 74.0 (s = 5.66; 30.4% male) and an average MMSE of 27.1 (s = 2.19). Table 1 displays the effect sizes and moderator variables for all 25 physical exercise interventions included in the current meta-analysis.

Cognitive training. Across the 23 eligible studies involving cognitive training interventions (i.e., 21 with solely cognitive training and two with separate cognitive and physical training treatments), 89 effect sizes were calculated. The averaging of effect sizes within each study resulted in 26 effect sizes, with three studies involving two treatment conditions and a separate effect size calculated for each treatment condition. Altogether these interventions involved 1,158 participants in experimental conditions and 1,088 participants in control conditions. The combined participant sample presented an average age of 73.3 (s = 4.31; 40.9% male) and an average MMSE of 26.0 (s = 3.92). Table 2 reports the effect sizes and moderator variables for all 23 cognitive training interventions included in the current meta-analysis.

Effect Sizes and Analyses

Effect sizes. All average effect sizes represent weighted averages, with their respective weights described earlier. The estimated between-study variance (τ^2) equaled 0.02 for the overall effect estimates. The weighted average effect size for all studies was 0.19, df = 57, $p < .01$, 95% CI [0.11, 0.26]. Cognitive training alone presented an average effect of 0.26, df = 25, $p < .01$, 95% CI [0.13, 0.39], and

Table 1

Controlled Physical Exercise Trials: Effect Sizes and Reported Variables

	Study features		Outcomes	Sample size		Participant features					Treatment characteristics				
Authors	Year	PEDro	Constructs and effect sizes	Exp. (*n*)	Con. (*n*)	Avg. age	% Male	Cognitive status	Avg. MMSE	Group size	Control cond.	Length (wks)	Session length (hrs)	Sess./wk	Type
Anderson-Hanley et al.	2010	3	ES = .49; EA = .37; Inh. = .30; WM = .79	16	16	72.9	87.5			Group	WL	4	1	3	Nonaerobic
Barry et al.	1966	3	PS = −.37	8	5	71	51.3				WL	12	0.67	3	Combined
Blumenthal et al.* & Madden et al.+	1989 1989	7	ES = −.04; EA = .10; Flu. = −.06; Inh. = −.20; WM = .04;	31	32	66.7	49.3			Group	WL	16	1	3	Aerobic
			+ES = .05 +EA = .08; +WM = −.01	25	26										
			ES = −.07; EA = −.03; Flu = −.05; Inh. = −.12; WM = −.07;	34	32	67.3	48.5			Group	WL	16	1	2	Nonaerobic
			+ES = −.03 +AC = .02; +WM = −.09	28	26										
Brown et al.*	2009	5	ES = .13; EA = .01; Flu. = .18; Inh. = .02; WM = .31	66	34	78.8	13.3	Healthy	26.4	Group	WL	24	1	2	Nonaerobic
			ES = .09; EA = −.08; Flu. = .14; Inh. = .12; WM = .19	26	34	79.8	10.8	Healthy	26.1	Group	WL	24	1	2	Nonaerobic
Hawkins et al.	1992	7	EA = .24	18	18	68.2	27.6	Healthy		Group	Active	10	0.75	3	Aerobic
Kimura et al.	2010	8	EA = −.04	65	54	74.4	41.0	Healthy	27.85	Group	Active	12	1.5	2	Nonaerobic
Klusmann et al.^	2010	8	ES = .21 EA = .59; Flu. = .20; Inh. = −.14	80	69	73.6	0	Healthy	28.7	Group	WL	24	2	3	Combined
Legault et al.^	2011	8	ES = −.18; EA = −.35; Inh. = .34 WM = −.24;	16	17	75.7	61.1	Healthy		Group	Active	16	1.25	2	Aerobic
Liu-Ambrose et al.	2008	6	ES = .31; EA = .17; Inh. = .54; WM = .21	28	24	82.3	30.6	Healthy	28	Indiv.	Active	24	0.5	5	Nonaerobic
Liu-Ambrose et al.*	2010, 2012	8	ES = .13; EA = −.01; Inh. = .25; WM = .16	46	42	69.7	0	Healthy	28.7	Group	Active	52	1	2	Nonaerobic
			#Inh. = 1.14	15	17	69.8			28.9						
			ES = .22; EA = −.10; Inh. = .36; WM = .39	47	42	69.7	0	Healthy	28.7	Group	Active	52	1	1	Nonaerobic
			#Inh. = .34	20	17	69.7			28.9						
Maki et al.	2012	7	ES = .07; EA = .03; PS = −.08; Flu. = .31	66	67	72	29.3	Healthy	27.8	Group	Active	12	1.5	1	Aerobic
Molloy et al.	1988	7	Flu. = −.42	23	22	82.7	0	Healthy	24.8	Group	WL	12	0.5	3	Nonaerobic
Moul et al.*	1995	7	PS = .35	10	10	69.4	36.7			Group	Active	16	0.67	5	Aerobic
			PS = .22	10	10	69.5	36.7			Group	Active	16	0.67	5	Nonaerobic
Oken et al.*	2006	8	ES = .05; EA = .30; WM = −.08; Inh. = −.07	38	42	71.4	27.3	Healthy		Group	WL	24	1.5	1	Nonaerobic
			ES = .06; EA = .43; WM = −.07; Inh. = −.17	38	42	72.4	23.1	Healthy		Group	WL	24	1.5	1	Aerobic

Table 1 (*continued*)

	Study features		Outcomes	Sample size		Participant features					Treatment characteristics				
Authors	Year	PEDro	Constructs and effect sizes	Exp. (*n*)	Con. (*n*)	Avg. age	% Male	Cognitive status	Avg. MMSE	Group size	Control cond.	Length (wks)	Session length (hrs)	Sess./wk	Type
Powell	1974	4	PS = .55	11	12	69.3	43.3	Impaired$		Group		12	1	5	Combined
Scherder et al.*	2005	7	ES = .40; EA = .42; Flu. = .38	15	15	85	10.0	Impaired: MCI		Indiv.	Active	6	0.5	3	Aerobic
			ES = .57; EA = .88; Flu. = .26	13	15	87.5	11	Impaired: MCI		Indiv.	Active	6	0.5	3	Nonaerobic
Schwenk et al.	2010	7	EA = .30	20	29	81.4	36.1	Impaired: Dementia	21.35	Group	Active	12	2	2	Nonaerobic
Suzuki et al.	2012	8	ES = −.02; Flu. = .56; Inh. = −1.18	25	25	76.1	54	Impaired: Amnesic MCI	26.7	Group	Active	48	1.5	2	Aerobic
van Uffelen et al.	2008	9	ES = .00; Flu. = −.10; Inh. = .10	77	75	74.8	60.2	Impaired: MCI	29	Group	Active	52	1	2	Aerobic
Williams & Lord	1997	5	PS = .14	71	78	71.7	0.0			Group	WL	42	1	2	Combined
Williamson et al.	2009	7	Inh. = −.07	45	47	77.4	28.7	Healthy		Indiv.	Active	52	0.67	4	Combined
Yágüez et al.	2011	7	WM = .33	15	12	73.1	40.8	Impaired: AD	24.2	Group	Active	6	2	1	Nonaerobic

Note. ES is listed as the overall effect size for a study, derived from all outcomes regardless of executive construct. For studies including only one executive construct, ES is equivalent to the estimate of the single construct. AD = Alzheimer's disease; Avg. = average; Con. = control; EA = executive attention; ES = effect size; Exp. = experimental; Flu. = fluency; Indiv. = individual; Inh. = inhibition; MCI = mild cognitive impairment; MMSE = Mini-Mental State Examination; PS = problem solving; Sess./wk = sessions per week; WL = waitlist; WM = working memory.

* Denotes a study with multiple experimental group, with each group including the same control group features in effect size and weight calculation. + Denotes effect sizes from Madden et al. (1989), which used a subsample of participants from Blumenthal et al. (1989). # Denotes effect sizes from Liu-Ambrose et al. (2012), which used a subsample of participants from Liu-Ambrose (2010). $ Powell (1974) included institutionalized geriatric mental health patients with no specific diagnosis. ^ Denotes a study with both a physical exercise and a cognitive training intervention.

physical exercise resulted in an average effect of 0.12, $df = 31$, $p < .01$, 95% CI [0.04, 0.20].

Fail-safe *N*. For the overall effect size (both physical and cognitive treatments included in the weighted average), the fail-safe *N* calculation indicated a required addition of 450 effect sizes of zero to reduce the effect to nonsignificance. Separated by treatment style, dropping the effect size this low for cognitive training and physical exercise would require 177 and 27 effect sizes of zero, respectively.

Test of heterogeneity. The calculated effect sizes ranged from −0.45 to 1.37, resulting in significant heterogeneity, $Q(57) = 76$, $p < .05$, $I^2 = 0.25$. This marginal level of variance across trials supported the further exploration of their effect sizes through extracted study-related variables. See Figures 2 and 3 for a graphical summary of all results reported hereafter.

Weighted mean comparisons. The overall effect sizes of physical exercise interventions and cognitive training interventions did not significantly differ from each other, $t = 1.76$, $p = .09$, but the 95% CIs of these estimates only overlapped by a slight margin (i.e., .07), indicating a potential trend toward an advantage of cognitive interventions over those involving physical exercise. For the three design-related group comparisons (i.e., healthy vs. impaired samples, group vs. individual treatments, active vs. waitlist control groups), no significant differences were found between the means.

Although waitlist and active control conditions did not significantly differ from each other, only the effect size for waitlist studies differed significantly from zero for cognitive training interventions ($d_{ppc} = .29$, $p < .01$). Contrarily, physical exercise studies involving active controls reached significance ($d_{ppc} = .16$, $p = .01$), whereas waitlist studies did not differ from zero for this treatment modality ($d_{pcc} = .06$, $p = .27$). For physical exercise studies, group interventions produced effects significantly greater than zero ($d_{ppc} = .12$, $p < .01$), and individual interventions did not ($d_{ppc} = .12$, $p = .29$). For cognitive training, effect sizes for individual interventions reached significance ($d_{ppc} = .34$, $p = .01$), and effect sizes for group interventions only neared significance ($d_{ppc} = .19$, $p = .02$). Although healthy ($d_{ppc} = .11$, $p = .08$) and cognitively impaired samples ($d_{ppc} = .21$, $p = .05$) both produced only nonsignificant effects for physical exercise, cognitive training showed a significant effect for healthy samples ($d_{ppc} = .26$, $p < .01$), but a nonsignificant effect for impaired samples ($d_{ppc} = .20$, $p = .19$). See Table 3 for all weighted mean effect sizes based on categorical extracted variables and their respective *p* values. As part of the online supplemental materials, Appendix C provides tables listing unweighted mean and median effect sizes based on these same variables.

Construct analyses. Each construct had a separate τ^2 value: problem solving ($\tau^2 = .09$; $Q(16) = 36.93$, $p < .01$, $I^2 = .57$,), executive attention ($\tau^2 = .01$; $Q(27) = 31.14$, $p > .05$, $I^2 = .13$), working memory ($\tau^2 = .03$; $Q(26) = 37.85$, $p > .05$, $I^2 = .31$), inhibition ($\tau^2 = .07$; $Q(21) = 46.08$, $p < .01$, $I^2 = .54$), and fluency ($\tau^2 = .04$; $Q(21) = 37.13$, $p < .05$, $I^2 = .43$). For physical exercise, each construct produced an overall modest effect (range: 0.06–0.15); however, only the effects for executive attention ($d_{ppc} = .15$, $p = .02$) neared significance. In regards to cognitive training interventions, effect sizes showed a much greater range across constructs (range: 0.03–0.47), with problem solving differing reliably from zero ($d_{ppc} = .47$, $p = .01$).

Table 2

Controlled Cognitive Training Trials: Effect Sizes and Reported Variables

	Study features		Outcomes	Sample size		Participant features					Treatment characteristics			
Authors	Year	PEDro	Constructs and effect sizes	Exp. (*n*)	Con. (*n*)	Avg. age	% Male	Cognitive status	Avg. MMSE	Group size	Control cond.	Length (weeks)	Session length (hrs)	Sess./wk
Borella et al.	2010	7	ES = 1.18; PS = 1.14; WM = 1.70; Inh. = .67	20	20	69.1	42.5			Indiv.	Active	2	1	1.5
Bottino et al.	2005	8	ES = .12; EA = −.20; WM = .87 Flu. = −.30	6	7	73.8	29.8	Impaired: AD	22.4	Group	Active	20	1.5	1
Buiza et al.*	2008	7	ES = .09; PS = .13; Flu. = .06;	85	85	73.2	27.5	Healthy		Group	WL	104	1.5	2
			ES = .31; PS = .45; Flu. = .17	68	85	71.7	28.0	Healthy		Group	WL	104	1.5	2
Cahn-Weiner et al.	2003	7	ES = −.22; EA = −.32; Flu. = −.11	15	14	76.9	41.2	Impaired: AD	24.7	Group	Active	6	1.3	1
Calero & Navarro*	2007	6	WM = .30	51	27	76.9	65.4	Healthy		Group	WL	7	1	2
			WM = .28	27	28	76.9	65.4	Impaired#		Group	WL	7	1	2
Ceccato et al.	2012	8	WM = −.23	27	23	86.4	19.8	Impaired	16.7	Group	WL	12	.75	2
Clare et al.	2010	7	ES = −.15; EA = .00; Flu. = −.59	20	21	77.3	40.9	Impaired: AD	22.7	Indiv.	WL	8	1	1
Craik et al. & Stuss et al.	2007 2007	8	WM = .09	29	20	78.8	44.9	Healthy	28.0	Group	WL	14	4	1
Davis et al.	2001	9	ES = .09; WM = .22; Flu. = .05	19	18	70.6	43.0	Impaired: AD	22.3	Indiv.	Active	5	.50	7
Klusmann et al.^	2010	8	ES = .12; EA = .30; Inh. = −.09; Flu. = .14	80	69	73.6	0.0	Healthy	28.7	Group		24	1.5	3
Legault et al.^	2011	8	ES = −.01; EA = −.35; WM = .00; Inh. = .64	16	17	75.7	61.1	Healthy		Group	Active	16	.80	1.5
Margrett & Willis*	2006	7	PS = .74	30	34	71.3	50.0	Healthy		Indiv.	WL	6.5	1	2
			PS = .79	34	34	71.3	50.0	Healthy		Group	WL	6.5	1	2
Moro et al.	2012	5	ES = −.06 EA = −.49; PS = .05; Inh. = −.52; Flu. = .60	15	15	70.9		Impaired: Amnesic MCI		Indiv.	WL	24		1.33
Mozolic et al.	2011	8	ES = .39; EA = .41; WM = .34; Inh. = .03	30	32	69.4	47.0	Healthy	28.4	Indiv.	Active	8	1	1
Nouchi et al.	2012	10	Glob. = −.81			69.1	46.4	Healthy	28.5	Indiv.	Active	4	.25	5
Peretz et al.	2011	9	ES = .25; WM = .35 Glob. = .15	84	71	67.8	38.5	Healthy	29.0	Indiv.	Active	12	.50	3
Quayhagen & Quayhagen	1989	5	PS = 1.37	10	10	67.4	28.3	Impaired: AD		Indiv.	WL	32	1	6
Quayhagen et al.	1995	6	ES = .83; PS = .88; Flu. = .78	25	25	73.6	65.4	Impaired: AD		Indiv.	WL	12	1	6
Quayhagen et al.	2000	5	ES = .38; PS = .52; Flu. = .24	21	15	74.5	36.9	Impaired: Dementia		Indiv.	WL	8	1	5
Richmond et al.	2011	7	ES = .45;	21	19	66	20	Healthy	29	Indiv.	Active	4	.50	5

Table 2 (*continued*)

	Study features		Outcomes	Sample size		Participant features					Treatment characteristics			
Authors	Year	PEDro	Constructs and effect sizes	Exp. (*n*)	Con. (*n*)	Avg. age	% Male	Cognitive status	Avg. MMSE	Group size	Control cond.	Length (weeks)	Session length (hrs)	Sess./wk
Stine-Morrow et al.	2008	6	PS = −.41; WM = −.50 ES = .22; PS = .17; WM = .16; Flu. = .79	87	63	72.5				Group		20		1
Vance et al.	2007	7	EA = −.04	82	77	75.2		Healthy	28.6	Group	Active	12.3	1	0

Note. AD = Alzheimer's disease; Avg. = average; Con. = control; cond. = condition; EA = executive attention; Exp. = experimental; Flu. = fluency; Glob. = global; Indiv. = individual; Inh. = inhibition; MCI = mild cognitive impairment; MMSE = Mini-Mental State Examination; Sess./wk = sessions per week; PS = problem solving; WL = waitlist; WM = working memory.
* Denotes a study with multiple experimental group, with each group including the same control group features in effect size and weight calculation. ^ Denotes a study with both a physical exercise and a cognitive training intervention. # Calero & Navarro (2007) used a subsample considered cognitively impaired based on a cutoff for Spanish MMSE scores.

Meta-regression models. The first model included mean age and percent male as predictors of effect sizes. For physical exercise effect sizes, the overall test of conditionality failed to reject the null, $F(2, 29) = 0.820$, $p = .450$, $R^2 = 0.054$, resulting in nonsignificant β-weights for both average age ($\beta_{age} = -.001$, $p = .889$) and percent male ($\beta_{sex} = -.002$, $p = .211$). The same model neared significance when predicting cognitive training effect sizes, $F(2, 20) = 3.889$, $p = .037$, $R^2 = 0.280$, but both average age ($\beta_{age} = -.035$, $p = .046$) and percent male ($\beta_{sex} = .008$, $p = .062$) were nonsignificant predictors. The second model included treatment length, session length and session frequency as predictors of effect sizes. The overall model did not reach significance for either physical exercise effect sizes, $F(2, 28) = 0.248$, $p = .862$, $R^2 = 0.025$, or cognitive training effect sizes, $F(3, 20) = 0.223$, $p = .879$, $R^2 = 0.032$, with all predictors producing null regression-weights for both intervention styles.

Weighted correlations. Three continuous moderators not assessed in metaregression models were evaluated by examining their weighted correlations with treatment effect sizes: mean MMSE, year of publication, and study quality (i.e., PEDro score). For physical exercise studies, effect sizes did not correlate significantly with any of these variables. For cognitive training interventions, only year of publication neared significance ($r_w = -0.42$, $p = .03$), with more recent publications producing lower effect sizes. See Table 4 for all weighted correlations between effect sizes and extracted continuous variables. Appendix C, part of the online supplemental materials, includes a table listing these same correlations, but with unweighted effect sizes.

Discussion

Both physical exercise and cognitive training interventions presented reliable positive effect sizes for executive functions, demonstrating the shared efficacy of these two highly researched lifestyle interventions. Notably, physical exercise resulted in far narrower CIs for all categorical comparisons, showing more consistency in effect sizes across studies based on these moderators. This consistency likely derives from the common features of physical exercise treatments across studies (e.g., aerobic activity), whereas cognitive training paradigms present far greater diversity in both targeted cognitive abilities and intervention style. Notably, with the specificity of cognitive training toward specific neuropsychological abilities, its potential advantage over physical exercise may derive from the similarity of training and outcomes (i.e., training techniques share common features with dependent variables). This refers to near-transfer effects, where cognitive training procedures involve participants practicing tasks similar or identical to those serving as research outcomes. Notably, the trial producing one of the largest effects of cognitive training on problem solving, as measured through the Cattell test, did not train participants on this measure or any similar measure, with the researchers labeling the Cattell test as a far-transfer outcome (Borella et al., 2010). Contrarily, another trial resulting in a large problem solving effect size involved reasoning training, resulting in a near-transfer of the trained abilities to the problem solving outcomes (i.e., Letter Series Test, Word Series Test; Margrett & Willis, 2006). Similarly, Quayhagen and colleagues (1989, 1995, 2000) described the training of problem solving in each of their designs and found medium to large effects for this same construct. In turn, near-transfer effects have likely impacted the current results, but problem solving still appears susceptible to far-transfer effects of cognitive training (Borella et al., 2010). Nonetheless, the potential advantages of cognitive training should be considered in light of traditional fitness benefits of exercise among older adults (Heyn, Johnsons, & Kramer, 2008), where such interventions may better serve general health than a computerized cognitive training paradigm.

The benefits of physical exercise interventions appeared common across both cognitively impaired and healthy samples, elucidating the importance of exercise in the treatment of older adults across varying levels of cognitive decline. For cognitive training, the impaired samples did not experience significant treatment effects, and cognitively healthy samples did experience significant positive effects. A previous review of solely physical exercise trials similarly identified common cognitive benefits across older adults with and without cognitive impairment (van Uffelen, Chin A Paw, Hopman-Rock, & van Mechelen, 2008). However, considering the heterogeneity of the impaired sample within the current meta-analysis, this finding must be interpreted with caution. Impaired samples produced a much wider

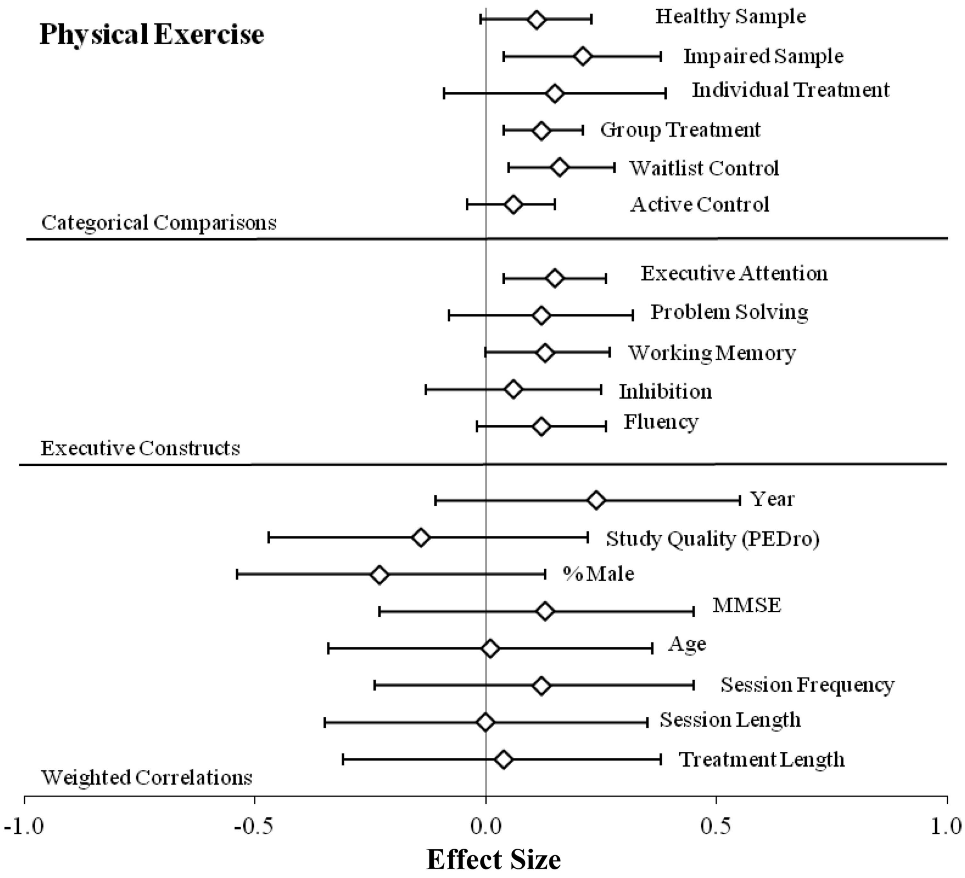

Figure 2. Forest plot of weighted correlations and weighted mean effect sizes by categorical variables and executive constructs for physical exercise interventions. MMSE = Mini-Mental State Examination. Error bars represent 95% confidence intervals.

CI, which suggests a higher variability in treatment outcomes among those experiencing cognitive declines, potentially due to the various diagnoses within this category. Addressing this conclusion with a continuous variable, MMSE scores did not meaningfully predict treatment outcomes, producing small and nonsignificant correlations with effect sizes for both styles of intervention. In turn, baseline global cognitive performance appears fairly unrelated to treatment efficacy, although only a limited amount of eligible studies reported MMSE scores, which biases the value of this conclusion.

Implications for Research

Across the nearly 50 years of research reviewed within this meta-analysis, publication year showed unique relationships with effect sizes across treatments. For cognitive training, effect sizes tended to decrease with more recent years of publication and study quality, indicating that earlier, lower-quality studies likely overestimated the effects of cognitive training interventions. Alternatively, although nonsignificant, recently published physical exercise trials tended to produce higher effects. Study quality did not significantly correlate with effect sizes for either treatment type; however, the correlations were in the direction of lower effect sizes with higher study quality, with the results for both interventions supporting two respective conclusions: (a) former research has not informed new designs to improve the accuracy of effect sizes across time, and (b) low quality studies may positively bias the supported benefits of these interventions. Figure 4 presents two scatter plots that graphically display each trend.

Despite these findings, studies beyond this meta-analysis may have produced larger effects, as some high quality clinical trials (e.g., Ball et al., 2002; Smith et al., 2009; Voss et al., 2010) did not meet inclusion criteria; and their exclusion from the meta-analysis biases the results and likely influences interpretation. In addition, many researchers may have conducted studies with progressive and more effective designs, but did not include an executive-related measure as an outcome. Both this meta-analysis and previous meta-analyses (e.g., Colcombe & Kramer, 2003; Sitzer et al., 2006) have demonstrated the benefits of these interventions on executive functions, regardless of the magnitude of the effect; and, in turn, future trials should always include and report an executive-related outcome, considering the importance of executive functions in the daily lives of aging populations (Boyle et al., 2003; Cahn-Weiner et al., 2002; Grigsby et al., 1998; Johnson et al., 2007; Martyr & Clare, 2012; Pereira et al., 2008). Further informing future research, these results indicate a need for higher quality trials,

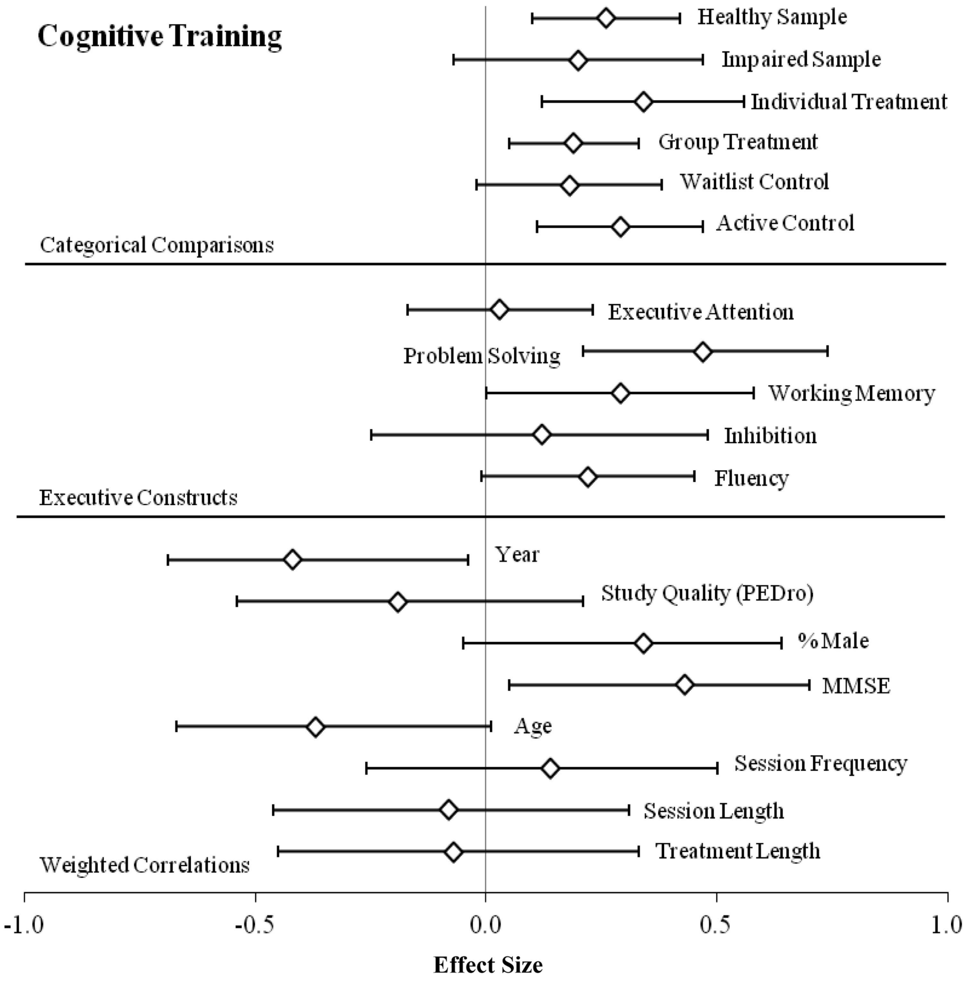

Figure 3. Forest plot of weighted correlations and weighted mean effect sizes by categorical variables and executive constructs for cognitive training interventions. MMSE = Mini-Mental State Examination. Error bars represent 95% confidence intervals.

informed by the strengths and weaknesses of past studies, to produce more accurate effects.

Implications for Practice

Cognitive training and physical exercise interventions did not significantly differ in effect sizes, although cognitive strategies resulted in a slightly larger overall effect. As both intervention styles present efficacy, they each have a role in clinical practice, and may produce their greatest benefits in combination (Thom & Clare, 2011); however, many additional variables related to these interventions likely influence their established effects. Social engagement may stand as a salient factor underlying the efficacy of physical exercise interventions (Kramer & Erickson, 2007); however, based on the reported meta-analytic findings, group designs did not show any significant difference from individual treatments for executive-related outcomes. Notably, group and individual treatments both approached significance for cognitive training, but only group treatments produced a significant effect for physical exercise interventions. In turn, group exercise may involve more positive social engagement than solitary exercise, while the benefits of individualized and group cognitive training appear less distinct. This result conflicts with past findings identifying a positive relationship between executive functions and social contact throughout the aging process (Seeman et al., 2011). In turn, the group versus individual design comparison may have lacked sufficient sensitivity to detect treatment gains attributable to the increased sociality of group-based interventions. Social engagement does appear cognitively beneficial (Fratiglioni, Paillard-Borg, & Winblad, 2004; Zunzunegui, Alvarado, Del Ser, & Otero, 2003) and should likely have a role in any applied behavioral intervention for older adults.

Furthermore, the treatment features examined in the current meta-analysis showed insignificant relationships with effect sizes. The outcomes of the treatments showed limited heterogeneity, which likely impacted the small relationships between effect sizes and these moderators. Notably, the studies yielding the largest effects for physical exercise (Anderson-Hanley et al., 2010, d_{ppc} = .49; Powell, 1974, d_{ppc} = .55; Scherder et al., 2005, d_{ppc} = .40, .57) had fairly frequent sessions (i.e., 3–5/week) with relatively short treatment durations (4–12 weeks). Contrarily, the largest effects for cognitive training

Table 3
Weighted Mean Effect Sizes (D_{ppc}), Standard Errors, and 95% Confidence Intervals (CI)

Physical exercise					Cognitive training				
Comparisons	$\bar{x}$	*df*	*p*	95% CI	Comparisons	$\bar{x}$	*df*	*p*	95% CI
Overall average	0.12	31	< .01	[0.04, 0.20]	Overall average	0.26	25	<.01	[0.13, 0.39]
Healthy sample	0.11	15	.08	[−0.01, 0.23]	Healthy sample	0.26	14	<.01	[0.10, 0.42]
Impaired	0.21	6	.05	[0.04, 0.38]	Impaired	0.20	9	.19	[−0.07, 0.47]
Individual	0.15	4	.29	[−0.09, 0.39]	Individual	0.34	12	.01	[0.12, 0.56]
Group	0.12	25	<.01	[0.04, 0.21]	Group	0.19	12	.02	[0.05, 0.33]
Waitlist control	0.06	12	.27	[−0.04, 0.15]	Waitlist control	0.29	13	<.01	[0.11, 0.47]
Active control	0.16	17	.01	[0.05, 0.28]	Active control	0.18	10	.11	[−0.02, 0.38]
	$\bar{x}$	*df*	*p*	95% CI		$\bar{x}$	*df*	*p*	95% CI
Construct					Construct				
Executive attention	0.15	19	.02	[0.04, 0.26]	Executive attention	0.03	7	.78	[−0.17, 0.23]
Problem solving	0.12	5	.29	[−0.08, 0.32]	Problem solving	0.47	10	.01	[0.21, 0.74]
Working memory	0.13	13	.08	[0.00, 0.27]	Working memory	0.29	12	.07	[0.00, 0.58]
Inhibition	0.06	16	.54	[−0.13, 0.25]	Inhibition	0.12	4	.56	[−0.25, 0.48]
Fluency	0.12	10	.13	[−0.02, 0.26]	Fluency	0.22	10	.09	[−0.01, 0.45]

Note. Mean values represent weighted means.

derived from more inconsistently designed studies, with very brief interventions (Borella et al., 2010, $d_{ppc} = 1.18$) and much longer interventions (Quayhagen & Quayhagen, 1989, $d_{ppc} = 1.37$) producing the largest effects. Explaining this similarity in outcomes despite variation in treatment length, longer interventions may reach ceiling effects. In such cases, increased treatment duration would merely maintain the established benefits rather than increase them. In regards to session length, nearly all studies involved sessions lasting approximately an hour, resulting in limited between-study variance for this moderator.

Notably, the three physical exercise studies producing the largest effects involved distinct styles of exercise, including a strength conditioning intervention (Anderson-Hanley et al., 2010), two trials assessing strength and aerobic training separately (Scherder et al., 2005) and a combined aerobic/strength exercise program (Powell, 1974). For the cognitive training studies, the largest effect derived from a training trial of verbal working memory, which would have clearly stimulated the executive system throughout the intervention (Borella et al., 2010). Similarly, the second largest effect derived from a program involving "conversation, memory-provoking exercises, and problem-solving techniques" (Quayhagen & Quayhagen, 1989, p. 151); however, these researchers provided only a very brief description of their intervention.

In combination with past research findings, the evidence synthesized by the current meta-analysis emphasizes a triad of treatment features important for more multifaceted behavioral interventions targeting executive functions, specifically mental stimulation (Woods et al., 2012), physical exercise (Colcombe et al., 2004), and social engagement (Seeman et al., 2011). In turn, the most efficacious interventions likely involve older adults regularly exercising their body and mind in enjoyable social settings.

Executive Functions and Everyday Life

Cognitive training and physical exercise interventions showed remarkable divergence in terms of their impact on different executive constructs. Notably, problem solving was associated with the largest effect size for cognitive training, but the effect of physical exercise on this construct did not differ reliably from zero. Problem solving is a relevant skill in many activities of daily living requiring a less-than-habitual program of action, from preparing food or planning the attire for the day to dealing with unpredictable situations such as a car breakdown or being rerouted while driving. Considering the importance of this construct in daily life, cognitive

Table 4
Weighted Correlations Between Effect Sizes (D_{ppc}) and Study Characteristics

	Physical exercise					Cognitive training				
	Moderator	r_w	*df*	*p*	95% CI	Moderator	r_w	*df*	*p*	95% CI
Study features	Year	0.24	31	.18	[−0.11, 0.55]	Year	−0.42	25	.03	[−0.69, −0.04]
	PEDro	−0.14	31	.45	[−0.47, 0.22]	PEDro	−0.19	25	.34	[−0.54, 0.21]
Participant features	Age	0.01	31	.96	[−0.34, 0.36]	Age	−0.37	25	.06	[−0.67, 0.01]
	% Male	−0.23	31	.21	[−0.54, 0.13]	% Male	0.34	22	.11	[−0.05, 0.64]
	MMSE	0.13	24	.66	[−0.23, 0.45]	MMSE	0.43	12	.15	[0.05, 0.70]
Treatment features	Session frequency	0.12	31	.53	[−0.24, 0.45]	Session frequency	0.14	25	.50	[−0.26, 0.50]
	Session length	0.00	31	.98	[−0.35, 0.35]	Session length	−0.08	23	.69	[−0.46, 0.31]
	Treatment length	0.04	31	.83	[−0.31, 0.38]	Treatment length	−0.07	25	.73	[−0.45, 0.33]

Note. Correlation values represent weighted correlations. CI = confidence interval; MMSE = Mini-Mental State Examination.

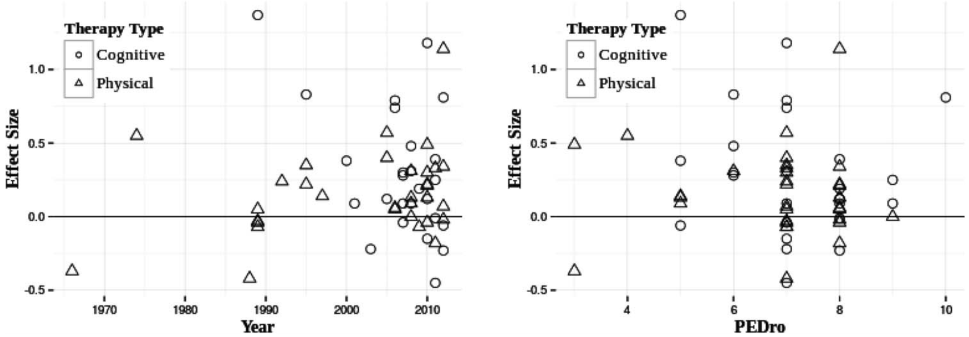

Figure 4. Scatter plots of effect sizes by year and study quality (PEDro).

training likely serves as a better treatment than physical exercise to enhance problem solving among older adults.

Cognitive training also presented a potential advantage at improving working memory, and physical exercise produced a modest effect as well. Notably, working memory remains a limited and less malleable capacity system, and as such it is not surprising that studies have documented performance gains that are only moderately transferable to other abilities required in everyday life activities (Dahlin, Nyberg, Båckman, & Neely, 2008). Nonetheless, both treatment styles appear to at least slightly benefit working memory, indicating the importance of such lifestyle interventions to maintain this ability into later age. As a construct related to working memory, fluency presented modest effects across treatment modalities, with a clear advantage of cognitive training.

Although cognitive training produced larger effects for most constructs, physical exercise showed an advantage specific to executive attention, although the treatment-related effect for this construct remained quite modest. Enhancement of executive control over attention (as demonstrated by rapid and efficient attentional switching from one stimulus to another when the contingencies of the environment change) has a positive effect on other abilities such as fluid intelligence (Karbach & Kray, 2009), which are relevant to everyday behaviors in an ever-changing environment. The advantage of physical exercise at improving this construct ensures its place in combined treatments targeting such higher-order cognitive abilities.

Finally, according to the current results, inhibitory control appears to be far less malleable than any other executive function in response to either intervention, demonstrating the lowest weighted mean effect size for physical exercise and second lowest for cognitive training. Studies on aging have rather consistently shown that increased age is associated with decreased performance on inhibitory tasks (Jurado & Roselli, 2007). The negative impact of a decreased inhibitory control may generalize to other goal-directed behaviors in older adults (Darowski et al., 2008) and neither cognitive training nor physical exercise may be effective at minimizing this decline.

Future Directions

Although combined treatments did not fit within the scope of the meta-analysis, the systematic review came across a small sample of combined treatment studies (Fabre, Chamari, Mucci, Massé-Biron, & Préfaut, 2002; Legault et al., 2011; Oswald, Gunzelmann, Rupprecht, & Hagen, 2006). Considering the saturated field of physical and cognitive interventions, these multifaceted designs likely characterize the future of behavioral intervention research among older adults (Thom & Clare, 2011). With the well-documented neurorehabilitative qualities of exercise (Colcombe & Kramer, 2003) and cognitive training (Sitzer, Twamley, & Jeste, 2006), the combined influence of these interventions requires further exploration to assess their collective influence on cognitive wellbeing at later age.

Encouraged to combine treatment strategies, researchers and practitioners must design holistic behavioral programs, incorporating an array of strategies to improve cognitive health outcomes throughout later age. In addition to the benefits of exercise and cognitive training, social engagement (Seeman et al., 2011) and dietary change (Karr, Alexander, & Winningham, 2011; Mattson, 2000) have similarly presented cognitive benefits among older adults. As the empirical support grows for multidimensional therapies, researchers must validate more comprehensive interventions; however, they must also maintain their simplicity to encourage their mobilization into community, retirement, and clinical settings. Such person-centered approaches encourage patients' control over cognitive aging, providing practical lifestyle solutions to maintain a healthy mind into later age.

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Correction to Wong et al. (2014)

In the article "Eye Movements Reveal Impaired Inhibitory Control in Adult Male Fragile X Premutation Carriers Asymptomatic for FXTAS" by Ling M. Wong, Naomi J. Goodrich-Hunsaker, Yingratana McLennan, Flora Tassone, Melody Zhang, Susan M. Rivera, and Tony J. Simon (*Neuropsychology*, 2014, Vol. 28, No. 4, pp. 571–584. doi:10.1037/neu0000066), references to "number of anticipatory saccades" in the prosaccade and antisaccade tasks should actually refer to "percentage of trials with anticipatory saccades."

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