Making Working Memory Work: A Meta-Analysis of Executive-Control and Working Memory Training in Older Adults

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Abstract
This meta-analysis examined the effects of process-based executive-function and working memory training (49 articles, 61 independent samples) in older adults (> 60 years). The interventions resulted in significant effects on performance on the trained task and near-transfer tasks; significant results were obtained for the net pretest-to-posttest gain relative to active and passive control groups and for the net effect at posttest relative to active and passive control groups. Far-transfer effects were smaller than near-transfer effects but were significant for the net pretest-to-posttest gain relative to passive control groups and for the net gain at posttest relative to both active and passive control groups. We detected marginally significant differences in training-induced improvements between working memory and executive-function training, but no differences between the training-induced improvements observed in older adults and younger adults, between the benefits associated with adaptive and nonadaptive training, or between the effects in active and passive control conditions. Gains did not vary with total training time.

Scientific interest in cognitive interventions designed to maintain or improve cognitive functions in the aging brain has been rapidly increasing over the past decade. Numerous studies investigating the effects of such interventions have shown that plasticity (i.e., the potential modifiability of a person's cognitive abilities and brain activity) is considerable up to very old age (Buitenweg, Murre, & Ridderinkhof, 2012; Hertzog, Kramer, Wilson, & Lindenberger, 2008; Karbach & Schubert, 2013; Lustig, Shah, Seidler, & Reuter-Lorenz, 2009; Noack, Lövdén, Schmiedek, & Lindenberger, 2009). In addition to demonstrating significant performance improvements on the training tasks, many studies have found near transfer to tasks that were not explicitly trained but measured the same construct as the training task, as well as far transfer to tasks that measured a different construct. However, the fact that these transfer effects have not been observed consistently across studies has recently inspired heated debates (e.g., Melby-Lervåg & Hulme, 2013; Redick et al.,

2013; Shipstead, Redick, & Engle, 2012). One reason for the inconsistent pattern of results may be that the large differences in the type, intensity, and duration of the training regimes and in analytic methods and designs adopted hamper the comparability of different studies' findings. For example, training regimes have ranged from a few days to months in duration, and trained individuals have been compared with active control groups in some studies and with passive, no-contact groups in others (Noack, Lövdén, & Schmiedek, 2014).

Generally, three basic categories of cognitive-training interventions can be distinguished: First, strategy-based training (e.g., training in the method of loci) typically results in large, and often long-lasting, improvements on

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the training task, but only limited transfer (Rebok, Carlson, & Langbaum, 2007; Verhaeghen, Marcoen, & Goossens, 1992). Second, multidomain training interventions (e.g., video-game training) are more complex and engage multiple cognitive processes, yielding broad, but often small, transfer effects (e.g., Basak, Boot, Voss, & Kramer, 2008; Park et al., 2014; Schmiedek, Lövdén, & Lindenberger, 2010). The main disadvantage of multidomain training is that its complex nature makes it hard to determine which specific features of the training regime induced transfer. Third, process-based training protocols target more general processing capacities, such as speed of processing or executive functions (EFs), which usually show a marked age-related decline (e.g., Li et al., 2004). EFs are a set of higher-level control processes supporting adaptation to changing environments and task demands. They include working memory (WM), inhibition, and cognitive flexibility (e.g., Miyake et al., 2000). Some process-based interventions, mainly focusing on EFs, have resulted in promising transfer of training in participants up to very old age (e.g., Brehmer, Westerberg, & Bäckman, 2012; Karbach & Kray, 2009; Li et al., 2008; Zinke et al., 2014), which suggests that process-based training might be more efficient at eliciting transfer than strategy-based interventions are. Because a systematic analysis comparing results of different types of training interventions in older adults had not yet been conducted, performing such a meta-analysis was one of the main goals of the study we report here.

In accordance with the typical terminology in the field, we make a distinction between WM training, aimed at improving scores on tests of WM capacity (e.g., operation span tests) or WM functioning (e.g., *n*-back tests), and EF training, aimed at improving scores on tests of dual-task performance, inhibition and interference control, task switching, and general forms of attention. We note that although *n*-back training is often considered WM training (e.g., Shipstead et al., 2012), *n*-back is also considered an updating task; therefore, we also analyzed effects of *n*-back and span training separately.

Another issue that has been debated in the cognitive-aging literature concerns age-related differences in cognitive plasticity—age differences in the magnitude of training and transfer effects. Studies of strategy-based memory training have repeatedly provided evidence for larger training gains in younger adults than in older adults (e.g., Brehmer, Li, Müller, von Oertzen, & Lindenberger, 2007; Lindenberger, Kliegl, & Baltes, 1992; Lövdén, Brehmer, Li, & Lindenberger, 2012; Verhaeghen & Marcoen, 1996; Verhaeghen et al., 1992; but see Gross et al., 2012). These magnification effects suggest that younger adults have more efficient cognitive resources to acquire and implement new strategies. In contrast, studies of process-based EF training have revealed larger

training-related gains in older adults than in younger adults (e.g., Bherer et al., 2008; Cepeda, Kramer, & Gonzalez de Sather, 2001; Karbach & Kray, 2009; Kramer, Larish, & Strayer, 1995; Kray, Eber, & Karbach, 2008). These compensation effects suggest that younger adults are already functioning at a more optimal level that leaves less room for performance improvements. Although these and other findings indicate that process-based training may be more beneficial than strategy-based approaches for older adults, a comprehensive analysis across studies is needed before more general conclusions regarding age differences in the effectiveness of process-based cognitive interventions can be drawn. In the current study, we looked for possible age-related differences in analyses restricted to studies that included both younger and older adults, so that differences between age groups were not confounded with other variables. Results of these analyses are of high relevance both for understanding the cognitive and neural underpinnings of cognitive plasticity and for adapting training interventions to populations with specific needs, such as individuals in old-old age or in clinical settings.

Thus, the aim of the present study was to apply meta-analytic techniques to quantitatively investigate the extent to which process-based cognitive training improves cognitive functions in older age. Meta-analyses allow for summarizing the association of two variables across different studies by calculating overall effect sizes as well as effect sizes for each study and for the influence of moderator variables. Given that EF and WM training seem to be particularly beneficial for older adults and can result in widespread transfer, we focused on training interventions targeting these domains. Our study extends previous meta-analyses (Hindin & Zelinski, 2012; Karr, Areshenkoff, Rast, & Garcia-Barrera, 2014; Melby-Lervåg & Hulme, 2013) because we included a sizable number of recently published training studies and systematically examined possible age differences in the effects of different types of process-based EF and WM training across the adult life span.

Method

We searched Science Direct databases (PsycInfo, PsycArticles) with the following key terms: "executive-functions training," "cognitive-control training," "working-memory training," "updating training," "inhibition training," and "switching training," in combination with "older adults," "aging," or both. We also checked the references in each of the collected articles for studies that might have been overlooked. Our search was concluded in December 2013. Studies were included in our analyses if (a) they contained process-based EF or WM training or a practice condition consisting of repeated exposure to the

relevant task (we excluded multicomponent treatments, such as those involving game playing, training batteries including other types of tasks, and combinations of cognitive and pharmacological or physical-exercise interventions), (b) they examined at least one sample of healthy older adults (mean age > 60 years), (c) the data were reported in a format amenable to meta-analysis, and (d) the study was published in the English language in a peer-reviewed journal.

The final sample consisted of 49 articles, reporting results for 61 different experiments or independent subject groups (for a list of the articles, see Appendix 1 in the Supplemental Material available online). Some of these studies included a passive control condition, in which subjects were retested at approximately the same time interval as in the training condition without receiving any additional treatment; some included an active control condition, in which subjects were retested at approximately the same time interval as in the training condition and received additional treatment that did not qualify as WM or EF training (e.g., they filled out questionnaires, completed physical training or computer training, attended educational lectures, learned trivia, played games, performed visual search tasks, or took quizzes). Some of the studies also included one or more samples of younger adults. The mean age of the samples ranged from 17 to 31 for younger adults and 63 to 87 for older adults.

Our first analysis concerned gain scores: We calculated treatment gain as the mean standardized difference in performance between posttest and pretest, ($M_{\text{posttest}} - M_{\text{pretest}}$)/$SD_{\text{pooled}}$. This statistic indicates how many standard deviations separate performance before treatment and performance after treatment. When means or standard deviations were not reported, we used inferential statistics to determine effect sizes. All effect sizes were corrected for sample size (Hedges & Olkin, 1985). We compared treatment gains in training groups with treatment gains in passive and active control groups to determine whether the gains associated with training were due to the specific interventions rather than to retest, placebo, or reactivation effects. Our second analysis concerned the net treatment effect at posttest, expressed as the mean standardized difference between trained and control subjects, ($M_{\text{trained}} - M_{\text{control}}$)/$SD_{\text{pooled}}$, weighted for sample size.

All effect sizes were coded such that positive values denote better (i.e., faster or more accurate) performance. Some effects are typically expressed as difference scores (e.g., dual-task costs, task-switching costs, flanker effects, Stroop effects). In such cases, if difference scores were not provided, we calculated them from the relevant conditions; the standard deviations of the difference scores were calculated from the component conditions using a

between-conditions correlation of .9 for Stroop, Trail Making, and flanker tests and .8 for task-switching and dual-task paradigms; these estimates were based on our own previous data as well as other researchers'.[1] For Stroop effects, we restricted ourselves to response time measures.

For each of the included studies, we recorded the following variables: age of participants, number of participants, session duration, number of sessions, number of hours of training, pretest-posttest interval, type or types of intervention (training, passive control, or active control), and type of dependent measure (or measures). We classified each measure as a target, near-transfer, or far-transfer measure. A target measure was performance on a task explicitly practiced in the training condition. A near-transfer measure was performance on a task that was not explicitly included in the training but measured the same construct that the training focused on; for example, if participants were trained in an *n*-back task (a WM task), an operation span task would be a near-transfer task, and if task-switching training involved two tasks A and B, a test alternating tasks C and D would be a near-transfer task. A far-transfer measure was performance on a task that measured a construct different from the one that was the focus of training; for example, if a WM task was used for training, a task-switching task or a reasoning test would be a far-transfer task. Selected characteristics of the studies are reported in Table 1.

Initially, an effect size was calculated for each dependent measure in a given study. These values were then collapsed so that only a single estimate per study entered the final comparisons for a given type of measure (e.g., we averaged all target measures within a study to form a single-point estimate for target measures). We pooled effect sizes within each grouping of interest (e.g., near-transfer measures) by calculating a mean effect size (d_+), weighted for sample size (cf. Hedges & Olkin, 1985).

Results

Gain scores

Figure 1 presents the funnel plots used to investigate publication bias in the gain scores for older adults. In each graph, sample size is plotted against effect size in the training condition; the size of the bubbles is proportionate to the precision of measurement, as indexed by 1/SE (thus, larger bubbles denote more precise measurements). The plot for target measures (Fig. 1a) is not significantly asymmetric, which suggests a lack of publication bias, Egger's bias (Egger, Smith, Schneider, & Minder, 1997) = 1.19, *p* = .67. Only the single largest effect size was an outlier according to disjoint cluster analysis (Hedges & Olkin, 1985). Removing this data point from

Table 1. Selected Characteristics of the Studies Included in the Meta-Analysis

Characteristic	All studies (*k* = 61)	Executive-control studies (*k* = 48)	Working memory studies (*k* = 13)
Mean number of sessions	9.81 (*SD* = 14.85)	7.96 (*SD* = 15.19)	16.66 (*SD* = 11.61)
Mean session duration (hr)	0.87 (*SD* = 0.40)	0.96 (*SD* = 0.42)	0.67 (*SD* = 0.25)
Mean number of hours of training	8.93 (*SD* = 16.06)	8.24 (*SD* = 17.75)	10.69 (*SD* = 11.05)
Mean time between pretest and posttest (days)	24.16 (*SD* = 31.38)	21.46 (*SD* = 33.92)	32.25 (*SD* = 21.28)
Mean number of older-adult subjects	21.34 (*SD* = 13.98)	20.69 (*SD* = 14.18)	23.73 (*SD* = 13.48)
Average age of older-adult subjects	69.42 (*SD* = 3.45)	69.25 (*SD* = 2.71)	70.01 (*SD* = 5.49)
Mean number of younger-adult subjects	21.51 (*SD* = 16.40)	21.12 (*SD* = 16.96)	25.50 (*SD* = 9.66)
Average age of younger-adult subjects	22.45 (*SD* = 2.69)	22.20 (*SD* = 2.66)	24.97 (*SD* = 1.70)
Studies that included younger adults (%)	55	65	23
Studies that included near-transfer tasks (%)	56	44	100
Studies that included far-transfer tasks (%)	44	29	100
Studies that included a passive control condition (%)	30	29	31
Studies that included an active control condition (%)	39	31	69
Studies that included adaptive training (%)	21	6	77
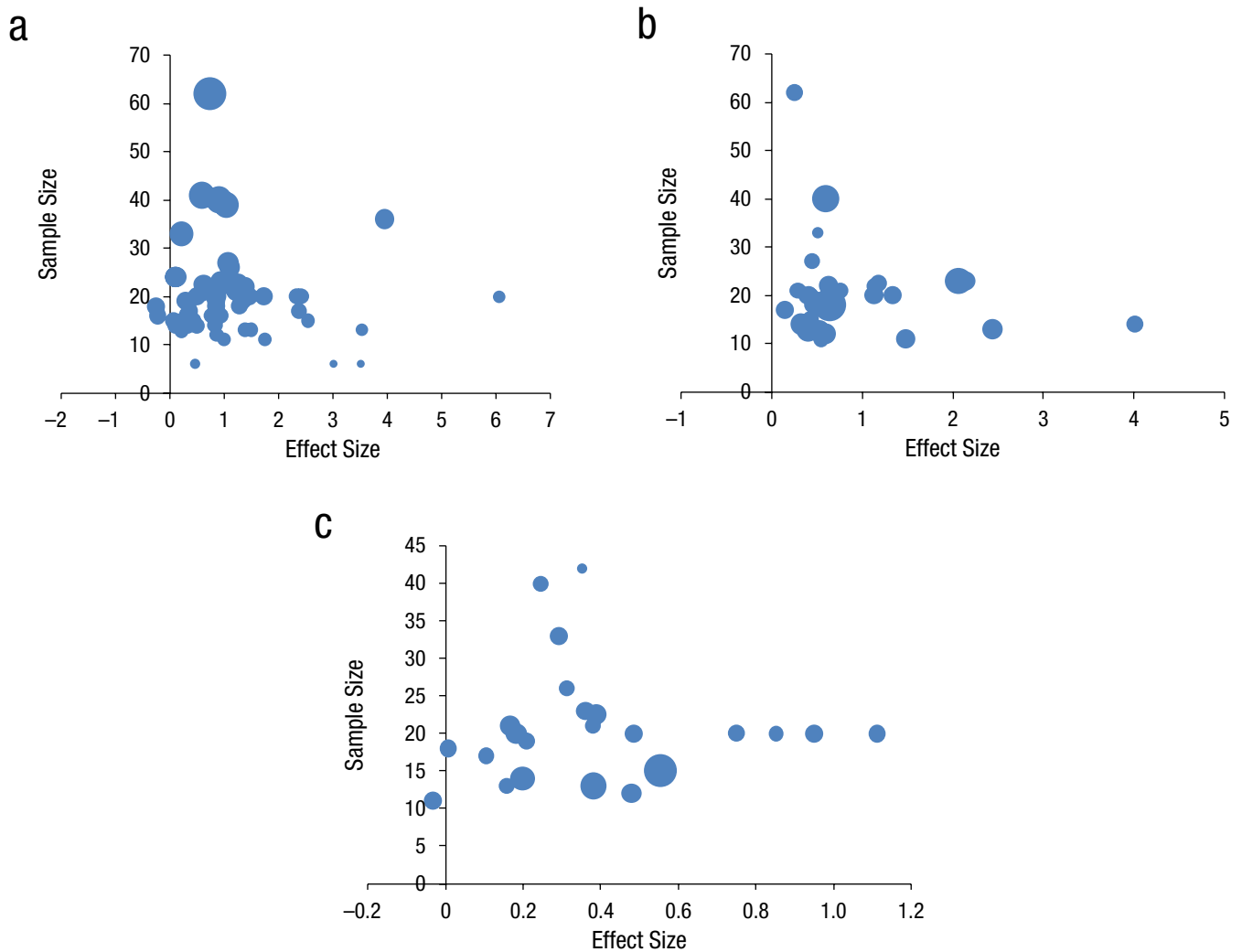
Fig. 1. Funnel plots for pretest-to-posttest gain: sample size as a function of effect size for (a) target measures, (b) near-transfer measures, and (c) far-transfer measures. Bubble size denotes 1/*SE*; thus, larger bubbles indicate more precise measurements.
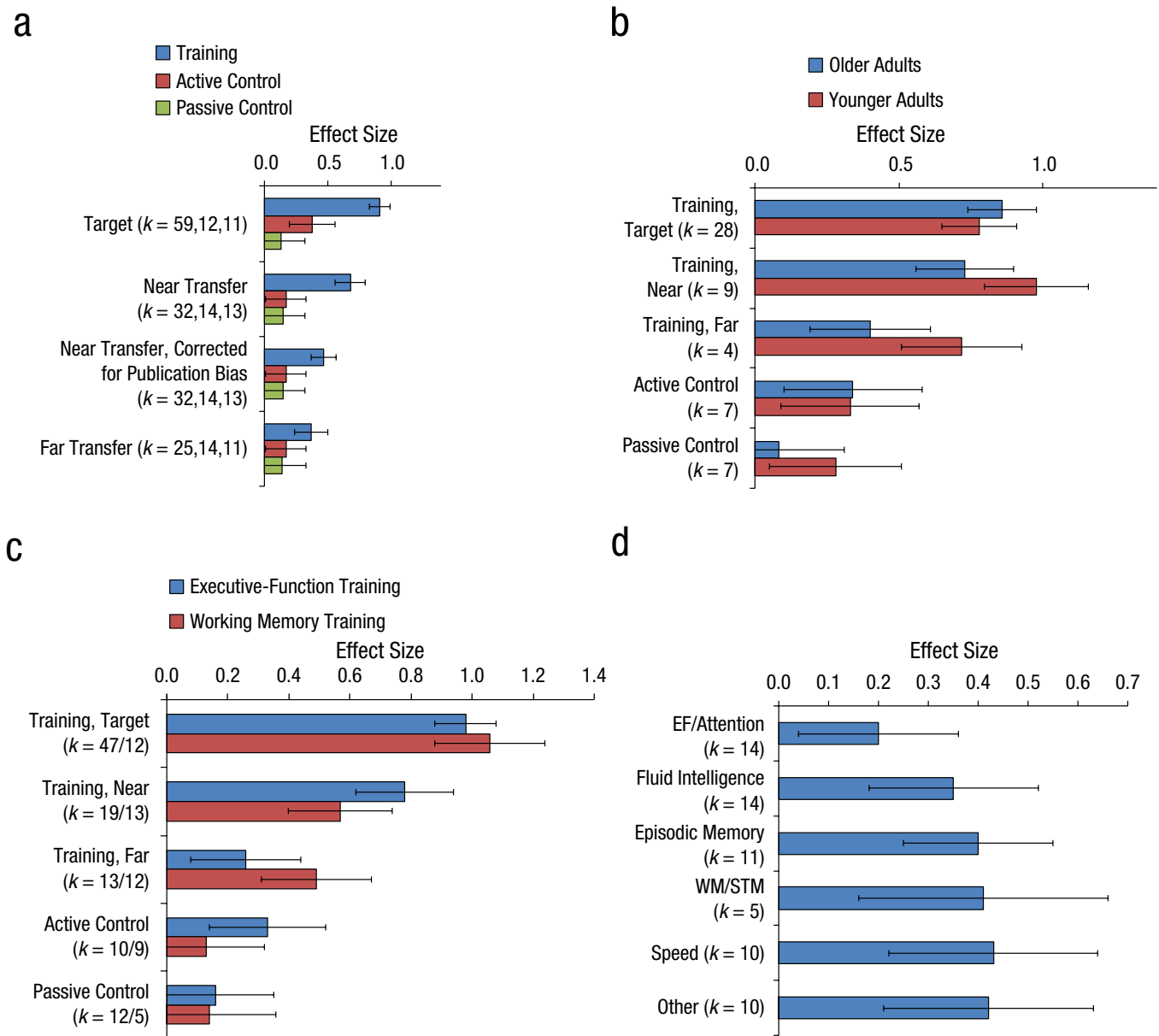
Fig. 2. Average pretest-to-posttest gains. The graph in (a) shows effect sizes for older adults by treatment (training, active control, and passive control) and type of measure (target, near transfer, and far transfer). Note that results for near-transfer measures both before and after correction for publication bias are shown. The graph in (b) shows effect sizes for older and younger adults by treatment and type of measure; only studies that included both younger and older adults were included, and because of the small number of comparisons within the younger-adults samples, we collapsed over measures within each of the control treatments. Data for older adults (all studies) are presented in (c) broken down by whether executive-function (EF) training or working memory (WM) training was tested. The graph in (d) shows effect sizes for far transfer of training in older adults broken down by the specific type of transfer measure. In all panels, error bars denote 95% confidence intervals. The number of studies, *k*, that contributed effect sizes is shown for each bar or set of bars; when there are multiple values, their order matches the order in which the bars are presented on the graph (top to bottom). STM = short-term memory.

analysis did not alter the results substantially; therefore, we conducted all analyses on the full data set. The plot for near-transfer measures (Fig. 1b), however, is significantly asymmetrical, Egger's bias = 5.76, p = .03. The skew suggests that there are fewer studies with negative results than might be expected. We therefore conducted our analyses both on the near-transfer gain scores as

found and on the average weighted effect size corrected for publication bias, using Duval and Tweedle's (2000) trim-and-fill correction. The far-transfer data showed no indication of publication bias (Fig. 1c), Egger's bias = 0.54, p = .79.

Figure 2a shows the overall effects of training and control treatments on target and transfer measures among

older adults. Effect sizes were heterogeneous for target measures ($Q_W = 286.53$) and near-transfer measures ($Q_W = 96.03$) within the training groups, which suggests that the effect sizes within these groups were highly variable (detailed effect sizes and homogeneity statistics, including results for younger adults, are presented in Appendix 2 in the Supplemental Material available online).

Five conclusions emerged from analysis of the effect sizes among older adults. First, in the training groups, effects on target and near- and far-transfer measures were all significantly larger than zero. Second, training led to significantly larger improvements on target measures than either control treatment did (training vs. active control: $Q_B = 30.01$, $p < .001$; training vs. passive control: $Q_B = 51.86$, $p < .001$). The training-related effect on target measures was 0.91, whereas the effect sizes were 0.38 for active control groups and 0.13 for passive control groups. Third, the effects of training on near-transfer and far-transfer measures were reliably smaller (as demonstrated by nonoverlap in the 95% confidence intervals, or CIs) than the effects of training on target measures (near transfer: effect size = 0.68 uncorrected, 0.47 corrected; far transfer: effect size = 0.37; target: effect size = 0.91). Fourth, effect sizes for transfer measures were also reliably larger for the training conditions than for either control treatment in three of the four comparisons—near transfer, training versus active control: $Q_B = 26.53$, $p < .001$; near transfer, training versus passive control: $Q_B = 26.11$, $p < .001$; far transfer, training versus passive control, $Q_B = 4.17$, $p < .05$; far transfer, training versus active control, $Q_B = 3.66$, $p = .056$. The net gain associated with training was about 0.50 SD (0.30 SD after removal of publication bias) for near-transfer tasks and 0.20 SD for far-transfer tasks. Finally, active and passive control treatments yielded statistically indistinguishable effects—target measures: $Q_B = 3.33$, $p = .068$; near-transfer measures: $Q_B = 0.03$, $p = .86$; far-transfer measures: $Q_B = 0.07$, $p = .79$. We note, though, that the difference for target measures was marginally significant, which suggests that active control treatment might lead to larger effects on target measures than passive treatment does, maybe because of Hawthorne or other expectancy effects.

To explain the heterogeneity of effect sizes within the trained subjects, we conducted two random-effects meta-regression analyses, one on target measures and one on near-transfer measures, using the following predictors: age, total time spent in training, type of training (0 = EF; 1 = WM), and whether or not the training was adaptive. The fit was poor for both analyses ($R^2 = .04$ and .11, respectively), and none of the predictors were significant.

Figure 2b summarizes pretest-to-posttest gain for younger and older adults separately. We restricted our analyses to studies including both younger and older adults, so that differences between age groups could not

be ascribed to any of the other variables included in the studies. Because of the small number of comparisons within the younger-adults samples, we collapsed over measures within the active and passive control groups. Our analyses led to a simple conclusion: No reliable age differences were detectable within the set of studies analyzed (largest $Q_B = 2.45$, $p = .12$). Again, within the training groups, effect sizes were heterogeneous for target measures ($Q_W = 145.34$ for older adults and 177.29 for younger adults) and near-transfer measures ($Q_W = 88.55$ for older adults and 30.00 for younger adults).

In Figure 2c, the older-adult data from all the studies in the meta-analysis are separated according to the type of training used in the study: WM or EF. The two types of training did not differ reliably in their effects on cognition (there were marginal effects on near transfer, $Q_B = 3.16$, $p = .075$, and far transfer, $Q_B = 3.03$, $p = .082$, going in opposite directions). Effect sizes within the training groups were again heterogeneous for target measures ($Q_W = 353.97$ for EF training and 28.06 for WM training) and near-transfer measures ($Q_W = 65.90$ for EF training and 29.98 for WM training). Within the WM-training sample, a comparison of training focused on WM capacity (eight studies) and training focused on *n*-back tasks (four studies) revealed a significant difference in effect sizes for target measures only (WM capacity: 0.93; *n*-back: 1.44; $Q_B = 6.10$, $p < .05$).

Figure 2d shows effect sizes for far transfer of training in older adults for more specific categories of cognitive measures (note that subdividing the measures also diluted statistical power, resulting in wider CIs). The CIs for all types of measures overlap, which suggests that training benefited them all equally. All these effect sizes were significant.

Net treatment effects at posttest

Figure 3 presents the funnel plots used to investigate publication bias in the net treatment effects in older adults. The plot for target measures (Fig. 3a) is significantly asymmetric, Egger's bias = 14.95, $p = .035$. Neither the near-transfer plot (Fig. 3b; Egger's bias = 8.16, $p = .11$) nor the far-transfer plot (Fig. 3c; Egger's bias = 2.32, $p = .35$) shows significant asymmetry. We therefore conducted our analyses of target measures both on the net gains as found and on the average weighted effect sizes corrected for publication bias (trim-and-fill correction).

Figure 4 shows the average effect sizes for the net effect at posttest among older adults. (Sample sizes were smaller than in the previous analyses simply because not all studies included one or both of the control conditions.) The main result (Fig. 4a) is clear: All effects were significantly larger than zero. This indicates that the effects of WM or EF training were reliably larger than
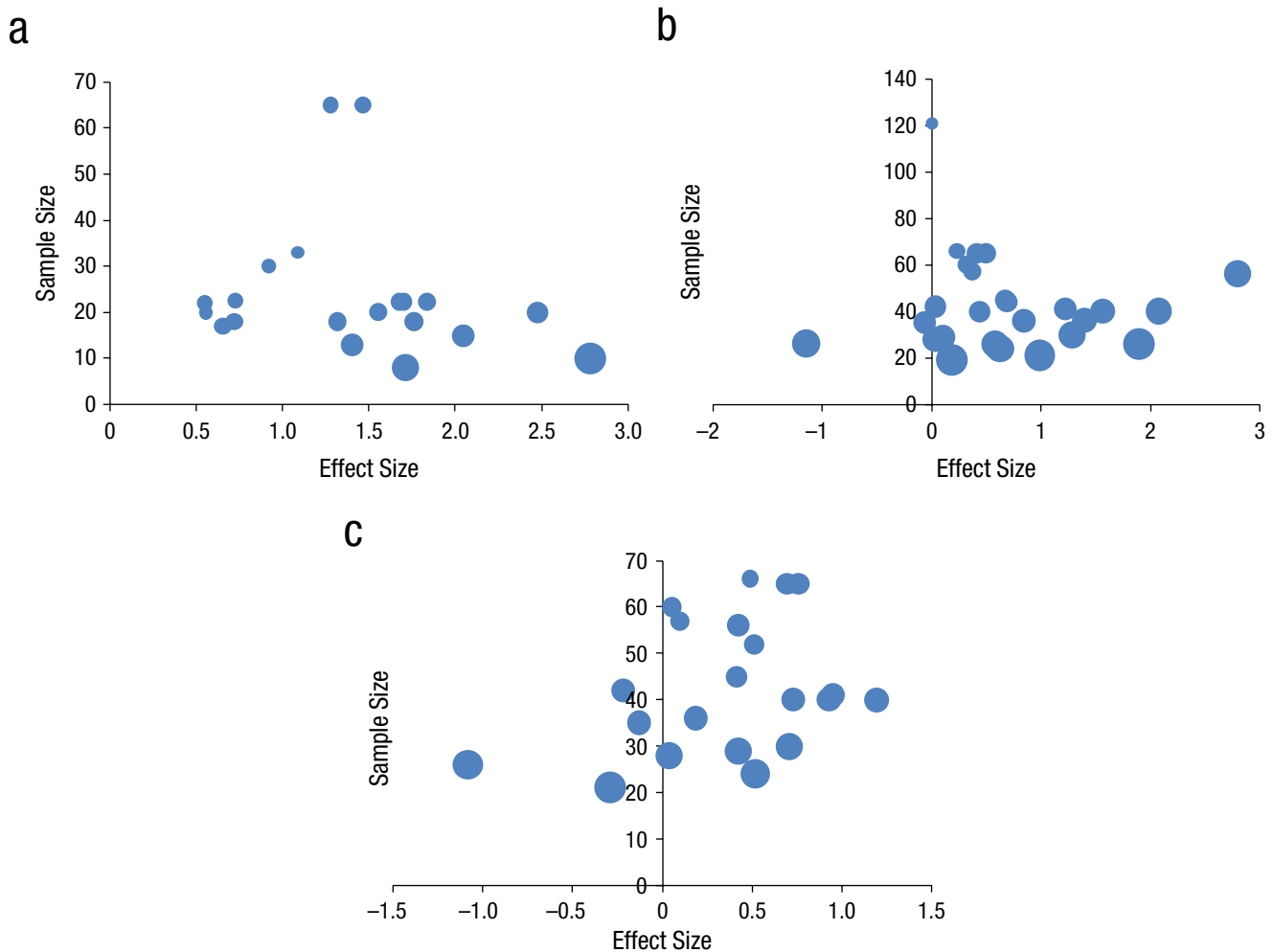
Fig. 3. Funnel plots for net treatment effects at posttest on (a) target measures, (b) near-transfer measures, and (c) far-transfer measures among older adults. Net treatment effect was operationalized as the difference between effect size in the training condition and effect size in the control condition (or conditions). Bubble size denotes 1/*SE*; thus, larger bubbles indicate more precise measurements.

those of either passive or active control treatment, not only for target measures, but for measures of near and far transfer as well. A second result is that the net effect of treatment did not vary reliably with the type of control condition (active, passive). Figure 4b breaks down the effects by the type of training (WM training, EF training). In this analysis, the training groups were compared only with the active control groups because the samples for a comparison with the passive control groups were extremely small. With the exception of the effect of EF training on far-transfer measures, which was marginally significant (lower limit of the 95% CI was −0.013, two-tailed p = .063), all net treatment effects were significant, and the two types of training did not differ in their effects.

Discussion

The main goals of this meta-analysis were (a) to test the extent of cognitive benefits (including near and far

transfer) of process-based cognitive training in older adults and (b) to investigate age-related differences in training and transfer effects between younger and older adults.

The results regarding training improvements in older adults are clear: First, WM and EF training led to significant and large improvements in performance of the trained tasks. The raw gain was about 0.9 *SD* (Fig. 2a); the net gain after subtracting the effects of active control treatment was about 0.5 *SD*, and the net gain after subtracting the effects of passive control treatment was about 0.8 *SD*. The net treatment effect at posttest after correction for publication bias was about 1.1 *SD* (Fig. 4a). Second, WM and EF training resulted in clear and quite large near-transfer effects in older adults. (One could consider these the effects of training at the level of the latent variable.) The raw gain was about 0.7 *SD* (Fig. 2a; or 0.5 *SD* after statistical removal of publication bias), and the net gain after subtracting the effects of control
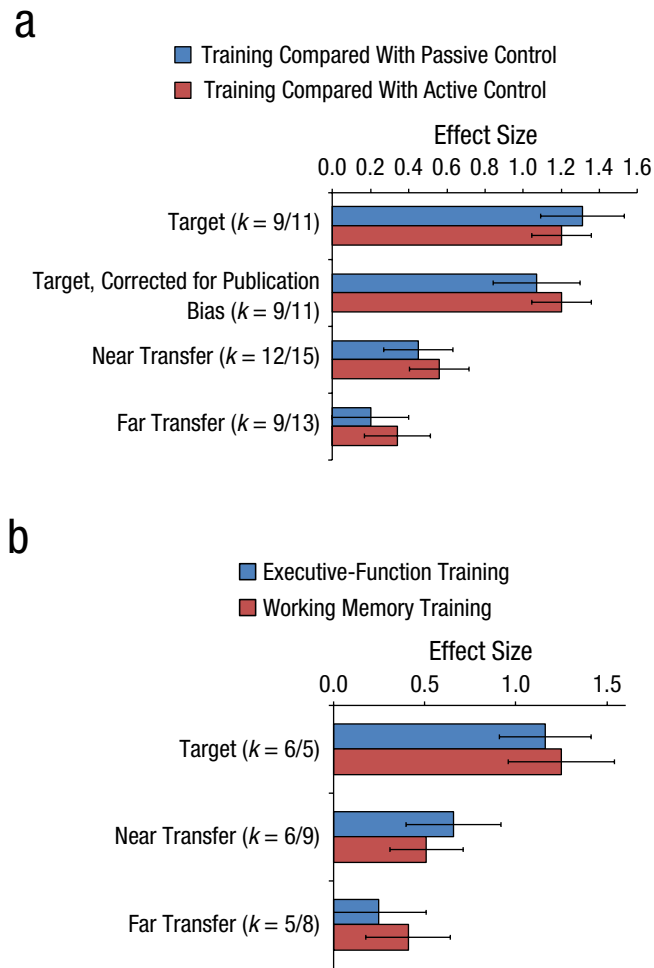
a

b

Fig. 4. Average net treatment effect at posttest for older adults. The graph in (a) shows effect sizes by type of measure (target, near transfer, and far transfer), separately for the comparison between the training and passive control treatments and the comparison between the training and active control treatments. Note that results for target measures both before and after correction for publication bias are shown. The graph in (b) shows effect sizes for executive-function (EF) training and working memory (WM) training compared with active control treatment, separately for the three types of measures. In both panels, error bars denote 95% confidence intervals. For each set of bars, the number of studies, *k*, that contributed effect sizes is shown in the order in which the bars are presented on the graph (top to bottom).

treatment was about 0.5 *SD* (or 0.3 *SD* after statistical removal of publication bias); the net treatment effect at posttest was about 0.5 *SD* (Fig. 4a). Third, WM and EF training resulted in clear but smaller far-transfer effects in older adults. The raw gain was about 0.4 *SD* (Fig. 2a), and the net gain after subtracting the effects of control treatment was about 0.2 *SD*; the net treatment effect at posttest was about 0.3 *SD* (Fig. 4a).

Note that the evidence for transfer in older adults is not completely unambiguous under two-tailed testing assumptions. The net effect on gain scores was

significant in three of the four relevant comparisons (i.e., near and far transfer relative to active and passive control treatments, respectively); the two-tailed *p* value for the one nonsignificant effect was .056. Similarly, the net effect at posttest was significant in all but one comparison for far transfer; only the difference between EF training and the active control treatment was marginal, two-tailed *p* = .063. (If one accepts a one-tailed logic—which seems defensible—all effects involving far transfer were significant.) Of particular interest is the finding that gain on measures of fluid intelligence was not negligible (0.35 *SD*; Fig. 2d), which suggests that process-based training generalizes to tasks that are potentially extremely relevant for daily functioning. (Hindin & Zelinski, 2012, and Karr et al., 2014, reported similar effects in their meta-analysis; however, they did not make an explicit distinction between near and far transfer, and they included multidomain training groups or samples with cognitive impairments in their analyses.)

We note that these results for transfer effects in older adults are seemingly at odds with other, qualitative literature reviews on transfer effects in younger adults (e.g., Shipstead et al., 2012). Such qualitative reviews have relied, implicitly or explicitly, on vote-counting procedures; that is, they have focused on the proportion of studies that yielded a statistically significant effect. In our analysis, we found that the net gain for far-transfer measures was about 0.2 *SD* in older adults. Power to detect such a small effect with a typical sample size of about 20 subjects is only 16%; conversely, an effect of this size needs a sample of 310 subjects to be detectable with a power of 80%. Most studies in the field are thus seriously underpowered, and vote-counting methods for data pooling will underestimate the effect greatly. Our results are also inconsistent with recent meta-analyses suggesting that WM training does not yield significant transfer (Melby-Lervåg & Hulme, 2013) and that training and transfer effects are largest in very young age groups (i.e., infants; Wass, Scerif, & Johnson, 2012). It should be noted, however, that these studies are not easily compared with ours because they included (a) participants across very wide age ranges, from preschoolers to old adults; (b) both normally developing and clinical samples (e.g., people with attention-deficit/hyperactivity disorder, brain injury, and schizophrenia); and (c) many different types of training, such as strategy-based, process-based, and multidomain training. Thus, the benefits of process-based WM and EF training that we found in our meta-analysis may have been masked in these previous studies.

We argue that the process-based interventions summarized here fare very well compared with other known treatments aimed at improving cognition in older adults. First, meta-analyses of the literature have shown that the

benefits of two promising types of training (mnemonic strategies—Verhaeghen et al., 1992; cognitive speed—Verhaeghen, 2014) do not generalize to untrained measures; the benefits of EF and WM training, however, clearly do. Second, the one meta-analysis on the effects of fluid-ability training (e.g., figural reasoning; Verhaeghen, 2000) showed that this type of training did not yield effects that were reliably larger than those of retest control treatment; effects of EF and WM training, in contrast, reliably exceed those of control treatments. Third, in their meta-analysis, Colcombe and Kramer (2003) observed that aerobic-exercise training was associated with a gain of 0.5 *SD* in cognition, whereas the gain after control treatment was 0.2 *SD* (i.e., training was associated with a net gain of 0.3 *SD*). The fairest comparison with our own data would be either to near-transfer effects (net gain of 0.5 *SD*) or to far-transfer effects (net gain of 0.2 *SD*). The net gain in cognition (0.4 *SD*, averaged over near and far transfer) after (on average) 9 hr of EF or WM training is thus comparable in size to the effect observed after (on average) about 5 months of (presumably daily) 45-min sessions of aerobic training.

Our second question pertained to possible age effects in the benefits of cognitive training. Put succinctly, none were found. This finding goes against the magnification effects (i.e., smaller effects for older than for younger participants) often found for strategy training (for an early meta-analysis, see Verhaeghen & Marcoen, 1996), possibly because the correct implementation of complex strategies depends on intact cognitive resources. Even though our analyses of age effects included a relatively small subset of studies, they suggest that prolonged practice with a task results in comparable gains for younger and older adults, a conclusion in line with a recent meta-analysis on practice effects in other elementary tasks, namely, choice reaction time, serial reaction time, memory scanning, and visual search (Verhaeghen, 2014).

One additional finding was the absence of a dose-response relationship on target and near-transfer measures (i.e., total time in training was not a significant predictor of these measures; cf. Karr et al., 2014). One possible explanation is that researchers, by skill or sheer luck, tend to provide just the right amount of practice. Another, perhaps more likely, explanation is that other factors, such as the specific type of treatment or the population trained, overshadow the effects of length of treatment.

Finally, there are a few issues that could not be addressed by the present meta-analysis—and that we would like to offer as suggestions for further study. First, little is known about the durability of training effects. Even though the longevity of training-induced gains is considered a key measure of the value of an intervention, results of follow-up assessments are not consistently reported, and the time intervals for these assessments vary from a few weeks up to several years (e.g., Willis et al., 2006). Second, although process-based training reliably and positively affects fluid intelligence, which is presumably correlated with real-life cognition, there are no data on the generalizability of the effects of WM or EF training to daily life (for the long-term effects of fluid-ability training on everyday functioning, see Rebok et al., 2014). Third, a deeper study of individual differences in the effectiveness of training, and especially in the likelihood of eliciting transfer effects, would be desirable (see Jaeggi, Buschkuehl, Shah, & Jonides, 2014; Titz & Karbach, 2014; Zinke et al., 2014). Finally, more studies of the effects of process-based training on brain structure and function would be desirable as well. The few existing neuroimaging studies assessing plasticity in the aging brain have yielded heterogeneous findings, providing evidence for training-induced structural changes, but also both training-related increases and training-related decreases in cortical activity. These activation changes are thought to reflect shifts in strategy or processing and increased neural efficiency, respectively (Lustig et al., 2009).

In summary, we found that process-based EF and WM training in old age is highly effective, leading to reliable small to medium-sized transfer effects on both the latent construct trained and the wider cognitive system. No age differences in this form of plasticity were observed. These results suggest that EF and WM training might be useful tools for cognitive intervention in at least normal old age.

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