Cognitive and Working Memory Training

Perspectives From Psychology, Neuroscience, and Human Development

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Review of the Evidence on, and Fundamental Questions About, Efforts to Improve Executive Functions, Including Working Memory

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Introduction

Efforts to improve executive functions (EFs)—which include selective attention, self-control, working memory (WM), cognitive flexibility, and reasoning—to remediate deficits, improve academic performance, improve productivity, increase the likelihood of healthy choices and quality of life, and head off, slow, or reverse cognitive decline during aging. This systematic review is the most extensive review to date of interventions, programs, and approaches that have tried to improve EFs. Previous reviews have focused on one type of intervention, for example, the large literature cognitive training approaches to improving EFs or on physical-activity approaches to improving EFs. These reviews have also often concentrated only on children or only on adults. The review here looks at all the different methods that have been tried for improving EFs and at all ages.

In total, 179 studies (reported across 193 papers) from all over the world (North and South America, Europe, South and East Asia, the Middle East, and Oceania) are included. If a study a) evaluated a method to improve EFs, b) was published in English in a peer-reviewed journal by or before 2015, c) had at least one objective EF outcome measure, d) had least eight people per group, e) included a control group and compared EF improvement and/or posttest performance in the experimental and control groups, f) was not simply correlational, and g) involved more exposure to the approach or program than a single session, it is reviewed here. Since our primary focus is normal development and aging, we excluded all studies of participants with brain damage or dementia. We included studies with persons with attention deficit hyperactivity disorder (ADHD), since ADHD is primarily a problem with EFs, and a small random sampling of studies of individuals with other clinical conditions, such as depression or autism, or individuals who had a learning disorder. Tabulations were done both excluding results for clinical populations and including them.

The findings reveal some surprises. Perhaps the biggest surprise is that a relatively understudied approach—**mindfulness practices involving movement** (Chinese mind-body practices, taekwondo, t'ai chi, and Quadrato Motor Training)—yielded the strongest results for improving EFs.[1] Mindfulness practices involving movement produced the best results for improving EFs across all four different metrics we used for judging strength of EF benefits. When results were taken as reported, even including potentially spurious ones, mindful movement practices still produced the best results on two of the four metrics (see Table 8.1). Table 8.2 omits studies where positive results might not have survived the needed corrections for multiple comparisons or data analyses reflecting the level at which they randomized. These results are far better than those for any other approach to improving EFs. Often, initial findings look strong but then do not hold up in subsequent studies, so there is a chance that this category looks strongest because of the relatively small number of studies that have investigated it thus far. However, right now, all eight studies of mindful movement practices (100%) have found at least suggestive evidence of EF improvement. No other approach to improving EFs can claim that.

Tables 8.1 and 8.2 report results across our four metrics for 13 of the types of interventions we investigated. This review also looks at neurofeedback, combinations of aerobic exercise with other things, and programs using drama, music, photography, quilting, or Experience Corps®, but there were too few studies of each of those to include them in Tables 8.1 or 8.2.

In Table 8.1, **promising school programs** comprise the only approach to come in first or second on all four metrics. In Table 8.2, promising school programs comes in second every time, behind mindful movement practices. *Both approaches show results superior to those for all cognitive training interventions targeting EFs.* School programs have produced much better results for improving inhibitory control than any other approach. That is important because inhibitory control seems to be the EF most predictive of long-term outcomes.

Public school programs targeting EF skills are able to reach more children, more economically, and more fairly (in that ability to pay is irrelevant) than any other approach to improving EFs. When EF training is embedded in activities throughout the school day, children are challenged on diverse EFs under

[1] Yoga forms its own category in our review because there were a sufficient number of studies of yoga to make that possible. EF benefits from yoga have generally been disappointing, although a few studies found outstanding results. It is unclear why there is such a discrepancy across studies, but it might have to do with how yoga was taught (as a mindfulness practice or just as a physical activity) and/or characteristics of the instructor.

Table 8.1. Summary of Results for All EFs Assessed (Including Reasoning/Fluid Intelligence) Across All Program and Intervention Types

<table>
<thead>
<tr><th></th><th colspan="2">Percent of Studies Finding Even Suggestive[1] Evidence of EF Benefits (# of Studies)</th><th colspan="2">Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of Studies)</th><th colspan="2">Percent of EF Outcome Measures on which Experimental Group Improved More Than Control Group (# of Measures)</th><th colspan="2">Percent Of EF Measures on which Experimental Group Performed Better at Posttest Than Control Group (# of Measures)</th></tr>
</thead>
<tbody>
<tr><td>Cogmed Training</td><td colspan="2">60% (15)</td><td colspan="2">23% (13)</td><td colspan="2">42% (138)</td><td colspan="2">28% (104)</td></tr>
<tr><td>N-back Training</td><td colspan="2">46% (13)</td><td colspan="2">31% (13)</td><td colspan="2">24% (93)</td><td colspan="2">20% (91)</td></tr>
<tr><td>Computerized Complex-Span Training</td><td colspan="2">25% (4)[3]</td><td colspan="2">0% (4)</td><td colspan="2">27% (30)</td><td colspan="2">24% (29)</td></tr>
<tr><td>Task-Switching Training</td><td colspan="2">20% (5)</td><td colspan="2">0% (5)</td><td colspan="2">47% (51)</td><td colspan="2">24% (42)</td></tr>
<tr><td>Other Computerized Cognitive Training (including commercial products)[4]</td><td colspan="2">44% (27)</td><td colspan="2">13% (24)</td><td colspan="2">29% (223)</td><td colspan="2">13% (196)</td></tr>
<tr><td>Noncomputerized Cognitive Training</td><td colspan="2">67% (12)</td><td colspan="2">20% (10)</td><td colspan="2">45% (74)</td><td colspan="2">30% (60)</td></tr>
<tr><td>Plain Aerobic Exercise</td><td>31% (16)</td><td rowspan="2">43%(35)</td><td>6% (16)</td><td rowspan="2">7%(30)</td><td>17% (70)</td><td rowspan="2">27%(151)</td><td>11% (64)</td><td rowspan="2">13%(111)</td></tr>
<tr><td>Aerobic Exercise with Cognitive and/or Motor Skill Demand[5]</td><td>53% (19)</td><td>7% (14)</td><td>36% (81)</td><td>15% (47)</td></tr>
<tr><td>Resistance Training</td><td colspan="2">22% (9)</td><td colspan="2">0% (8)</td><td colspan="2">25% (36)</td><td colspan="2">7% (30)</td></tr>
<tr><td>Yoga</td><td>43% (7)</td><td rowspan="2">73%(15)</td><td>14% (7)</td><td rowspan="2">51%(55)</td><td>38% (32)[6]</td><td rowspan="2">51%(55)</td><td>23% (35)</td><td rowspan="2">31%(51)</td></tr>
<tr><td>Mindfulness Practices Involving Movement (other than yoga)</td><td>100% (8)</td><td>29% (7)</td><td>70% (23)</td><td>50% (16)</td></tr>
</tbody>
</table>

(continued)

Table 8.1. Continued

	Percent of Studies Finding Even Suggestive[1] Evidence of EF Benefits (# of Studies)	Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of Studies)	Percent of EF Outcome Measures on which Experimental Group Improved More Than Control Group (# of Measures)	Percent Of EF Measures on which Experimental Group Performed Better at Posttest Than Control Group (# of Measures)
More Sedentary Mindfulness Practices	61% (23)	17% (23)	36% (91)	30% (96)
Promising School Programs[7]	75% (8)	57% (7)	61% (28)[8]	53% (38)

Note. There were too few studies in any of the following categories to include them here, although they appear in Tables 8.3 and 8.4 and are discussed in the chapter: interventions that combined aerobic exercise with other interventions, neurofeedback, theater, piano, photography, quilting, and Experience Corps.

[1] Suggestive = more EF improvement or better EF posttest performance than control group on ≥ 50% of measures.

[2] Clear = more EF improvement and better EF posttest performance than control group on ≥ 67% of measures. Whenever a study reported ≥ 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations here because it is possible the results of the study might have met our criteria for "clear" had the results not reported been included.

[3] Six complex-span training studies are included in the review. Two were noncomputerized and are included under "noncomputerized training" in Table 8.1 rather than under computerized complex-span training.

[4] Other Computerized Cognitive Training includes both interventions classified as miscellaneous computerized cognitive training and commercial computerized cognitive training products, including the noncommercial BrainGame Brian.

[5] If the FITKids studies are counted as three separate, independent studies, then for enriched aerobic exercise, the results would be 52% (21) for suggestive evidence, 6% (16) for clear evidence, 35% (91) for improvement, and 14% (57) for posttest.

[6] One yoga study did not do pretesting.

[7] Included in the Promising School Programs category are the following school programs: Attention Academy, Chicago School Readiness Program (CSRP), MindUP, Montessori, PATHS, and Tools of the Mind.

[8] Two studies of School Programs did not do pretesting.

Table 8.2. Summary of Results for EFs Assessed (Including Reasoning/Fluid Intelligence) Across All Program and Intervention Types, Omitting Studies Whose Positive Results Might Not Have Held up Had They Corrected for Multiple Comparisons or Conducted Data Analyses Reflecting the Level at Which They Randomized

	Percent of Studies Finding Even Suggestive[1] Evidence of EF Benefits (# of Studies)	Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of Studies)	Percent of EF Outcome Measures on which Experimental Group Improved More Than Control Group (# of Measures)	Percent of EF Measures on which Experimental Group Performed Better at Posttest Than Control Group (# Of Measures)
Cogmed Training	54% (13)	27% (11)	36% (103)	28% (69)
N-back Training	30% (10)	30% (10)	18% (72)	18% (72)
Computerized Complex-Span Training	33% (3)	0% (3)	30% (10)	22% (9)
Task-switching Training	20% (5)	0% (5)	47% (51)	24% (42)
Other Computerized Cognitive Training (including commercial products)[3]	45% (22)	10% (20)	33% (145)	14% (125)
Noncomputerized Cognitive Training	67% (12)	20% (10)	45% (74)	30% (61)
Plain Aerobic Exercise	31% (16) } 41%(34)	6% (16) } 7%(29)	17% (70) } 26%(145)	11% (64) } 13%(105)
Aerobic Exercise with Cognitive and/or Motor Skill Demand	50% (18)	8% (13)	33% (75)	17% (41)
Resistance Training	22% (9)	0% (8)	25% (36)	7% (30)
Yoga	20% (5) } 60%(10)	20% (5) } 33%(9)	16% (19) } 40%(30)	14% (22) } 26%(27)
Mindfulness Practices Involving Movement (other than yoga)	100% (5)	50% (4)	82% (11)	80% (5)

(continued)

Table 8.2. Continued

	Percent of Studies Finding Even Suggestive[1] Evidence of EF Benefits (# of Studies)	Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of Studies)	Percent of EF Outcome Measures on which Experimental Group Improved More Than Control Group (# of Measures)	Percent of EF Measures on which Experimental Group Performed Better at Posttest Than Control Group (# Of Measures)
More Sedentary Mindfulness Practices	59% (22)	18% (22)	38% (86)	28% (85)
Promising School Programs[4]	67% (6)	40% (5)	53% (19)	52% (25)

Note. There were too few studies in any of the following categories to include them here, although they appear in Tables 8.3 and 8.4 and are discussed in the chapter: interventions that combined aerobic exercise and other things, neurofeedback, theater, piano, photography, quilting, and Experience Corps.

[1] Suggestive = more EF improvement or better EF posttest performance than control group on ≥ 50% of measures.

[2] Clear = more EF improvement and better EF posttest performance than control group on ≥ 67% of measures. Whenever a study reported ≥ 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations here because it is possible the results of that study might have met our criteria for "clear" had the results not reported been included.

[3] Other Computerized Cognitive Training includes both interventions we classified as miscellaneous computerized cognitive training and commercial computerized cognitive training products, including the noncommercial BrainGame Brian.

[4] Included in the Promising School Programs category are the following school programs: Attention Academy, Chicago School Readiness Program (CSRP), MindUP, Montessori, PATHS, and Tools of the Mind.

very diverse circumstances. That is important for improvement on multiple EFs and for being able to generalize skills to novel situations. School programs are also able to provide greater doses, frequency, and duration than most other approaches to improving EFs. The data suggest that this combination of a great deal of training and practice under diverse circumstances pays off.

Despite much hype in the popular press and even some influential reviews in high-profile journals, there is a glaring lack of evidence that interventions tried thus far of **resistance training** or **aerobic exercise** consistently improve EFs. Across all the different methods investigated thus far for improving EFs, only resistance training and "plain" aerobic exercise (e.g., running or brisk walking) fall in the bottom half on *all* four measures we used to assess intervention efficacy in both Tables 8.1 and 8.2. (Results are slightly better for aerobic exercise with more cognitive or motor-skill challenges. It shows better results than plain aerobic exercise on three of the four metrics, with comparable results on the fourth. However, it still falls in the bottom half of interventions on three of the four metrics.) *No study of resistance training* and only two studies each of plain aerobic exercise and aerobic exercise with more cognitive or motor-skill challenges found clear evidence of EF benefits. Across all EF outcome measures, participants in resistance training or plain aerobic exercise improved more than control participants on only 17% to 25% of the measures. Compare that to mindfulness movement practices, task switching, or promising school programs, where across all EF outcome measures the experimental group improved more than the control group on 82%, 48%, and 53% of the measures, respectively (see Table 8.2). These results probably reflect how these types of physical-activity interventions have been structured rather than that aerobic activity does not benefit EFs. Persons who are more physically fit and people who spend more time doing physical activity consistently show better EFs. Engaging in physical activity might be driving EF benefits in ways that most intervention studies have not been capturing. (Hypotheses about that are offered in this chapter.)

Another approach that has received less media attention, **noncomputerized cognitive training**, looks potentially promising. Of the 13 approaches listed in Tables 8.1 and 8.2, it ranked third. It fell in the top 50% of programs on all four metrics in both Table 8.1 and Table 8.2. *Noncomputerized cognitive training has produced better EF results than any type of computerized cognitive training.* Across all studies of noncomputerized cognitive training, 67% report at least suggestive evidence of EF benefits, but only a few of those studies used blinded assessment. Note that all three approaches producing the best EF results involve more in-person interaction than computerized cognitive training. Perhaps some of the success of noncomputerized training has to do with the greater degree of instructor–trainee interaction when training is not computerized. On the other hand, perhaps there is just more room for unintentional biases of the trainers to affect the results when the training is not computerized.

Despite much fuss about possible benefits of **N-back training** for improving fluid intelligence, *only one N-back training study* with an active control group (out of six) found more improvement or better posttest performance on any measure of fluid intelligence in participants compared with control subjects. Compared to no-treatment control groups results look better, but still less than half of N-back studies found evidence of any benefit to fluid intelligence.

The computerized training approach most successful at improving EFs is Cogmed®. It ranked in the top 50% of programs on all four metrics in both Table 8.1 and Table 8.2, the only computerized method to do so. It is the only method to consistently show *sustained* near-transfer benefits. Benefits to WM from Cogmed have been shown to last for 3 to 6 months and even for a year. Benefits from Cogmed are narrow, though, extending only to the aspects of WM trained and perhaps some aspects of attention. Cogmed is marketed as being beneficial to children with ADHD, yet its generalization to ADHD symptomatology has not been confirmed by blinded observers or objective measures.

Results from three different studies suggest that the mentoring component of Cogmed may play a greater role in Cogmed's benefits than people have appreciated. The control version of Cogmed (where difficulty does not increase) also includes interaction with mentors, but it usually produces less benefit than the standard, adaptive version of Cogmed. Is mentoring then irrelevant to the benefits or might the mentors not expect similar benefits from the control condition? Interacting with an adult who believes in the efficacy of the training and expects you to improve is probably critical.

In all age groups, cognitive training, both computerized and noncomputerized, improves the cognitive skills on which one trains. There does not appear to be an age too young or too old. There is very limited evidence of transfer to untrained skills, however.

If someone has a specific deficit in WM (as can be common with aging), Cogmed or N-back training might be quite beneficial. There has been very little study of Cogmed with older adults, but WM deteriorates earlier and more severely during aging than most other cognitive skills. The few studies of Cogmed and N-back training with older adults suggest that such targeted cognitive training might be especially beneficial for that subset of the population.

It is clear that generally, sessions of 30 to 40 minutes (min) yield better EF outcomes than sessions shorter than 30 min, and that is true both for cognitive training and physical activity (although Quadrato Motor Training provides a notable exception). It is not clear, however, that even longer sessions yield better results. For aerobic exercise, the evidence suggests that sessions longer than an hour yield fewer benefits than sessions of 45 to 60 min (of which about 30–40 min is aerobic).

We predict that many activities not yet studied will likely improve EFs. We also predict that the way an activity is done and the human qualities of the mentors or trainers (such as how enjoyable they make the activity, their supportiveness, and their ability to communicate their unwavering faith in the participants and the program), as well as whether the activity is personally meaningful and relevant, inspiring a deep commitment and emotional investment from participants to the activity and to one another, will likely prove more decisive than what the activity is. We are impressed with the potential benefits of real-world activities, such as sports, theater, and Experience Corps®, that engender deep commitments, bring joy, build self-confidence and pride, challenge EFs, and build community. We would like to see more studies of these and other real-world activities, including more that are done outdoors in nature.

EFs certainly can be improved—at every age from infancy through old age. We are only at the beginning, however, of understanding what characterizes the approaches that are most successful and how success differs by type of approach, EF domain, and/or subject characteristics. We have hardly begun to explore how to make benefits generalize further and last longer. Much has been revealed about what works to improve EFs and what does not, but this is only the tip of the iceberg.

Executive Functions (EFs)

Before discussing the general principles that can be gleaned from the vast literature relevant to improving EFs, it is important to define EFs and to explain why it is important to try to improve them.

EFs (also called executive control or cognitive control) refer to a family of interrelated, top-down processes needed to concentrate and pay attention, when "going on autopilot" or relying on instinct or intuition would be ill-advised, insufficient, or impossible (Diamond, 2006, 2013; Espy, 2004; Hughes, 2005; Jacques & Marcovitch, 2010). There is general agreement that there are three core EFs (inhibitory control, WM, and cognitive flexibility; Diamond, 2013; Miyake et al., 2000; Lehto, Juujärvi, Kooistra, & Pulkkinen, 2003; Logue & Gould, 2013; see Figure 8.1). Using EFs is effortful. It is easier to continue doing what one has been doing than to change or to put thought into what to do next. It is easier to give into temptation than to resist it.

One core EF is inhibition (also called inhibitory control), under which are usually categorized both self-control (behavioral inhibition or response inhibition) and interference control (including selective attention [also called

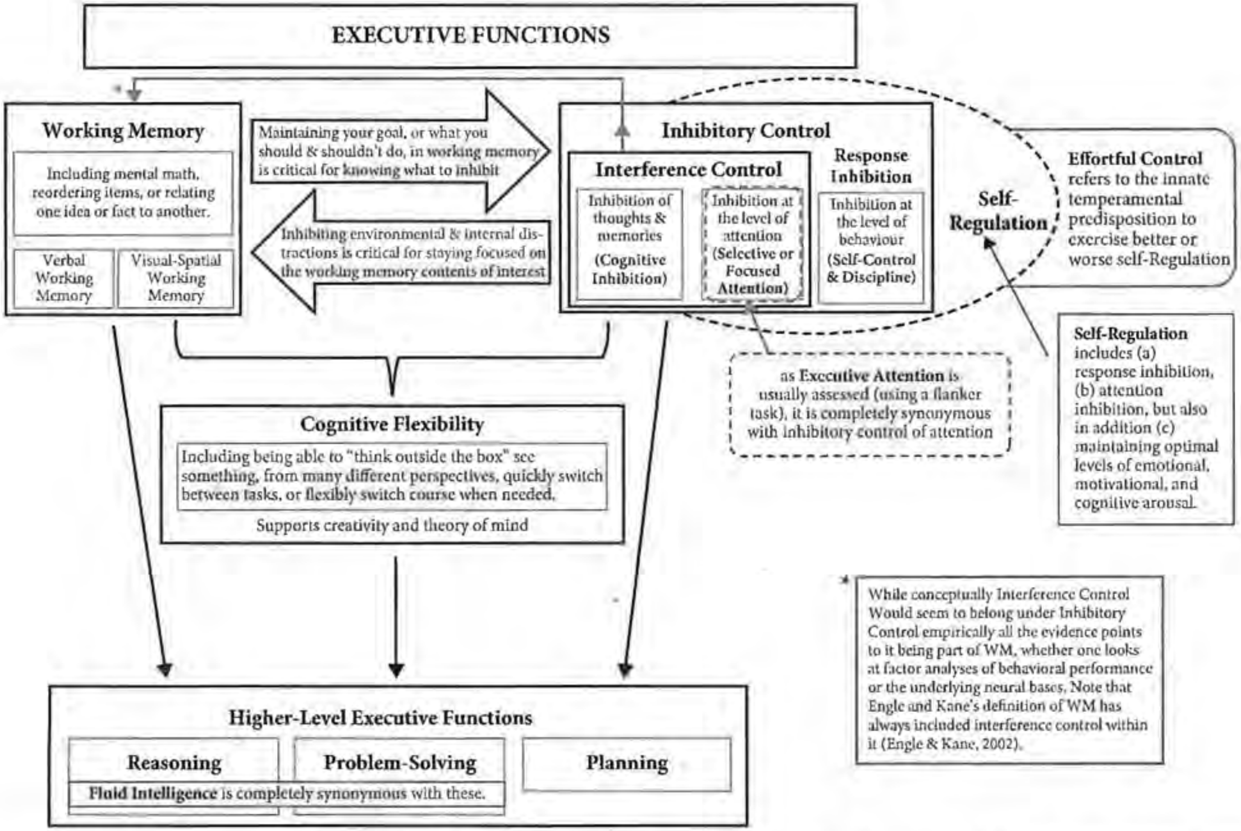

Figure 8.1. The components that together comprise executive functions (EFs) and the relation of EFs to other concepts. The two primary EFs are WM and inhibitory control, which together make cognitive flexibility possible. From the three core EFs, the higher order EFs of reasoning, problem-solving, and planning are built. Inhibitory control is usually thought to consist of response (or behavioral) inhibition and interference control, but there is increasing empirical evidence that interference control is more closely aligned with WM than with inhibitory control.

Reprinted from Diamond, A. (2016). Why assessing and improving executive functions early in life is critical. In P. McCardle, L. Freund, & J. A. Griffin (Eds.), *Executive Function in Preschool-age Children: Integrating Measurement, Neurodevelopment, and Translational Research* (pp. 11–43). Washington, DC: American Psychological Association.

executive attention or focused attention] and cognitive inhibition). Self-control involves control over one's behavior and control over one's emotions in the service of controlling one's behavior. Self-control is about resisting temptations and not acting impulsively. It is about suppressing a dominant response, or one's first inclination, and giving a more appropriate response instead. The strong inclination might be, for example, to reflexively strike back at someone who has hurt your feelings, blurt out an inappropriate remark, cut in line, or for a visitor from North America reflexively looking left when crossing the street in the United Kingdom (where looking right is the correct response). Importantly, self-control involves waiting before speaking or acting so that what comes out is a considered response rather than an impulsive one.

One aspect of interference control is inhibitory control of attention (interference control at the level of perception). It enables us to focus on what we choose and ignore distractions (Posner & DiGirolamo, 1998; Theeuwes, 1991). We need such selective attention at a cocktail party when we want to screen out all but one voice. Another aspect of interference control is cognitive inhibition—suppressing extraneous or unwanted thoughts or memories, resisting proactive interference from information acquired earlier, and resisting retroactive interference from items presented later (Anderson & Levy, 2009; Postle, Brush, & Nick, 2004).

WM, a second core EF, involves holding information in mind and mentally working with that information (Baddeley, 1992; Baddeley & Hitch, 1994, Cohen, Pearlstein et al., 1997; D'Esposito et al., 1995, 1998; Owen, Evans, & Petrides, 1996; Petrides, 1994; 1995; Smith & Jonides, 1999; Smith, Jonides, Marshuetz, & Koeppe, 1998). Translating instructions into action plans requires WM, as does updating one's thinking or planning, mentally re-ordering a to-do list, calculating a route, considering alternatives, or relating one piece of information to another. Two types of WM are distinguished by content—verbal WM and nonverbal (visuospatial) WM (Alloway, Gathercole, & Pickering, 2006; Baddeley, 1992).[2]

[2] The term *working memory* is not always used the same way. In this book, there is a chapter by Engle who uses WM to mean maintaining information in mind, sometimes also including manipulating information (e.g., Unsworth & Engle, 2007), although not always (e.g., Engle & Kane, 2004), but always including an element absent from the definition above—interference control (blocking or inhibiting other information from entering that active state; Engle & Kane, 2004). That is, Engle defines WM as holding information in mind (and perhaps also manipulating it) in the presence of interference that must be inhibited (Unsworth & Engle, 2007). As the note in Figure 8.1 indicates, empirical evidence is increasingly in support of the perspective of Engle and colleagues.

When WM is defined in this way, it also applies to holding information in mind while performing mental operations on other information (D'Esposito & Postle, 2015; Unsworth & Engle, 2007). An example of WM conceived of in this way would be holding in mind a question or comment you want to raise while you are trying to follow what others are saying.

Short-term memory (STM) involves just holding information in mind. WM also involves holding information in mind but while also performing mental operations. WM and STM cluster on separate factors in factor analyses of children, adolescents, and adults (Alloway, Gathercole, Willis, & Adams, 2004; Gathercole, Pickering, Knight, & Stegmann, 2004). They are linked to different neural subsystems. For example, WM relies more on dorsolateral prefrontal cortex (DLPFC), while maintaining information in mind but not manipulating it (as long as the number of items is not suprathreshold) does not require DLPFC (D'Esposito, Postle, Ballard, & Lease, 1999; Eldreth et al., 2006; Smith & Jonides, 1999). WM and STM also show different developmental progressions. STM develops earlier and faster (Diamond, 1995; Davidson, Amso, Anderson, & Diamond, 2006).

There are often misunderstandings about which tasks tap WM or STM. Forward Span tasks (which require recalling items in the order in which they were presented) assess STM because no manipulation of the information is needed. Re-ordering span tasks (mentally ordering the items according to some criterion, such as size) assess WM because they require manipulating the information presented. They are relatively pure tests of WM in the sense that they require little else besides WM.

N-back tasks, especially 2- or 3-back versions (e.g., "Press when you see an A two items after seeing an X" or "Press when any letter is repeated two items after that letter was shown"), require maintaining the ordinal position of items while continually entering incoming stimuli into WM, comparing them to new stimuli, and continually updating ordinal position and deleting old items so that one's WM capacity is not over-run. N-back tasks are more difficult WM tasks and require maintenance + manipulation + interference control. In addition, they require response inhibition when lures appear (e.g., an A one item after seeing an X), so they are not pure measures of WM. Further evidence that they do not require WM alone is that training on N-back tests may improve inhibitory control (attentional control and response inhibition) more than it improves WM (Jaeggi, Buschkuehl, Jonides, & Shah, 2012).

Complex-span tasks (e.g., counting span, reading span, and operations span) require holding in mind a piece of information from each previous trial in chronological order while performing the required mental operation on each trial. For example, the counting span task requires, on each trial, counting out loud all the blue dots (ignoring the yellow ones) and then announcing in chronological

One example of "the ability to keep a representation active, particularly in the face of interference and distraction" (Engle, Tuholski, Laughlin, & Conway, 1999, p. 309), i.e., maintenance plus interference control, would be spatial span tasks with a masking stimulus. For example, several boxes in a grid might be simultaneously illuminated momentarily (a masking stimulus) on each trial between presentation of a sequence of boxes in the grid being illuminated and when the subject can respond by touching the boxes in the order in which they were illuminated.

order the totals for blue dots on all previous trials. These are difficult tasks and clearly require maintenance plus manipulation. The problem is that they can also require inhibition (e.g., on the counting span, selectively attending to the blue dots and ignoring the yellow ones) and always require task switching (e.g., switching from the task of counting or reading to the task of reciting ordered information held in mind). Diamond has argued that complex-span tasks might more properly be considered measures of EFs in general, rather than WM in particular (Diamond, 2013).

The third core EF is cognitive flexibility (also called mental flexibility, set shifting, or task switching). One aspect of cognitive flexibility is being able to change perspectives, being able to see something from different perspectives. For example, from one perspective, the A and C in the string A*C1*5 go together because they are both letters. From another perspective, the *C* and *1* go together because they are both in italics. From yet another perspective, the A and the *1* go together because they are both first in their respective series. The ability to see different perspectives could also come into play in a situation in which two people seemingly share little in common, perhaps because they have different religious beliefs or political affiliations, but from other perspectives they might share commonalities in their taste in music or commitment to social justice.

Another aspect of cognitive flexibility involves changing how you think about something ("thinking outside the box"). If there's a problem no one has been able to solve, one might think outside the box to conceive of the problem, frame the problem in a new way, or come up with a new way of attacking it. Cognitive flexibility enables us to flexibly adjust to changing demands or priorities, take advantage of a sudden, unexpected opportunity (serendipity), and overcome sudden, unexpected problems.

From these three core EFs, higher-order EFs, such as reasoning, problem-solving, and planning, are built (Collins & Koechlin, 2012; Lunt et al., 2012; see Figure 8.1). The family of EFs depend on prefrontal cortex and other neural regions (especially the anterior cingulate cortex and parietal cortex) with which prefrontal cortex is interconnected (Aron, Behrens, Mith, Frank, & Poldrack, 2007; Cole & Schneider, 2007; Eisenberg & Berman, 2010; Leh, Petrides, & Strafella, 2010; Niendam et al., 2012; Zanto, Rubens, Thangavel, & Gazzaley, 2011).

Why It Is Important to Improve EFs

EFs are necessary in our ever-evolving world. Self-control is vital for a civil society where people abide by rules and norms, resisting temptations not to do so. Without inhibitory control, we would be at the mercy of impulse, old habits of thought or action (conditioned responses), and environmental stimuli that pull us this way or that. Thus, inhibitory control makes it possible for us to change and to choose how to react and behave, rather than being "unthinking" creatures of habit. While it is by no means easy, it makes it possible for us to avoid saying or doing things we would later regret.

WM is critical for making sense of anything that unfolds over time, for that always requires holding in mind what happened earlier and relating it to what comes later. Note that anything that involves language (oral or written) unfolds over time. WM is also critical for reasoning and problem-solving because those require holding items in mind to see their interrelations and to explore novel recombinations. Cognitive flexibility is the core of critical and creative problem-solving.

All of life's aspects require the presence of mind to wait before speaking or acting, being able to resist impulsively reacting, staying focused despite distraction, seeing tasks through to completion although tempted not to, holding alternatives in mind so one can look at a situation from multiple perspectives (creatively coming up with new ways to attack problems), flexibly changing course when needed, and seizing opportunities when they unexpectedly arise. As societies become increasingly complex and fast-paced, having good WM and cognitive flexibility allows us to keep up with information processing, contribute to meaningful conversations, reason, solve problems, read critically, and see things from another person's perspective. This is especially important in an increasingly virtual, borderless world, where people from different backgrounds, cultures, political ideologies, languages, and beliefs interact with one another on a daily basis through the Internet.

Research confirms that EFs are critical for *school readiness* (they are even more critical than IQ or entry-level reading or math; Blair, 2002; Blair & Razza, 2007; Carlson & Moses, 2001; Hughes & Ensor, 2008; Kochanska, Murray, & Coy, 1997; Morrison, Ponitz, & McClelland, 2010), *success in school from the earliest grades through university* (Alloway & Alloway, 2010; Borella, Carretti, & Pelgrina, 2010; Duncan et al., 2007; Fiebach, Ricker, Friederici, & Jacobs, 2007; Hamre & Pianta, 2001; Loosli, Buschkuehl, Perrig, & Jaeggi, 2012; McClelland et al., 2007; Nicholson, 2007; Savage, Cornish, Manly, & Hollis, 2006; St Clair-Thompson & Gathercole, 2006), *career success* (Bailey, 2007), *making and keeping friends* (Hughes & Dunn, 1998), *marital harmony* (Eakin et al., 2004), and *good health* (Crescioni et al., 2011; Cserjési, Luminet, Poncelet, & Schafer, 2009; Hall, Crossley, & D'Arcy, 2010; Miller, Barnes, & Beaver, 2011; Moffitt et al., 2011; Perry et al., 2011; Riggs, Spruijt-Metz, Sakuma, Chour, & Pentz, 2010).

An influential meta-analysis questioned whether improving EFs improves academic achievement (Jacob & Parkinson, 2015) but that analysis included no intervention studies (just longitudinal and cross-sectional ones) and the authors did two things that would greatly reduce the size of any effect found. One, in their analyses of causal relationships, they excluded studies that did not control for IQ. However, fluid intelligence and EFs correlate about 0.8 to 1.0 (Conway, Kane, & Engle, 2003; Stauffer, Ree, & Caretta, 1996). Indeed, in one study of patients with neurological and neuropsychiatric conditions impairing EFs, when the effects of fluid intelligence were partialled out, no clinical deficit remained on EF measures (Roca et al., 2014). Most IQ tests now assess both crystallized and fluid intelligence; to the extent that controlling for IQ resulted in controlling for the fluid intelligence aspect of IQ, studies were essentially guaranteeing no effect of EFs. Second, in a school context, rarely does a program only target EFs and not academic skills. Jacobs and Parkinson set too high a bar in specifying that if a school program worked on both, then there's no evidence that training EFs per se helps. What one needs to ask is: If the program is during the regular school day, does targeting EFs *in addition to academic subjects* yield greater benefits than a comparable program that only targets academic subjects?

Intervention studies show that improving EFs does indeed improve academic performance. For example, when the Chicago School Readiness Project was delivered in Head Start preschools, it was able to improve the EFs of disadvantaged children by the end of that preschool year (Raver et al., 2008, 2011). Those children continued to perform better in math and reading than their peers who had attended regular Head Start for the next 3 years, and those academic gains were mediated almost entirely through improved EFs (Li-Grining, Raver, & Pess, 2011). Similarly, the Tools of the Mind curriculumdelivered in kindergartens in Massachusetts not only improved EFs by the end of kindergarten, but also improved reading, vocabulary, and math more than did regular kindergarten, and that difference in academic progress was still evident in first grade (indeed, for reading, it was only evident in first grade; Blair & Raver, 2014).

Graduating from high school has become increasingly important, especially with a competitive job market and high rates of unemployment. It is becoming more and more difficult for someone without a high school degree to find work (Friedman et al., 2007; Winstok, 2009). Children with poor EFs are far less likely to graduate from high school (Friedman et al., 2007; Friedman, Miyake, Robinson, & Hewitt, 2011; Moffitt et al., 2011), which ultimately has a major economic impact on them and the entire nation. The estimated cost savings to Canada if high school graduation increased by only 1% is more than \$7.7 billion (Hankivsky, 2008).

With a large sample of over 14,000 youths, Miller et al. (2011) found that those who had poorer inhibitory control were "exponentially more likely" to suffer from nine of the ten adverse health conditions they examined, including asthma, high cholesterol, high blood pressure, and cancer. People with better EFs generally enjoy a better quality of life (Archontaki, Lewis, & Bates, 2013; Brown & Landgraf, 2010; Davis, Marra, Najafzadeh, & Lui-Ambrose, 2010; Moffitt, 2012; Tangney, Baumeister, & Boone, 2004) and live longer (Hall et al., 2010).

In a study of 1,000 children born in the same city in the same year, thos with worse inhibitory control as children (those who were less persistent, more impulsive, and had poorer attention regulation)later as adolescents were more likely to smoke, have unplanned pregnancies, and drop out of school. As adults 30 years later, they were likely to earn less,have worse health three times more likely to be addicted to drugs),be a single parent (twice as likely), and commit more crimes (four times more likely to have a criminal record) than those with better inhibitory control as children, controlling for IQ, gender, social class, home lives, and family circumstances growing up (Moffitt et al., 2011). They were also less likely to be happy as adults (Moffitt, 2012).

Evidence shows that EF deficits often do not disappear on their own and can grow larger (Nigg et al., 2006; O'Shaughnessy, Lane, Gresham, & Beebe-Frankenberger, 2003; Riggs, Blair, & Greenberg, 2003). Early EF deficits can lead to a negative self-image and maladaptive behaviors that can be extremely difficult to correct (Broidy et al., 2003; Nagin & Tremblay, 1999). EFs during early childhoo often predict adult outcomes better than does IQ or SES (Moffitt et al., 2011). Research suggests that early EF gains can reduce the later incidence of school failure, substance abuse, addiction, aggression, crime, and other antisocial or inappropriate behaviors (Hall et al., 2010; Nagin & Tremblay, 1999; Olson, Smeroff, Kerr, Lopez, & Wellman, 2005; Vitaro, Barker, Brendgen, & Tremblay, 2012).

Being able to enhance EF development early in a child's life is critical because it affects the trajectory (the negative or positive feedback loop) on which a child is launched. Indeed, Moffitt et al. (2011) predicted "that interventions that achieve even small improvements in [the inhibitory control component of EFs] for individuals could shift the entire distribution of outcomes in a salutary direction and yield large improvements in health, wealth, and crime rate for a nation" (p. 2694).

Also, any approach that slows the decline of EFs with aging, or reverses it, would improve the quality of life for millions of people. EFs are the last abilities to fully mature during development and the first to start to deteriorate in adulthood (as early as one's forties; de Luca & Leventer, 2008). Poor WM and attention are among the most common complaints of older adults and among the cognitive problems that most negatively impact their lives (e.g., Park & Payer, 2000; Reuter-Lorenz et al., 2001). A survey of older adults in the United States found that their number-one health-related concern was not being "mentally sharp" (National Council on Aging, 2015). Heading off those problems, slowing their advance, or reversing the decline, even if only a little, could have major health and economic benefits.

Principles of Experimental Design and Principles for Interpreting Results Often Violated in Training or Intervention Studies

a. Suppose study participants were given a passage to read at Time 1 and thengiven the same passage to read at Time 2, and they read it faster at Time 2. If we had them wash dishes between Times 1 and 2, would we be justified in concluding that washing dishes improves reading speed? Of course not. As obvious as this seems, more than a few training studies display a similar logic.

To conclude that what individuals did between Times 1 and 2 produced the improvement at Time 2, evidence of differential improvement is needed (e.g., those who washed dishes improved more in reading speed than those who did not wash dishes, or those who spent more time washing dishes improved more in reading speed than those who spent less time washing dishes). Without differential improvement (i.e., a group X change interaction; Chang, Tsai, Chen, & Hung, 2013) we cannot know if what people did between Times 1 and 2 caused the improvement, or if the improvement was simply due to practice effects from having taken the tests before, or, in the case of children, from developmental changes.

Thus, a control group is essential. Studies that show improvements from Time 1 to Time 2 but have assigned all participants to the training group are fundamentally unconvincing. Finding that participants did better after an intervention than before is insufficient to determine that the intervention helped. Comparable improvements might have been seen without training.

b. If two groups in a study both show completely comparable improvement and performance, we cannot know why that was found. Similarly, if the two groups in a study both received the same training, but one had longer sessions or more sessions, yet both improved comparably, one cannot conclude that the training helped and that length of sessions or their duration did not matter. That may be the case, but it could also be that the training didn't help at all, regardless of whether participants got more or less of it; there is no way of knowing. To conclude that the training helped, a group that does not show comparable improvements is needed.

c. Another all-too-common problem with treatment studies involving young adults or children is that a greater benefit to the experimental group than to the control group is based primarily on the control group performing worse at posttest than they performed on the same measures at pretest, rather than the experimental groupshowing much improvement. For studies with older adults, where a decline might be expected, it might well be that the intervention reduced the rate of decline or stopped it. However, for children and young adults, it is very problematic if one group performs worse the second time they are tested. Such results should be viewed with caution, indeed skepticism.

d. If the control group started out performing better on outcome measures and at posttest the experimental group had caught up to the control group, it is difficult to conclude with any surety that the intervention helped, rather than what whe are seeing is regression to the mean or normal differences in developmental timetables. For example, children in the control group might have experienced a spurt in EF development before the study started (hence they were better at pretest) and the experimental group might have experienced a spurt in EF development during the time of the study, independent of the intervention. For example, if children in one group but not in another were walking at 9 months, and then by 15 months children in both groups were comparable in walking, that could easily be due to normal developmental processes rather than any program that those not walking at 9 months received. When Iit is impressive that an experimental group caught up to a control group is when the experimental group is a clinical population and the control group consists of a nonclinical population (such as typically developing children) because one would not expect such catch-up in the normal course of events without the aid of some program.

An absolutely superb review of computer-based cognitive training approaches (Simons et al., 2016) that came out after our review had been submitted to the publisher makes a similar point: "The control group should be comparable to the experimental group before treatment or the results of the intervention will be effectively uninterpretable—that is, differential gains after training could just reflect those different starting points (Redick & Webster, 2014)."

A successful intervention should ideally produce both of theseresults: (1) significantly more improvement in the experimental group than the control group from before to after the intervention (i.e., better change scores), and (2) significantly better postintervention performance by the experimental group than the control group (i.e., better posttest scores). If the degree of change is significantly better, but posttest performance is not significantly better than the control group, that often (although not always) means that the intervention group started off worse and caught up or the control group inexplicably, mysteriously got worse. If the degree of change is no different, but the experimental group showed better posttest performance than the control group, that could be because the experimental group simply mantained the advantage they started with. Thus, what is needed are significant group differences both for final test results and for degree of improvement on the tests.

e. Another all-too-frequent problem is for studies to have a no-treatment, or business-as-usual, group as their only control group. People often get a boost from a change, any change (e.g., the Hawthorne effect; McCarney et al., 2007) and from receiving attention (as when they are trained on something new). Therefore, people in the control group should also receive a new program and similar amounts of attention. (See Boot, Simons, Stothart, & Stutts, 2013, for a similar argument.)

To compare a program or intervention to no treatment sets a very low bar for determining whether an intervention really worked. Anything might be better than nothing, and anything new might produce better results than business as usual. It is still unclear what the optimal control group would be (see the section "Limitations" in the section "Discussion"), but it is almost universally agreed that a control group should be actively engaged in something new and different (i.e., it should be an active control group), and ideally control subjects should get the same amount of attention and have the same expectations for benefits. See Simons et al. (2016) for a similar, and even more strongly worded, exposition on this point.

f. A related point is that people often do better simply because they believe what they are doing will yield improvements—a placebo effect (Boot et al., 2013), or expectancies become self-fulfilling (Jenner, 1990; Rosenthal & Jacobsen, 1968). Accordingly, the expectation of success on the dependent measures should be as high among members of the control group(s) and those interacting with them as among the experimental group.

g. Ideally, neither the subjects nor program providers (the trainers) should know which group is the experimental one and which is the control group (i.e., they should be blind to whether they are part of the group expected to show the most benefits or not).

h. Testers and raters should also be blind to who is in the group expected to do better. In studies involving children, when the outcome measures are adult ratings of, or reports on, children, if those adults are not blind to the children's group assignments, the raters might be inclined to think they saw greater improvement in children in the intervention group because of the expectation that they would improve more. Alas, too often training studies that use adult ratings as an outcome measure use adults who are fully knowledgeable about which groups the children were in.

Just as the raters of participants' performance should be blinded, people administering the outcome measures should also be blinded to participants' group assignments or the study's hypotheses, or both, because their expectations that participants in one group will perform better can cause those participants to perform better when otherwise they would not (Kit, Mateer, Tuokko, & Spencer-Rodgers, 2014; Pfungst, 1911; Rosenthal & Jacobsen, 1968; Rydell, Van Loo, & Boucher, 2014).

Textbooks often cite the need for double blinding (neither the participants in the study nor the people testing or evaluating them afterward should know who was in the experimental group), but when an intervention is delivered by people, a third group should be blind to thisas well, and that is the people providing the intervention and the control conditions.

i. Spurious benefits can be seen if low performers drop out of the intervention group and higher performers remain. Be concerned if a study has a high attrition rate, especially since it usually indicates that something about the intervention was insufficient to keep people engaged (perhaps it was too boring or too demanding).

j. Although attrition is often reported, compliance rarely is (only 35% of studies reviewed here reported compliance). How often individuals attended the condition to which they were assigned is likely to affect how much they benefitted from that condition. *Studies should report data on compliance and should look at the relation between attendance and degree of benefit.*

k. Fidelity to the program by the people administering it is also important and is rarely monitored or reported. Two different people can administer the 'same' program in different ways and produce very different outcomes. Whether the people administering the program believe in the program and in the ability of participants in the program to succeed can also have important effects on results (Frank, 1961; Rosenthal & Jacobsen, 1968).

l. Too often, intervention studies have been underpowered, with too few participants per group.

m. Occasionally, interventions with a great many participants per group report only significance levels and not effect sizes. A study with a great many participants per group can find even the most trivial, minimal difference to be significant at $p \leq .05$. That is too low a bar.

n. Many intervention studies reviewed here that have used cluster-randomized designs (e.g., randomizing schools or classes to condition) have ignored the design when analyzing their data; they analyzed their data as if they had randomized individuals to condition. When this error in data analysis is made, many results reported as significant would not have reached significance had the appropriate data analysis been conducted. (A footnote in Column 1 of Tables 8.3 and 8.4 indicates when a study has done this.)

o. Several studies reviewed here that conducted multiple comparisons in their data analyses did not correct for that in their significance testing. When many comparisons are made, one might find a significant difference on one or more just by chance. Therefore, one needs to include a correction, such as dividing the 0.05 significance level by the number of comparisons made. (A footnote in Column 1 of Tables 8.3 and 8.4 alerts the reader when such a correction was not made for any study with ≥ 5 between-group comparisons on EF outcome measures. This was a relatively low bar to pass, as many would set the bar at simply 5 or more comparisons, not limiting it to just the outcome variables of interest, which in this case were EF measures.)

p. All too often, nontraining studies (correlational studies that look at whether, for example, people who exercise more have better EFs or whether students who play in the orchestra have better EFs than students who do not) are discussed as if they show causality (that more exercise or playing in the orchestra helps one to have better EFs). *Causal inferences from such studies are unjustified.* It could be that students with better EFs are more likely to choose to be in the orchestra or are disciplined enough to exercise more. For example, among adults who had been doing t'ai chi, aerobic exercise, or meditation for 5 years or more, at least three times a week, for at least 30 min per session, those who meditated or did t'ai chi, but not those who did aerobic exercise, showed significantly better cognitive flexibility (smaller switching costs) than sedentary controls (Hawkes, Manselle, & Woollacott, 2013). Alas, we cannot know if some characteristic that caused participants to select their particular activity was responsible for the difference in cognitive flexibility, rather than the activity itself.

q. If participants in a training study are free to choose which group they would like to be in, then that training study is vulnerable to the same criticism—persons who chose the training condition might have differed at the outset in a way important to the outcome. Thus, for example, Verghese et al. (2003) recruited participants who were ≥ 75 years old and were dementia-free, and who regularly did one or another leisure activity, such as social dancing or reading and crossword puzzles. Five years later, the researchers found that participants who had been doing social dance showed a significantly greater reduction in the risk for dementia than participants who did any other activity. Even though the participants were followed longitudinally, we cannot conclude that dancing caused the reduced risk of dementia, because participants who chose social dance may have differed from other participants in other ways that were responsible for the difference in their risk of dementia.

r. If participants have no say in their group assignment, however, and if the characteristics of participants in the different groups are carefully matched on variables thought to be potentially relevant to the outcome, there is no particular benefit to random assignment unless the number of participants is very large. Indeed, random assignment when dealing with small numbers (such as 20 or fewer per group) can lead to problematic differences across groups. Random assignment is only likely to ensure comparable groups when numbers are large. Stratifying randomization by participant variables potentially important to the results can help yield more comparable groups while still preserving the advantages of randomization. Remember, though, that even groups well matched on variables thought to be relevant might not be matched on a critical variable because the experimenter had not realized it might be important.

s. The other problem with assigning participants to condition is that some participants might not want to do the activity to which they are assigned and might do it reluctantly or actively resist doing it, thus reducing the size of the effect. One way to address this is to include only participants who express interest in the intervention and then randomly assign half to the intervention and half to an attractive alternative. It is sometimes argued that teachers do not typically have a say in which curriculum they teach, so random assignment accurately reflects teachers' lack of choice. However, teachers who might be opposed to a new experimental curricular change and implement it only half-heartedly in a study might be much more willing to adopt it later after evidence of its effectiveness has been demonstrated. Randomly assigning teachers to an unproven program for a research study may not accurately reflect how they would implement the same program after studies have found promising results using it.

t. Studies of the effects of physical-activity interventions too rarely gather data on participants' activities outside the intervention. For example, children might be involved in sports, take dance lessons, bike to school, or bike a paper delivery route, etc. Participants in physical-activity studies should be asked about their activities and, when possible, should be asked to wear a simple device that monitors their amount and intensity of physical activity.

Studies Included in this Systematic Review

To locate studies for review, we searched PubMed and PsycNET for all publications that had any keyword or word in the title or abstract from both of the following sets: Set 1—*evaluate, evaluation, intervention, program, randomized control trial, train*, or *training*; Set 2—*attention* (apart from ADHD), *cognitive control, cognitive flexibility, executive function, inhibition, inhibitory control, fluid intelligence, mental flexibility, reasoning, self-control, self-regulation, set shifting, task switching*, or *working memory*. For all relevant articles found, we searched the reference lists for additional relevant publications. The inclusion criteria for assigning a study to the review were:

1. The study had to include at least one EF outcome measure.
2. At least one of the EF measures had to be an objective behavioral measure. Studies that included *only* self-, parental, and/or teacher ratings or questionnaires were not included.

3. The study had to be done on humans.
4. The study had to look at benefits other than improvements on the task(s) practiced during the intervention. There is much evidence that if someone practices a task or procedure, that person gets better at that task. We were interested in whether there was improvement in a basic cognitive ability that generalized at least to similar tasks.
5. A report of the study had to be published in English in a peer-reviewed journal by, or before, 2015.
6. The study could not be purely correlational (e.g., looking at the EFs of people who happen to be doing something, such as meditating or exercising), because causality cannot be determined from correlations.
7. The study must have included a control group. Without a control group, it is not possible to determine if improvements would have been found even if participants had not received the intervention.
8. The study had to provide data that enabled us to compare the improvement and/or posttest performance of the experimental and control groups. We did not include studies, for example, that reported whether each of the groups individually showed EF improvements, but never compared the relative sizes of those improvements. The point of a control group is to be able to determine whether an effect might have occurred anyway, even without the specific intervention of interest. To see that both groups improved, but not to know whether the intervention group improved significantly more, does not enable us to answer that question.
9. The study must not have examined *only* acute effects (i.e., immediate benefits after doing something only once). Such immediate benefits are usually transitory, lasting only minutes.
10. The study must have had at least eight participants per condition.
11. The study must not have had a design problem so severe that it is impossible to draw any conclusion about the experimental condition (e.g., administering twice as many sessions to the experimental group as the control group).

We included all studies with healthy subjects or with subjects with an EF disorder, such as ADHD, that met the above criteria. Since we are primarily interested in normal development, we randomly selected 10% of the studies conducted with other patient populations (thus many studies of older adults with a diagnosed ailment are not included here). We excluded studies of persons with brain damage, stroke, severe cognitive decline, or dementia. In the Appendix, which appears online (URL: http://www.devcogneuro.com/tables/supplemental.html), we list for each broad category (e.g., cognitive training or physical activity) the many studies we considered, but excluded, along with the reasons why each of the studies was excluded (i.e., which inclusion criteria each of the excluded studies had not met).

From the 403 articles that met our search criteria, we found, 179 studies that met criteria for inclusion in this systematic review. Their results were reported in a total of 193 peer-reviewed, published research papers. Tables 8.3 and 8.4 summarize the study characteristics and cognitive outcomes found in the studies. Given their level of detail (Tables 8.3 and 8.4 are quite long [over 100 pages]), these tables are provided online only.

These tables provide rich detail so that readers can check our conclusions against the data and explore hypotheses of their own. These tables summarize all cognitive and academic outcomes (not just EF outcomes), but they do not include any other outcomes (e.g., social, emotional, behavioral, motor skills, physical fitness, or personality outcomes). We also do not report on improvements on the trained task (although if the authors mention a lack of improvement there, we mention that in the text). We only looked at improvements on untrained tasks, since we are interested in improvements in a cognitive ability rather than facilitation in the performance of a specific task. In the text, we primarily discuss EF outcomes.

Table 8.3 (appears in online: URL: http://www.devcogneuro.com/tables/supplemental.html) includes only studies that had an active control group. Table 8.3a provides details on the participants and methods. Table 8.3b provides the results for near transfer. Table 8.3c provides the results for far transfer. Results for EF outcome measures are always provided before results for other measures.

Table 8.4 (appears in online: URL: http://www.devcogneuro.com/tables/supplemental.html) includes studies that compared the experimental condition to only a no-treatment or business-as-usual control group, a lower bar for an intervention to pass to say an effect was significant. In general, studies in Table 8.4 included only two groups (the experimental condition and no treatment). The studies by Ball et al. (2002), Brown, Liu-Ambrose, Tate, & Lord (2009), Klusmann et al. (2010), and Mortimer et al. (2012) also appear in Table 8.4, although they included other groups, because their analyses compared intervention groups only to the no-treatment group, not to one another. Table 8.4a provides details on the subjects and methods. Table 8.4b provides the results for both near and far transfer. Results for EF outcome measures are always provided before results for other measures.

Principles That Govern EF Training, Whatever the Form

Principle 1

There is good news: EFs can be improved. Moreover, it appears that improving EFs is possible across the lifespan and by different methods. The many reviews all more or less agree on that point (see Table 8.5).

Table 8.5. Review Papers on Different Methods for Improving EFs

Topic Reviewed	Authors and Year of Review
Cognitive Training	Au, Buschkuehl, Duncan, & Jaeggi, 2016; Au et al., 2015; Baltes & Lindenberger, 1988; Buitenweg, Murre, & Ridderinkhof, 2012; Cortese et al., 2015; Karbach & Verhaeghen, 2014; Kelly et al., 2014; Klingberg, 2010; Kueider, Parisi, Gross, & Rebok, 2012; Melby-Lervåg & Hulme, 2012; Melby-Lervåg, Redick, & Hulme, 2016; Morrison & Chein, 2011; Noack, Lövdén, Schmiedek, & Lindenberger, 2009; Rapport, Orban, Kofler, & Friedman, 2013; Redick & Lindsey, 2013; Reijnders, van Heugten, & van Boxtel, 2013; Schubert, Strobach, & Karbach, 2014; Schwaighofer, Fischer, & Bühner, 2015; Shipstead, Hicks, & Engle, 2012; Shipstead, Redick, & Engle, 2012; Simons et al., 2016; Spencer-Smith & Klingberg, 2015; Spierer, Chavan, & Manuel, 2013; Stine-Morrow & Basak, 2011; Tardif & Simard, 2011; von Bastian & Oberauer, 2013
Physical Exercise Training	Angevaren, Aufdemkampe, Verhaar, Aleman, & Vanhees, 2008; Best, 2010; Bustamante, Williams, & Davis, 2016; Chaddock, Pontifex, Hillman, & Kramer, 2011; Chang, Pan, Chen, Tsai, & Huang, 2012; Colcombe & Kramer, 2003; Donnelly et al., 2016; Fedewa & Ahn, 2011; Gates, Singh, Sachdev, & Valenzuela, 2013; Hillman, Erickson, & Kramer, 2008; Karbach & Verhaeghen, 2014; Kramer & Erickson, 2007; Penedo & Dahn, 2005; Scherder et al., 2014; Smith et al., 2010; Snowden et al., 2011; Streiner, 2009; Tomporowski, Lambourne, & Okumura, 2011; Tomporowski, McCullick, Pendleton, & Pesce, 2015; Tseng, Gau, & Lou, 2011; van Uffelen, Chinapaw, Hopman-Rock, & van Mechelen, 2008; Verburgh, Königs, Scherder, & Oosterlaan, 2014; Voss, Nagamatsu, Liu-Ambrose, & Kramer, 2011, Young, Angevaren, Rusted, & Tabet, 2015
Mindfulness	Mak, Whittingham, Cunnington, & Boyd, 2017; Ng, Ho, Chan, Yong, & Yeo, 2017; Zenner, Herrnleben-Kurz, & Walach, 2014; Zoogman, Goldberg, Hoyt, & Miller, 2015
Multiple Methods or Other Methods	Barenberg, Berse, & Dutke, 2011; Bryck & Fisher, 2012; Burke, 2010; Cortese et al., 2016; Diamond & Lee, 2011; Etnier, Nowell, Landers, & Sibley, 2006; Etnier et al., 1997; Gothe & McAuley, 2015; Green & Bavalier, 2008; Greenberg & Harris, 2012; Hindin & Zelinski, 2012; Howard-Jones, 2014; Law, Barnett, Yau, & Gray, 2014; Lustig, Shah, Seidler, & Reuter-Lorenz, 2009; Moreau & Conway, 2013; Muraven, 2010; Rabipour & Raz, 2012; Riccio & Gomes, 2013; Sedlmeier et al., 2012; Sonuga-Barke et al., 2013; Stine-Morrow & Basak, 2011

Principle 2

The bad news is that transfer of training is narrow. Individuals improve on what they practice and that generalizes to untrained tasks where the same skills they practiced are required. Benefits are fairly specific, rather than general, and rarely transfer to untrained skills. Across all intervention approaches, regardless of the skills targeted, participants improve on the skills they practice, and that usually transfers to other contexts where those same skills are needed (narrow transfer), but people rarely improve on what they have not practiced. This has been the nearly universal conclusion of reviews for the past 25 years (see Table 8.6). Simon et al. (2016) went farther and concluded that transfer is only to tasks nearly identical to the training tasks. Indeed, as we have pointed out previously, "It is not even clear that training nonverbal WM transfers to verbal WM or that training nonverbal analogical-reasoning transfers to nonverbal gestalt reasoning on Raven's Matrices (e.g., Bergman Nutley, Söderqvist, Ottersen, Grill, & Klingberg, 2012)" (Diamond & Ling, 2016, p. 36).

At one extreme, some worry that WM training may not improve WM at all, but simply train task-specific strategies or response patterns useful for similar tasks but nothing else (Harrison et al., 2013; Simon et al., 2016). At the other extreme, some read the evidence as indicating that WM training can improve intelligence (that is, fluid intelligence, as assessed by tests of reasoning; Jaeggi, Buschkuehl, Jonides, & Perrig, 2008; for a meta-analysis, see Au et al., 2015). For example, an excellent meta-analysis of N-back training studies (Au et al., 2015) and a meta-analysis of diverse "process-based" EF and WM training programs (Karbach & Verhaeghen, 2014)[3] both found small but significant improvements in fluid intelligence (i.e., reasoning). However, other excellent meta-analyses (Melby-Lervåg & Hulme, 2012; Melby-Lervåg et al., 2016) found no convincing evidence of generalization of WM training to other cognitive skills and concluded that training produces "short-term, specific training effects that do not generalize" (Melby-Lervåg & Hulme, 2012, p. 270).

As an example, of the Cogmed computerized training studies that met criteria for inclusion in the present review, fully 79% of the 15 studies that looked at near transfer found at least suggestive evidence of it, but only 33% of the 12 studies that looked at far transfer found even suggestive evidence of it (see Figure 8.2). Of the 10 N-back training studies that looked at near transfer, 40% found at least suggestive evidence of it. Of the 11 N-back studies that looked at far transfer, only 30% found clear or suggestive evidence of it.

[3] Karbach and Verhaeghen's (2014) meta-analysis is not discussed further in this paper because it included many studies that did not meet criteria for inclusion in the systematic review. For example, they included studies did not include a control group (e.g., Buchler, Hoyer, & Cerella, 2008), they only looked at performance on the training tasks (e.g., Dorbath, Hasselhorn, & Titz, 2011), they did not include any EF outcome measure (e.g., Mahncke et al., 2006), or they only looked at acute effects from a single training session (Karbach, Mang, & Kray, 2010).

Table 8.6. Conclusions About the Presence or Absence of Far Transfer Drawn by Reviews

Authors (Year of Review)	Conclusion
Baltes & Lindenberger (1988)	"Effects of cognitive training do not spread beyond the boundaries of the task spaces trained."
Park, Gutchess, Meade, & Stine-Morrow (2007)	"Improving speed through training has not resulted in a global improvement in a range of cognitive processes—improvements have thus far been limited to only the trained ability."
Green & Bavalier (2008)	"Learning tends to be quite specific to the trained regimen and does not transfer to even qualitatively similar tasks."
Noack et al. (2009)	"Cognitive intervention studies . . . have failed to observe generalizable performance improvements. . . . Evidence for far positive transfer is almost entirely absent."
Diamond & Lee (2011)	"EF training appears to transfer (i.e., produce benefits to performance of tasks other than the task used in training), but transfer . . . thus far has been narrow."
Melby-Lervåg & Hulme (2012)	"Meta-analyses indicated that the programs produced reliable short-term improvements in WM skills. . . . There was no convincing evidence of the generalization of WM training to other skills."
Melby-Lervåg et al. (2016)	"Working memory training programs appear to produce short-term, specific training effects that do not generalize to measures of 'real-world'cognitive skills. . . . Attempts to increase working memory capacity by repetitively practicing simple memory tasks on a computer are unlikely to lead to generalized cognitive benefits."
Simons et al. (2016)	"Practice generally improves performance, but only for the practiced task or nearly identical ones; practice generally does not enhance other skills, even related ones."

Only one of the seven N-back studies *with an active control group* that looked at far transfer found more improvement or better posttest performance among the N-back trainees than active controls on any far-transfer measure. The lone exception (Stephenson & Halpern, 2013) found a positive result on only one of their four far-transfer measures, although all four were measures of fluid intelligence. Indeed, the N-back training study with the greatest dose, duration,

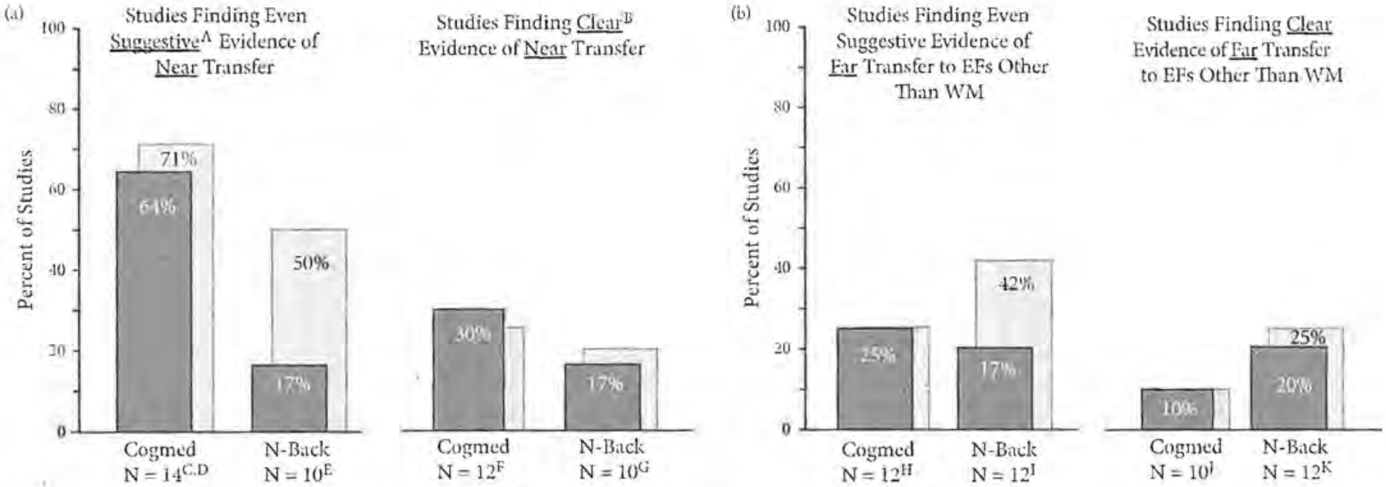

Figure 8.2. Success rates for improving EFs of the two most heavily studied computerized cognitive training approaches. Figure 8.2a presents the success rates for near-transfer measures. Figure 8.2b presents the results for EF far-transfer measures (including reasoning). The darker bars in the foreground present the results without the studies with possibly spurious positive results. The lighter bars in the background present the results for all studies. Studies omitted for having positive results that might not have held up were those that had not corrected for multiple comparisons or that had not conducted data analyses reflecting the level at which they randomized. [A] Suggestive evidence = more EF improvement *or* better EF posttest performance than control group on ≥50% of measures. [B] Strong, or clear, evidence = more EF improvement *and* better EF posttest performance than control group on ≥ 67% of measures. Whenever a study reported > 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations of strong evidence because it is possible the results of that study might have met our criteria had the results not reported been included. The

number of studies per group were: For Cogmed, suggestive evidence of near transfer: omitting some studies = 11; all studies = 14. For N-back, suggestive evidence of near transfer, omitting some studies = 6; all studies = 10. For Cogmed, strong evidence of near transfer: omitting some studies = 10; all studies = 12. For N-back, strong evidence of near transfer: omitting some studies = 6; all studies = 10. For Cogmed, suggestive evidence far transfer: omitting some studies = 12; all studies = 12. For N-back, suggestive evidence of far transfer: omitting some studies = 10; all studies = 12. For Cogmed, strong evidence far transfer: omitting some studies = 10; all studies = 10. For N-back, suggestive evidence of far transfer: omitting some studies = 10; all studies = 12. For near transfer: For *Cogmed*, the studies with the needed statistical analyses that found suggestive evidence of near transfer were: Green et al. (2012), Holmes et al. (2009), Hovik et al. (2013), Klingberg et al. (2005), and Thorell et al. (2009). Across all Cogmed studies, those reporting suggestive evidence of near transfer were: Bergman-Nutley and Klingberg (2014), Bergman-Nutley et al. (2011), Bigorra, Garolera, Guijarro, and Hervás (2015), Brehmer, Westerberg, and Bäckman (2012), Dunning, Holmes, and Gathercole (2013), Green et al. (2012), Gropper, Gotlieb, Kronitz, and Tannock (2014), Holmes, Gathercole, and Dunning (2009), Hovik, Saunes, Aarlien, and Egeland (2013), Klingberg et al. (2005), and Thorell et al. (2009). For N-back, the studies with the needed statistical analyses that found suggestive evidence of near transfer were Li et al. (2008) and Pugin et al. (2014). Across all N-back studies, those reporting suggestive evidence of near transfer were: Jaeggi et al. (2008), Li et al. (2008), Pugin et al. (2014), Redick et al. (2013), and Stepankova et al. (2014). For *Cogmed*, the studies with the needed statistical analyses that found strong evidence of near transfer were: Green et al. (2012), Holmes et al. (2009), Hovik et al. (2013), Klingberg et al. (2005), and Thorell et al. (2009). Across all Cogmed studies, those reporting strong evidence of near transfer were: Green et al. (2012), Holmes et al. (2009), Hovik et al. (2013), Klingberg et al. (2005), and Thorell et al. (2009). For N-back, the study with the needed statistical analyses that found strong evidence of near transfer was Li et al. (2008). Across all N-back studies, those reporting strong evidence of near transfer were: Jaeggi et al. (2008), Li et al. (2008), and Stepankova et al. (2014). For far transfer to EFs other than WM, including reasoning/fluid intelligence: For Cogmed, the studies with the needed statistical analyses that found suggestive evidence of far transfer were Green et al. (2012) and Klingberg et al. (2005). Across all Cogmed studies, those reporting suggestive evidence of far transfer were: Dunning et al. (2013), Green et al. (2012), Gropper et al. (2014), and Klingberg et al. (2005). For N-back, the studies with the needed statistical analyses that found suggestive evidence of far transfer were Jaeggi et al. (2010) and Rudebeck et al. (2012). Across all N-back studies, those reporting suggestive evidence of far transfer were: Jaeggi et al. (2008), Jaeggi, Buschkuehl, Perrig, and Meier (2010), Rudebeck, Bo, Ormond, O'Reilly, and Lee (2012), Stepankova et al. (2014), and Stephenson and Halpern (2013). For Cogmed, the study with the needed statistical analyses that found strong evidence of far transfer was Green et al. (2012). Across all Cogmed studies, those reporting strong evidence of far transfer were Green et al. (2012) and Klingberg et al. (2005). For N-back, the studies with the needed statistical analyses that found strong evidence of far transfer were Jaeggi et al. (2010) and Rudebeck et al. (2012). Across all N-back studies, those reporting strong evidence of far transfer were: Jaeggi et al. (2008, 2010), Rudebeck et al. (2012), and Stepankova et al. (2014).

and frequency (Redick et al., 2013) found neither more improvement nor better posttest performance on any of several fluid-intelligence/reasoning measures compared to controls, although they found the strongest near-transfer results of any N-back study with an active control group.

Similarly, task-switching training improves task switching, but not WM or reasoning. Reasoning training improves reasoning but not memory or speed of processing.

As the reader will see in our discussion below of each training method, there is only extremely spotty evidence for far transfer to untrained cognitive skills for any training method. The rare claims of far transfer have not held up to scrutiny. For example, the much-cited study by Jaeggi et al. (2008) has failed twice to be replicated by the same group (Buschkuehl, 2011) and had methodological problems (Moody, 2009; Redick et al., 2013; Shipstead et al., 2012; Tidwell, Dougherty, Chrabaszcz, Thomas, & Mendoza, 2014). No study has looked at transfer after WM training to a test of reasoning controlling for the WM components of the reasoning test.

Moreau and Conway (2014) suggest a very plausible explanation for why many cognitive training studies find such narrow transfer. Their suggestion deserves to be tested empirically. WM is typically trained using minor variations of laboratory tasks that challenge WM. These tests are repetitive and predictable; the timing of displays, type of stimuli, and response requirements stay the same. "Intense practice [on these] exacerbates the importance of domain-specific processes, because these tasks, when administered repeatedly, allow honing strategies or skills rather than tapping domain-general processes" (Moreau & Conway, 2014, p. 334). Following this logic, interspersing very different types of WM and EF challenges and reducing the predictability of what is presented or what is required to generate a correct response should improve both the generalizability and longevity of the effects of cognitive training, which echoes findings from the older learning literature for both cognitive skills (Bransford, Franks, Morris, & Stein, 1977) and motor skills (Kerr & Booth, 1978).

Another possibility is that training skills for arbitrary, decontextualized use (as in computerized WM training) may be the problem. *Engaging in a real-world activity* may be critical. We are less motivated to learn skills for which wehave no direct use. We evolved to be able to learn to help us act in the world, to do what we need to do. We are more motivated and learn something at a deeper level when we *need* that deeper level of understanding for a real-world use we care about (Cordova & Lepper, 1996; Freeman et al., 2014; Olson, 1964). It may be that what is needed is to engage participants in activities they care deeply about, where improving EFs is needed for what they want to do.

Consistent with that, Gathercole, Dunning, and Holmes (2012) suggested that "a fruitful approach may be to provide the trainee with practice in transferring their newly learned strategies to other situations that more directly simulate the everyday classroom demands on working memory. . . . We are therefore working on developing a complex task environment designed to bridge the gap between highly structured and relatively artificial activities that load WM and its flexible use in educational settings" (p. 202).

To see widespread benefits, diverse skills must be practiced. Where broader benefits have been found, it is usually true that there was narrow transfer of each of the practiced skills. For example, broad EF benefits have been found for a variant of taekwondo (Lakes & Hoyt, 2004) that trained diverse EF skills. In their meta-analysis of 15 randomized control trials (RCTs), Cortese et al. (2015) found that WM training had little or no effect on ADHD symptoms or other cognitive skills, even ones thought to rely on WM (such as reading and math). Their meta-analysis found, however, that interventions that targeted multiple cognitive skills had large effects on ADHD symptoms. Indeed, Cortese and colleagues call for the development and evaluation of multicomponent training models as a critical priority.

In an older meta-analysis of 25 studies (not all of which were RCTs and five of which had no control group), Rapport et al. (2013) also concluded that "claims regarding the academic, behavioral, and cognitive benefits associated with extant cognitive training programs are unsupported in ADHD" (p. 1237). Contrary to Cortese et al., however, Rapport et al. included studies in their review of programs that targeted EFs but did not produce improvements on any cognitive processes they targeted and so naturally failed to show far transfer.

Principle 3

To see benefits, continued challenge (i.e., adaptive practice) is critical. If participants are not challenged to keep improving, but simply continue doing what is easy, minimal benefit is seen. This is not only true for EF training and has been known for some time. It applies to all skills and ages. From studying experts in many different fields, Ericsson has repeatedly found that key to their prowess was continually pushing themselves to keep working at the outer limit of their competence and comfort zones (Ericsson, Nandagopal, & Roring, 2009; this is what Vygotsky, 1978, termed the "zone of proximal development"). The trick, of course, is not to discourage or frustrate someone by presenting too great a challenge (as can happen when aiming for the zone of proximal development but overreaching it). Yet, undershooting risks not helping participants progress as fast and far as they might or boring them and causing them to lose interest. Often participants are allowed to determine how fast and far they progress; the key then is to motivate them to keep pushing themselves to improve.

Principle 4

Studies have demonstrated that EF benefits can last months or even years (Ball et al., 2002; Bigorra et al., 2015; Borella et al., 2010; Brehmer et al., 2012; Carretti, Borella, Zavagnin, & de Beni, 2013; Dovis, van der Oord, Wiers, & Prins, 2015; Dunning et al., 2013; Gropper et al., 2014; Holmes et al., 2009; Klingberg et al., 2005; Li et al., 2008; Li-Grining et al., 2011; Plemons, Willis, & Baltes,1978; Pugin et al., 2014; Roberts et al., 2016; Van der Donk, Hiemstra-Beernink, Tjeenk-Kalff, van der Leij, & Lindauer, 2015; van der Oord, Ponsioen, Geurts, Ten Brink, & Prins, 2014; Willis et al., 2006), but naturally they grow smaller as the time since training increases. It would be unrealistic to expect benefits to last for a long time after practice ends. If you had gone to the gym regularly and improved your physical fitness, you would not expect the fitness benefits to last indefinitely if you stopped working out. On the other hand, if someone keeps using and challenging EF skills, then, presumably, benefits could last indefinitely. Use it or lose it. Also, academic benefits from improving EFs have occasionally increased over time or only shown up later (Bigorra et al., 2015; Blair & Raver, 2014; Holmes et al., 2009; Li-Grining et al., 2011).

Principle 5

Those more behind on EFs benefit the most from any intervention—within limits. Thus, persons with poorer WM or smaller WM spans (Holmes et al., 2009; Sibley & Beilock, 2007; Zinke, Zeintl, Eschen, Herzog, & Kliegel, 2012), worse attention (Flook et al., 2010), worse inhibitory control (Drollette et al., 2014), worse EFs in general (Flook, Goldberg, Pinger, & Davidson, 2015; Lakes & Hoyt, 2004), ADHD (Holmes et al., 2010; Klingberg et al., 2005), or the beginnings of cognitive decline with aging (Colcombe & Kramer, 2003; Kramer & Erickson, 2007) benefit more than others without the same challenges. Indeed, Fedewa and Ahn's (2011) meta-analysis of studies looking at the effects of physical activity on children's achievement and cognitive outcomes found that the mean effect size was twice as large for "cognitively impaired" children as for typically developing ones.

Children who are socioeconomically disadvantaged tend to have worse EFs (Blair & Raver, 2014; Farah et al., 2006; Noble, McCandliss, & Farah, 2007). These children benefit more from EF interventions than children with more socioeconomic advantages (Blair & Raver, 2014; Lillard & Else-Quest, 2006;

Mackey, Hill, Stone, & Bunge, 2011; Raver et al., 2008, 2011). This is seen in particularly stark relief in the Blair and Raver (2014) study, where effect sizes for children from diverse socioeconomic backgrounds rarely exceeded 0.1 standard deviation, but effect sizes for lower-income children were as high as 0.8! (See Figures 8.3a and 8.3b.) In all the aforementioned studies, similar subjects in the control group did not show similar gains, so disproportionate benefits to those farthest behind is not due to simple regression to the mean. Since those farthest behind on EFs tend to benefit the most from any EF intervention, and since less socioeconomically advantaged children tend to have poorer EFs, EF training might be a good way to reduce societal disparities in academic achievement and/or health associated with social disparities in EFs, especially if the EF training can be done early, before self-images of not being capable become established.

Extreme groups, such as children with a very low IQ or adults with severe cognitive decline, have benefitted less from cognitive training than have those

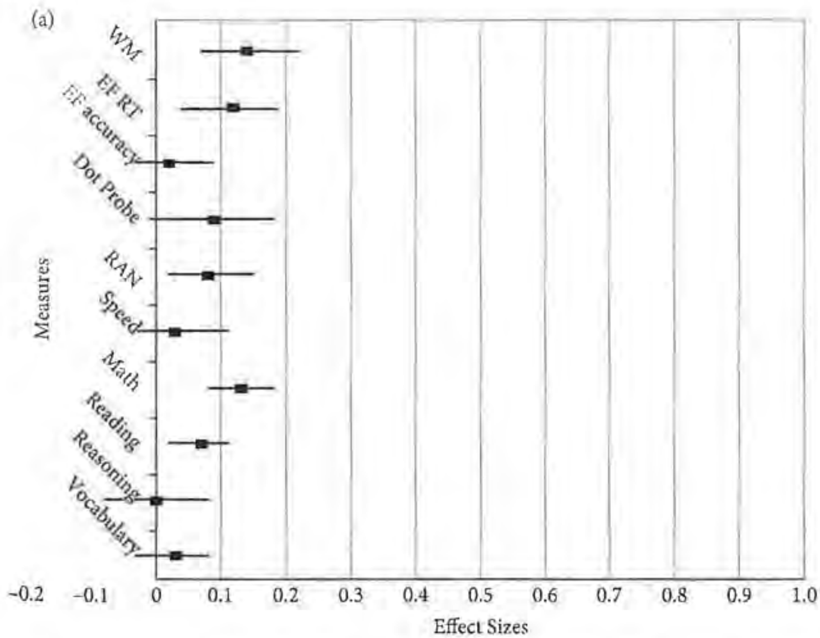

Figure 8.3a. Effect-size estimates including all schools in the study for main effects of Tools of the Mind versus the control group at the end of kindergarten in Blair and Raver (2014). Note that no effect size exceeds 0.2. Error bars represent ± 1 standard error. RT = reaction time; RAN = rapid automatic naming.

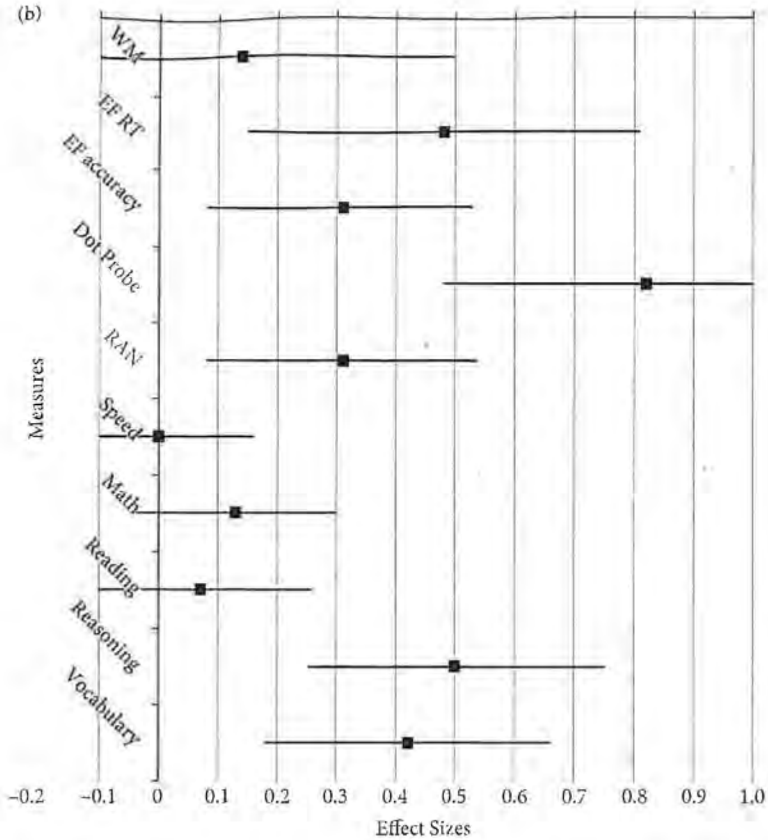

Figure 8.3b. Effect-size estimates including only the high poverty schools for main effects of Tools of the Mind versus the control group at the end of kindergarten in Blair and Raver (2014). Note that effect sizes now are as large as 0.8. Comparing Figures 8.3a and 8.3b, it is clear that Tools of the Mind had a far greater effect on EFs and academic skills among economically disadvantaged children than among more economically advantaged children. Errors bars represent ± 1 standard error. RT = reaction time; RAN = rapid automatic naming. Psychological Association. Figure 8.3 is reprinted, with permission, from Diamond, A., & Ling, D. S. (2016). Conclusions about interventions, programs, and approaches for improving EFs that appear justified and those that, despite much hype, do not. *Developmental Cognitive Neuroscience, 18*, 34–48.

with more intact cognitive skills or just the beginnings of dementia (Colcombe & Kramer, 2003; Söderqvist et al., 2012). That may be because the training was too demanding for them.

Principle 6

Duration matters. Within the range of durations studied, generally more weeks of computerized cognitive training has produced better results than fewer weeks. For example, training using Rise of Nations® (90-min sessions; three sessions per week) for 5 weeks produced better EF benefits than doing the same for only 2½ weeks (Basak, Boot, Voss, & Kramer, 2008). Perhaps one reason that Cogmed training has generally been more successful at improving EFs than N-back training is that the duration of Cogmed training is usually longer (5–8 weeks vs. 2–5 weeks).

Among the three mindfulness retreats for which EF benefits were investigated, the one that lasted the longest produced the best EF outcomes (MacLean et al., 2010), but the retreats also differed in type of mindfulness practiced, how long a mindfulness session lasted, and outcome measures. One year of resistance training has been found to improve inhibition (Stroop task performance), whereas only a half year into the training, that benefit was not evident (Liu-Ambrose et al., 2010). Masley, Roetzheim, and Gualtieri (2009), who studied extremely short durations of 5 to 7 days versus 3 to 4 days of a comprehensive program that included exercise, found that more days produced better EF results than fewer days.

There is evidence that at least four school curricular programs (MindUP™, Tools of the Mind™, PATHS™, and CSRP) do an excellent job at improving inhibitory control in young children. These programs include scaffolding, training, practicing, and challenging inhibitory control throughout a good part of the school day over the entire school year.

In short, Ericsson's conclusion about the critical importance of the amount of time spent practicing (with difficulty progressively increasing) for becoming really good at anything (Ericsson, 2006; Ericsson et al., 2009; Ericsson & Towne, 2010) appears to apply to EF skills just as it does to all the skills Ericsson studied.

One exception is EF benefits from aerobic exercise interventions. There is no evidence of greater EF benefits from longer aerobic exercise programs (see Table 8.7). This is contrary to the conclusion by Colcombe and Kramer (2003) that cognitive benefits to older adults were greater from longer aerobic exercise interventions, as many subsequent studies since their review have shown.

Principle 7

Dose (the length of individual sessions) appears to matter as well; 30 min or more seems better than less than 30 min. (The results for Quadrato Motor Training, with only 7-min sessions, are a marked exception however [Ben-Soussan,

Table 8.7. Duration, Dose, and Frequency of Aerobic Exercise Interventions Broken Down by Plain Aerobics (e.g., Brisk Walking) Versus Enriched Aerobics and by Whether EF Benefits Were Found

	Studies where benefits were found on at least half the EF measures										Studies where benefits were not found at all or were found on less than half of EF measures								
EF Benefits?	Study	Compared to AC or NT?	Duration (weeks)	Dose in Minutes [AE portion in brackets]	Fre- quency per week	*N*	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?	EF Benefits?	Study	Compared to AC or NT?	Duration (weeks)	Dose in Minutes [AE portion in brackets]	Fre- quency per week	*N*	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?
Suggestive	Albinet et al., 2010	AC	12	60 [40]	3	12	65–78	71	no	0	Blumenthal et al., 1989	NT	16	60 [45]	3	34	60–83	67	no
Suggestive	Dustman et al., 1984[2]	AC	16	60 [n/a]	3	14	55–70	60	YES	0	Erickson et al., 2011, Leckie et al., 2014, McAuley et al., 2011	AC	52	40 [10–40]	3	65	55–80	67	YES
Clear	Kramer et al., 1999	AC	24	? [n/a]	?	62	60–75	67	YES	0	Fabre et al., 2002	AC	8	60 [45]	2	8	60–76	66	no
Clear	Moul et al., 1995[2]	AC	16	30–40 [30–40]	5	10	65–72	69	no	< 50%	Fisher et al., 2011[2]	NT	10	60 [60]	2	32	5–7	6	YES
Suggestive	Tuckman & Hinkle, 1986	NT	12	30 [30]	3	77	8–12	10	no	0	Legault et al., 2011	AC	17	60 [40]	2	18	70–85	76	YES

										0	Mortimer et al., 2012[2]	NT	40	50 [30]	3	30	60–79	68	no
										0	Oken et al., 2006	NT	26	90 [60]	1	45	65–85	72	no
										0	Schmidt et al., 2015	AC	6	45 [45]	2	60	10–12	11	YES
										0	Smiley-Oyen et al., 2008	AC	40	45–50 [25–30]	3	28	65–79	70	no
										0	Voelcker-Rehage et al., 2011	AC	52	60 [35–50]	3	15	63–79	70	no
Means			16	46 [35]	4	35		55 [67]			Means		27	57 [42]	2	34		57 [70]	

Aerobic Exercise Enriched with Cognitive and/or Motor Skill Demands

Studies where benefits were found on at least half the EF measures										Studies where benefits were NOT found at all or were found on less than half of EF measures									
EF Benefits?	Study	Compared to AC or NT?	Duration (weeks)	Dose (min) [AE portion in brackets]	Frequency per week	N	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?	EF Benefits?	Study	Compared to AC or NT?	Duration (weeks)	Dose in Minutes [AE portion in brackets]	Frequency per week	N	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?
Clear	Chang et al., 2014	NT	8	90 [40]	2	15	5–10	8.5	no	< 50%	Chaddock-Heyman et al., 2013, Hillman et al., 2014, Kamijo et al., 2011	NT	36	120 [77]	5	14	8-9	9	no

(*continued*)

Table 8.7. Continued

EF Benefits?	Studies where benefits were found on at least half the EF measures									EF Benefits?	Studies where benefits were not found at all or were found on less than half of EF measures								
	Study	Compared to AC or NT?	Duration (weeks)	Dose in Minutes [AE portion in brackets]	Frequency per week	*N*	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?		Study	Compared to AC or NT?	Duration (weeks)	Dose in Minutes [AE portion in brackets]	Frequency per week	*N*	Age Range (years)	Mean Age (years)[1]	Was a demanding EF measure used?
Suggestive	Chuang et al., 2015	AC	13	30 [30]	3	8	65–75	68	no	0	Dalziell et al., 2015	NT	16	60 [n/a]	2	23	9-10	10	no
Suggestive	Gallotta et al., 2015[2,3]	AC	20	60 [30]	2	52	8-11	9.5	no	< 50%	Davis et al., 2007, 2011	NT	13	40 [35]	5	44	7-11	9	YES
Suggestive	Kim et al., 2011[2]	NT	26	60 [45]	2	26	60-78	68	YES	< 50%	Klusman et al., 2010	NT	24	90 [30]	3	91	70-93	74	no
Suggestive	Maillot et al., 2012	NT	12	60 [60]	2	16	65–78	74	YES	< 50%	Krafft, Pierce, et al., 2014, Krafft, Schaeffer, et al., 2014	AC	32	40 [40]	7	22	8-11	9.8	YES
Suggestive	Moreau et al., 2015[2]	AC	8	60 [40]	3	22	18–52	30	no	0	Legault et al., 2011	AC	17	60 [40]	2	18	70-85	76	YES
Suggestive	Predovan et al., 2012	NT	13	60 [15–40]	3	25	57–80	68	YES										
Suggestive	Staiano et al., 2012	NT	10	30 [30]	1	18	15–19	16.5	index or latent	0	Marmeleira et al., 2009	NT	12	60 [60]	3	16	60-82	68	no

Clear	Williams & Lord, 1997	NT	42	50–55 [35]	2	94	≥ 60	72	YES	< 50%	Pesce et al., 2013 -	NT	26	60 [60]	1	83	5-10	7	YES
										< 50%	Schmidt et al., 2015	AC	6	45 [45]	2	57	10-12	11	YES
Means[4]			17	56 [38]	2	31		46 [70]			Means[5]		20	64 [48]	3	41		30 [73]	

Note. AC = active control group; NT = no treatment group; AE = aerobic exercise. A demanding measure = a measure like the Wisconsin Card Sort Test or Tower of London, on which group differences are often more easily found than on easier EF tasks.

Clear = more EF improvement and better EF posttest performance than control group on ≥ 67% of measures.

Suggestive = more EF improvement or better EF posttest performance than control group on ≥ 50% of measures.

Index or Latent = creating a composite index from multiple EF measures or looking at the latent variable underlying performance on multiple EF measures is noted because those are likely to be more reliable and more sensitive than individual EF measures.

Reed et al. (2010) did not report near transfer EF measures, so is not included in this table.

[1] The number in brackets includes only studies where the mean age was ≥ 60 years.

[2] The authors of this study did not include a correction for multiple comparisons. It is unclear which of their results would remain significant had they done that.

[3] Gallotta et al. (2015) randomized by school but appear to have analyzed the data as if they randomized by individual children.

[4] If the FITKids studies are counted as three separate, independent studies, then for studies of enriched aerobic exercise where benefits were found on at least half the measures, the mean duration would be 18 weeks, mean dose would be 62 min of intervention with 41 min of aerobics, mean frequency would be three times per week, mean number of subjects would be 30, and the mean age of participants would be 42 years.

[5] If the FITKids studies are counted as three separate, independent studies, then for enriched aerobic exercise, the mean duration would be 22 weeks, mean dose would be 70 min of intervention with 47 min of aerobics, mean frequency would be four times per week, mean number of subjects would be 48, and the mean age of participants would be 28 years.

Glicksohn, & Berkovich-Ohana, 2015].) Colcombe and Kramer (2003), in their review of aerobic exercise interventions, reached a similar conclusion: "Short bouts of exercise (< 30 min) had very little impact on cognitive function; the effect at this training duration was not significantly different from zero" (p. 128). Davis et al. (2007, 2011) found better EF outcomes from longer sessions of aerobic games than from shorter ones (40 min per session vs. 20 min; see also McNaughten & Gabbard, 1993, who found greater cognitive benefits from physical exercise for 30 or 40 min vs. 20 min). Note, however, that for aerobic exercise at least, for doses of 30 min or more, there is no evidence that dose matters or that longer sessions produce better results (see Table 8.7).

Cogmed results have been better for children of 7 to 14 years than for children of 4 to 5 years (the former practiced for 30–45 min at a time, the latter for only 15 min). More Cogmed studies than N-back studies have found at least suggestive evidence of EF benefits (79% vs. 40%), and Cogmed sessions generally lasted 30 to 45 min, while N-back sessions generally lasted 15 to 30 min.

There are two exception to training for ≥ 30 min being better than < 30 min. One is the review of N-back studies by Au et al. (2015), which found a trend for shorter N-back training sessions to yield greater benefits than longer ones (within the range of 18.5–60 min, but only two N-back studies had sessions > 30 min). The other is a study by Mawjee and colleagues (Mawjee et al., 2014; Mawjee, Woltering, & Tannock, 2015), who compared 45 min of Cogmed to 15 min, both 5 days a week for 5 weeks, and found no difference. Neither duration produced any improvement, even on near-transfer measures. This is one of only three Cogmed studies *with adults* that looked at EF outcomes (the participants were 18–35 years old). Perhaps Cogmed works better with children than with young adults. Of concern, 23% of experimental group participants dropped out (very high attrition in that group alone). Either age or attrition, alone or in combination, might account for why they failed to find a WM benefit from Cogmed when most studies have.

The developers of the Tools of the Mind preschool and kindergarten curriculum initially tried their program as an add-on to existing curricula so that children did activities designed to improve EFs for roughly an hour a day. Benefits were narrow and specific to the context in which the skills had been practiced. Only when training and practicing of EFs was embedded in all school activities were benefits seen (Bodrova & Leong, 2007). Clements, Sarama, and Layzer (2012) replicated the limited benefits from Tools of the Mind as an add-on. Diamond, Barnett, Thomas, and Munro (2007) and Blair and Raver (2014) replicated marked EF benefits with Tools of the Mind as an all-day curriculum.

Principle 8

Spacing matters. Spaced, or distributed, practice produces better long-term outcomes than massed practice. That is, relatively shorter training or practice sessions spaced out over time produce better outcomes than compressing the training into a shorter time period with longer sessions (this is often referred to as the spacing effect). Skills and content are learned more slowly with spaced practice, but the learning is deeper and lasts longer. This has been shown for many cognitive skills (Cepeda et al., 2006; Rea & Modigliani, 1985) and motor skills (Lee & Magill, 1983; Shea & Morgan, 1979). Penner et al. (2012) provides an example of this with WM training. Their group that trained 2 days a week over 8 weeks (spaced practice) performed better on three near-transfer measures than the group that trained 4 days a week over 4 weeks (massed practice) and than no-treatment controls. Improvement of the massed-practice and no-treatment groups did not differ. Similarly, of three mindfulness retreats, the one that had the most spaced practice—2 hours a day for 13 weeks (MacLean et al., 2010)—produced better EF results than two other retreats that had more hours of meditation per day over a shorter time—11 hours a day for 1.5 weeks (Chambers, Lo, & Allen, 2008) and 9.8 hours a day for 4 weeks (Zanesco, King, MacLean, & Saron, 2013).

Principle 9

Often, the benefits of an intervention are only seen, or are seen most clearly, on outcome measures that push the limits of participants' EFs (Albinet, Boucard, Bouquet, & Audiffren 2010; Alesi et al., 2016; Chan, Sze, Siu, Lau, & Cheung, 2013; Davis et al., 2007, 2011; Diamond et al., 2007; Hillman et al., 2014; Manjunath & Telles, 2001; Predovan, Fraser, Renaud, & Bherer, 2012; Schmidt, Jäger, Egger, Roebers, & Conzelmann, 2015; Tucha et al., 2011; Westendorp et al., 2014). Complex, multicomponent measures (such as the Tower of London or Wisconsin Card Sort Test, which require multiple EF skills) are often excellent for distinguishing between groups, although because they require multiple EF skills they are not good for isolating which particular EF skill improved. They are good candidates for detecting outcome differences between the intervention and control groups precisely because they tax more than one EF skill.

How Different Approaches to Improving EFs Measure Up

Computerized Cognitive Training

Cogmed

Cogmed and N-back training have been researched more than any other cognitive training method. Of the 14 studies of Cogmed that met criteria for inclusion here and that reported WM outcomes, 10 found improved WM (71%) on at least 50% of their WM outcome measures. That is better than any other computerized training program (although noncomputerized cognitive training shows slightly better results: 75% of 12 studies found training benefits on at least 50% of their near-transfer measures).

An important caveat is that most Cogmed studies used outcome measures very similar to the Cogmed games on which participants trained (e.g., they might differ only in the precise stimuli, with task demands and structure being the same as in Cogmed, such as training participants to recall digits in reverse order and then testing participants on Backward Digit Span). Thus, improvements on Backward Digit Span, Spatial Span, or Corsi Blocks are not that impressive after Cogmed training because training games closely resemble those tasks. On the other hand, transfer to performance on complex-span tasks, which differ more from Cogmed training, was also reported by all 10 Cogmed studies that included one or more complex-span outcome measures.

Another important caveat is that although 70% to 71% of Cogmed studies found more improvement on ≥ 50% of their near-transfer EF measures in Cogmed trainees than in control subjects, only 25% to 33% of Cogmed studies found strong evidence of this (see Table 8.8 and Figure 8.2).

Twelve Cogmed studies administered at least one far-transfer task on which they compared groups. Of the 12, only 10 looked at both improvement and posttest. Of the 10 studies, only one (Green et al., 2012) found clear evidence of far transfer. Compared to a nonincrementing version of Cogmed, Green et al. found reduced inattentiveness in youths with ADHD in a naturalistic setting.

Note, however, that seven other studies of persons with ADHD (*six studies with children*: Bigorra et al., 2015; Chacko et al., 2014; Egeland et al., 2013; Gray et al., 2012; Klingberg et al., 2005; van der Donk et al., 2015; *one study with adults*: Gropper et al., 2014) looked for far transfer but found mixed results. Chacko et al., Gray et al., and van der Donk et al. found no far transfer. Bigorra et al. found some far transfer among children with ADHD—better improvement on a continuous performance test (CPT) and on teacher ratings on the BRIEF of metacognition and of children's ability to initiate an activity and independently problem-solve. Yet Bigorra et al. failed to find far transfer more often than they found it, and it is not clear that all their positive findings

Table 8.8. Summary of Results for Only Near-Transfer EF Outcomes for All Cognitive Training Approaches

	Percent of Studies Finding even Suggestive[1] Evidence of EF Benefits (# of studies)	Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of studies)	Percent of EF Outcome Measures on Which Experimental Group Improved more than Control Group (# of measures)	Percent of EF Measures on Which Experimental Group Performed Better at Posttest than Control Group (# of measures)
A. All Studies Are Included Here				
Cogmed Computerized Training	71% (14)[3]	25% (12)	69% (61)	33% (51)
N-back Computerized Training	50% (10)[4]	20% (10)	34% (35)	31% (35)
Computerized Complex-Span Training	50% (3)[5]	50% (2)	39% (18)	39% (18)
Task-switching Computerized Training	40% (5)	0% (5)	45% (47)	26% (38)
Other Computerized Cognitive Training (including Commercial Products)[6]	50% (26)	13% (23)	33% (168)	15% (150)
Noncomputerized Cognitive Training	75% (12)	30% (10)	47% (49)	38% (45)
B. Studies With Possibly Spurious Results Are Omitted Here[7]				
Cogmed Computerized Training	64% (11)	30% (10)	61% (36)	41% (32)
N-back Computerized Training	17% (6)	17% (6)	28% (29)	28% (29)

(continued)

Table 8.8. Continued

	Percent of Studies Finding even Suggestive[1] Evidence of EF Benefits (# of studies)	Percent of Studies Finding Clear[2] Evidence of EF Benefits (# of studies)	Percent of EF Outcome Measures on Which Experimental Group Improved more than Control Group (# of measures)	Percent of EF Measures on Which Experimental Group Performed Better at Posttest than Control Group (# of measures)
Computerized Complex-Span Training	—[8]	—[8]	—[8]	—[8]
Task-switching Computerized Training	67% (3)	0% (3)	74% (23)	43% (14)
Other Computerized Cognitive Training (including Commercial Products)	43% (23)	10% (21)	29% (143)	12% (130)
Noncomputerized Cognitive Training	73% (11)	33% (9)	46% (41)	39% (36)

[1] Suggestive = more EF improvement or better EF posttest performance than control group on ≥ 50% of measures.

[2] Clear = more EF improvement and better EF posttest performance than control group on ≥ 67% of measures. Whenever a study reported ≥ 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations here because it is possible the results of that study might have met our criteria for "clear" had the results not reported been included.

[3] Fifteen Cogmed studies are included in our review. One study did not include near-transfer measures and so does not appear in Table 8.8A.

[4] Thirteen N-back training studies are included in our review. Three did not include near-transfer measures.

[5] Six complex-span training studies are included in our review. One study did not include near-transfer measures and so does not appear in Table 8.8A. Two were noncomputerized and are included under Noncomputerized Training in Table 8.8A rather than under complex-span training.

[6] Other Computerized Cognitive Training includes both interventions we classified as miscellaneous computerized cognitive training and commercial computerized cognitive training approaches, as well as the noncommercial BrainGame Brian.

[7] Studies whose positive results might not have held up had they corrected for multiple comparisons or conducted data analyses reflecting the level at which they randomized are omitted here. Studies that made either error but found few, if any, positive results are not omitted, since the errors only increase the likelihood of false positives, not false negatives.

[8] There were too few computerized complex-span studies (only one) to be included here.

would have survived correction for multiple comparisons. They did not find far transfer on the Tower of London, Wisconsin Card Sort, any item on the parent version of the BRIEF, or most scales of the teacher version of the BRIEF. Klingberg et al. found more improvement on the Stroop task, Raven's Colored Matrices, and parental ratings of inattentiveness and hyperactivity/impulsivity, and better posttest scores for Raven's Colored Matrices. Posttest scores between groups for parental ratings of hyperactivity/impulsivity, however, were not significant. There was neither more improvement nor better posttest scores for teacher ratings of inattentiveness and hyperactivity/impulsivity. The far-transfer benefits were no longer present 3 months later in the Klingberg et al. study but were still robustly present 6 months later in the study by Bigorra et al. Indeed, more benefits on the BRIEF showed up later in the Bigorra et al. study than were present initially.

Egeland et al. did not find far transfer on the Stroop or Trail Making tests, any index on Conners' CPT or on the parent or teacher version of the BRIEF, or math. However, they found far transfer to reading, and the benefit was even greater 8 months later. Gropper et al. found EF benefits on self-report measures (Cognitive Failures Questionnaire and Adult ADHD Self-Report Scale [reduced occurrences of off-task behavior]) but no benefit on their one behavioral far-transfer measure (the Ruff 2 and 7 selective attention task).

In typically developing adults, Brehmer et al. (2012) found far transfer on the Cognitive Failures Questionnaire (more improvement and better posttest scores) yet no transfer benefit at all on the Stroop or Raven's Matrices tests. In typically developing schoolchildren, Dunning et al. (2013) reported more improvement and better posttest scores on far transfer to a CPT task after Cogmed versus no treatment, which seems to be due to the no-treatment group getting worse at posttest. No differences were found in change or posttest scores compared to the nonincrementing Cogmed group.[4]

Overall, the results for Cogmed were slightly better for typically developing schoolchildren than for schoolchildren with ADHD: When nonincrementing Cogmed and Cogmed were compared, suggestive WM benefits for Cogmed were found by 100% of studies of children without ADHD but only 75% of studies of children with ADHD. When Cogmed was compared to no treatment, suggestive WM benefits for Cogmed were found by 100% of studies, whether the children participating were typically developing or had ADHD. Strong evidence of EF benefits was similar for typically developing children and those with ADHD: Fifty percent of studies found strong evidence of

[4] Brehmer et al. (2012) did not correct for multiple comparisons. Dunning et al. (2013) did not either, nor did they conduct multilevel data analyses reflecting their cluster RCT design. It is unclear if their positive findings would have reached significance had these things been done.

WM benefits with Cogmed compared to nonincrementing Cogmed whether the children had ADHD or not. No studies found strong evidence of WM benefits when Cogmed was compared to no treatment whether the children had ADHD or not.

For some individuals with ADHD, auditory selective attention and auditory WM can be more of a challenge than visual or visual-auditory processing or WM (Fabio, Castriciano, & Rondanini, 2015; Gomes et al., 2012). Thus, training that includes challenging auditory selective attention and WM might be more beneficial for those with ADHD than protocols that are exclusively visual and visual-auditory.

Cogmed seems quite effective at improving the WM skills it trains people on, and those benefits last 3 to 12 months. Benefits have been found to last 3 months (Brehmer et al., 2012; Klingberg et al., 2005), 4 months (Beck, Hanson, Puffenberger, Benninger, & Benninger, 2010), 6 months (Bigorra et al., 2015; Holmes et al., 2009; van der Donk et al., 2015), and even 1 year (Dunning et al., 2013; Roberts et al., 2016). There is no evidence that the benefits last much beyond that, however, and little evidence that benefits extend to other cognitive skills. Three meta-analyses have all concluded that Cogmed does not reduce ADHD symptoms like inattentiveness or hyperactivity/ impulsivity when the raters are blind to who got Cogmed (Cortese et al., 2015; Rapport et al., 2013; Sonuga-Barke et al., 2013). Spencer-Smith and Klingberg (2015) reached a different conclusion but included unblinded parental raters.

A recent RCT of Cogmed with 452 first-graders found that while the WM improvement was still robust after 1 year (which is better than most methods can claim), it was no longer present 2 years later, and those who had trained on Cogmed performed *worse* in math 2 years later than the control group who received regular classroom instruction while their peers were training on Cogmed (Roberts et al., 2016, which was too recent to be included in Table 8.4 or our tabulations).

There is some evidence that Cogmed WM training might reduce inattentiveness in daily living for ADHD patients (Green et al., 2012; Gropper et al., 2014; Spencer-Smith & Klingberg, 2015) but there is a lack of evidence that any WM training improves performance on tests of attention. Positive evidence of reduced inattentiveness in daily living comes primarily from unblinded raters, with Green et al. (2012) an important exception. There is a great need for objective measures of attentiveness in daily life, such as the Restricted Academic Setting Task (RAST) used by Green et al. (2012). The few Cogmed studies that have looked at *objective laboratory measures* of attention have found either no benefit (Chacko et al., 2014; Egeland, Aarlien,

& Saunes, 2013; Gray et al., 2012; Gropper et al., 2014; van der Donk et al., 2015) or more improvement but not better posttest scores (the WM training group caught up: Klingberg et al., 2005; Thorell et al., 2009). No study has found WM training to yield both more improvement and a better end result on any objective laboratory tests of attention than they found in control subjects, except the study by Dunning et al. (2013), where it was not that the WM training particularly improved attention but that the control group mysteriously got worse.

Results on Near-Transfer EF Measures for School-Age Children Trained on Cogmed versus a Nonincrementing Version of Cogmed

The best results for Cogmed have been found with school-age children (7–15 years old) using a nonincrementing (a nonadaptive) version of Cogmed as the control condition (where difficulty does not keep increasing). On a total of eight WM measures in two studies (Dunning et al., 2013; Holmes et al., 2009), typically developing children who trained on Cogmed improved more than active controls on 88% of the measures, performed better at posttest than active controls on 38%, and showed better change and posttest scores on 38% (see Table 8.9).

Table 8.10, (appears in online: URL: http://www.devcogneuro.com/tables/supplemental.html) which presents the percentage across all EF measures (except reasoning/fluid intelligence) on which persons trained on Cogmed showed more improvement and/or better posttest results than comparison groups across all studies and ages, broken down by study, appears online.

On 100% of their three WM indices—a composite score for WM of shape or orientation (what they call visual-spatial WM), a composite score for WM for spatial location (what they call visual-spatial STM), and the Counting Span task (what they call verbal WM)—Holmes et al. (2009) found more improvement and better posttest performance for those who trained on Cogmed than among those who played a nonadaptive version of Cogmed, among 10-year-olds with initially poor WM. The Cogmed group also showed significant improvement in mathematical reasoning but that did not show up immediately after training. It was first evident at follow-up testing 6 months later.

For school-age children (7–15 years old) with ADHD, comparing Cogmed to a nonincrementing version of Cogmed on 13 WM measures across four studies (Bigorra et al., 2015; Chacko et al., 2014; Green et al., 2012; Klingberg et al., 2005) those trained on Cogmed improved more than active controls on 62% of the measures. Two studies did not compare whether posttest performance was better for Cogmed versus nonincrementing Cogmed, but for both studies that did (Klingberg et al., 2005; Green et al., 2012), those trained

Table 8.9. Percentage of WM Measures on Which Persons Who Trained on Cogmed Showed More Improvement and/or Better Posttest Results Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.
YOUNG CHILDREN (3–6 YEARS OLD)															
1	Bergman Nutley et al., 2011	Cogmed	Nonincrementing Cogmed	2	2	100%	1	2	50%	1	2	50%	1	2	50%
1	Bergman Nutley et al., 2011	Nonverbal reasoning (NVR)	Nonincrementing Cogmed	1	2	50%	1	2	50%	1	2	50%	1	2	50%
1	Bergman Nutley et al., 2011	Cogmed & NVR	Nonincrementing Cogmed	2	2	100%	1	2	50%	1	2	50%	1	2	50%
2	Thorell et al., 2009[A]	Cogmed	No treatment + commercially available computer games, where minimal need for WM or inhibition	2	2	100%	0	2	0%	0	2	0%	0	2	0%
2	Thorell et al., 2009[A]	Training on inhibitory control computer games	No treatment + commercially available computer games, where minimal need for WM or inhibition	0	2	0%	0	2	0%	0	2	0%	0	2	0%

	Totals and Percents for young children who trained specifically on WM using Cogmed compared with nonincrementing Cogmed			4	4	100%	1	4	25%	1	4	25%	1	4	25%
	Grand Totals and Percents for young children who trained on Cogmed, NVR, and inhibitory control computer games compared with nonincrementing Cogmed or no treatment + commercially available computer games			7	10	70%	3	10	30%	3	10	30%	3	10	30%
	SCHOOL-AGE CHILDREN (7–17 YEARS OLD) WITH NO CLINICAL DIAGNOSIS														
3	Dunning et al., 2013[B,C]	Cogmed	Nonincrementing Cogmed	4	5	80%	0	5	0%	0	5	0%	0	5	0%
4	Holmes et al., 2009[D]	Cogmed	Nonincrementing Cogmed	3	3	100%	3	3	100%	3	3	100%	3	3	100%
	Totals and Percents for school-age children with no clinical diagnosis who trained on Cogmed compared with a nonincrementing version of Cogmed			7	8	88%	3	8	38%	3	8	38%	3	8	38%
3	Dunning et al., 2013[B]	Cogmed	No treatment	4	5	80%	3	5	60%	3	5	60%	3	5	60%

(continued)

Table 8.9. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Totals and Percents for school-age children with no clinical diagnosis who trained on Cogmed compared with a nonincrementing version of Cogmed or no treatment			11	13	85%	6	13	46%	6	13	46%	6	13	46%
	SCHOOL-AGE CHILDREN (7–17 YEARS OLD) WITH ADHD														
5	Gray et al., 2012[C,E]	Cogmed	Academy of Math: Math Training	2	5	40%	1	5	20%	1	5	20%	1	5	20%
	Roberts et al. 2016[F]	Cogmed	No treatment	2	3	67%	2	3	67%	2	3	67%	2	3	67%
6	Bigorra et al., 2015[C,G,H]	Cogmed	Nonincrementing Cogmed	4	6	67%									
7	Chacko et al., 2014[H]	Cogmed	Nonincrementing Cogmed	1	4	25%									
8	Green et al., 2012	Cogmed	Nonincrementing Cogmed	1	1	100%	1	1	100%	1	1	100%	1	1	100%
9	Klingberg et al., 2005	Cogmed	Nonincrementing Cogmed	2	2	100%	2	2	100%	2	2	100%	2	2	100%
	Totals and Percents for school-age children with ADHD who trained on Cogmed compared with a nonincrementing version of Cogmed			8	13	62%	3	3	100%	3	3	100%	3	3	100%

10	*Bergman Nutley & Klingberg, 2014*[I]	*Cogmed*	*No treatment (typically developing)*	2	2	100%	1	2	50%	1	2	50%	1	2	50%
11	Egeland et al., 2013[J]	Cogmed	No treatment												
12	Hovik et al., 2013	Cogmed	No treatment	3	3	100%	0	3	0%	0	3	0%	0	3	0%
	Grand Totals and Percents for school-age children 7–17 years old with ADHD who trained on Cogmed compared with a nonincrementing version of Cogmed or no treatment			**13**	**18**	**72%**	**4**	**8**	**50%**	**4**	**8**	**50%**	**4**	**8**	**50%**
13	van der Donk et al., 2015	Cogmed	Paying Attention in Class, which combines WM & psychoeducational training	1	5	20%	1	5	20%	1	5	20%	1	5	20%
	Grand Totals and Percents for school-age children with or without a clinical diagnosis who trained on Cogmed compared with only nonincrementing Cogmed and no-treatment controls			**24**	**31**	**77%**	**10**	**21**	**48%**	**10**	**21**	**48%**	**10**	**21**	**48%**
	Grand Totals and Percents for school-age children with or without a clinical diagnosis who trained on Cogmed compared with nonincrementing Cogmed, no treatment, and the two studies with promising active-control conditions			**27**	**41**	**66%**	**12**	**31**	**39%**	**12**	**31**	**39%**	**12**	**31**	**39%**
	ADULTS (18–55 YEARS OLD) WITHOUT CLINICAL DIAGNOSES														
14	Brehmer et al., 2012[C]	Cogmed	Nonincrementing Cogmed	4	4	100%	2	4	50%	2	4	25%	2	4	50%
	ADULTS (18–55 YEARS OLD) WITH ADHD														
15	Gropper et al., 2014[C]	Cogmed	No treatment	2	6	33%	2	6	33%	2	6	33%	2	6	33%
	Grand Totals and Percents for all ages and populations who trained on Cogmed compared with no treatment			**11**	**16**	**69%**	**6**	**16**	**38%**	**6**	**16**	**38%**	**6**	**16**	**38%**

(continued)

Table 8.9. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# Measures	% Sign.	# Sign.	# of Measures	% Sign.
			OLDER ADULTS (OLDER THAN 55 YEARS) WITHOUT CLINICAL DIAGNOSES												
14	Brehmer et al., 2012	Cogmed	Nonincrementing Cogmed	3	4	75%	0	4	0%	0	4	0%	0	4	0%
	Grand Totals and Percents for all ages and populations who trained on Cogmed compared with a nonincrementing version of Cogmed ($N = 10$ studies)			26	33	79%	10	23	43%	10	23	43%	10	23	43%
	Grand Totals and Percents for all ages and populations who trained on Cogmed compared with no treatment ($N = 7$ studies)			13	18	72%	6	18	33%	6	18	33%	6	18	33%
	Grand Totals and Percents across all Cogmed studies (including only nonincrementing Cogmed and no-treatment control conditions)			39	51	76%	16	41	39%	16	41	39%	16	41	39%
	Grand Totals and Percents across all Cogmed studies (including the promising active-control conditions of Gray et al. & van der Donk et al.)			42	61	69%	18	51	35%	18	51	35%	18	51	35%

Note. Results for outcomes other than WM are not included here.

[A] Thorell et al. (2009) had separate no-treatment and active-control groups but they combined the two groups in their analyses. The results for Thorell et al.'s inhibitory control training are listed here, but they were not included in any calculations because it was not WM training.

[B] The authors of this study did not conduct the needed multilevel data analysis. It is unclear how many of their results would remain significant had they done that.

[C] The authors of this study did not correct for multiple comparisons. It is unclear which results, if any, would remain significant had that been done.

[D] *All participants had poor WM at study outset.*

[E] One might plausibly expect EF benefits from math training, so a failure to find a difference here might be due to both interventions' being beneficial, rather than Cogmed's being ineffectual, thus we have not included the null findings here when calculating totals or percentages, except where otherwise noted.

[F] The study by Roberts et al. (2016) was published after the 2015 cutoff date. We include it here because we think it is important, but we did not include it in calculations of totals or percentages.

[G] One might plausibly expect EF benefits from the "Pay Attention in Class" intervention, so a failure to find a difference here might be due to both interventions' being beneficial, rather than Cogmed's being ineffectual, thus we have not included the null findings here when calculating totals or percentages, except where otherwise noted.

[H] These studies did report the difference between posttest scores.

[I] All participants in the experimental group for Bergman-Nutley & Klingberg (2014) had WM deficits at the outset of the study and most had ADHD.

[J] Egeland et al. (2013) did not report any near-transfer EF results.

on Cogmed performed better at posttest on all (100%) of their three WM measures.

For example, on the composite WM score from the WISC-IV, Green et al. (2012) found more improvement and superior posttest performance among those who trained on Cogmed versus those who played a nonadaptive version of Cogmed among 10-year-olds with ADHD. Also, on an objective measure of inattentive behavior in a naturalistic paradigm designed to simulate attentional demands in the classroom, those trained on Cogmed improved more and performed better at posttest than active controls. Such positive results were obtained even though Green et al. had participants train for a relatively short time (4 weeks), whereas other Cogmed studies have had participants train for 5 to 8 weeks.

Bigorra et al. (2015) created a composite WM score consisting of the two WM subscales from the WISC-IV (Backward Digit Span and Letter-Number Sequencing) plus Backward Spatial Span from the Wechsler Memory Scale-III. They found that 7- to 12-year-old children with ADHD who trained on Cogmed improved significantly more on the WM composite and performed better on Conners' CPT than their peers who played a nonadaptive version of Cogmed. Their relatively better improvement on the WM composite, however, seems to be because the control group mysteriously got worse. Parental ratings on the BRIEF did not differ significantly between the two groups right after training, but 6 months later, the parents of those who had done Cogmed reported more improvements than the parents of controls. Teachers saw some benefits right away in Cogmed-trained children, but more and larger benefits 6 months laterthan compared with their ratings of children in the control group. Between-group comparisons of posttest performance were not reported.

Across all Cogmed studies (regardless of the age of participants or whether they had ADHD or not), those trained on Cogmed, when compared with those who played a nonadaptive version of Cogmed, (a) improved more on 79% of WM measures, (b) performed better at posttest on 43% of WM measures, and (c) showed both better change and better posttest scores on 43% of WM measures (i.e., near-transfer measures). The numbers improve considerably if studies that did not do the needed statistical analyses are excluded. Then, the percentage of measures both for better performance at posttest and for the combination of better posttest performance and more improvement was 89%.

Results on Near-Transfer EF Measures for Cogmed Training of School-Age Children versus No Treatment

Dunning et al. (2013) conducted the only study to compare Cogmed training to no treatment among typically developing children. Schoolchildren 7 to 9 years old who trained on Cogmed improved more and performed better at posttest than the no-treatment group on four of the five WM measures used (80%). Those benefits, and the benefits found in comparison to nonincrementing Cogmed, were still evident 12 months later (but see Footnote 4).

Three studies compared Cogmed training to no treatment for children with ADHD (Bergman-Nutley & Klingberg, 2014; Egeland et al., 2013; Hovik et al., 2013). Bergman-Nutley and Klingberg found that, after 5 weeks of Cogmed training, 11-year-old children with ADHD not only improved more than, but also outperformed, typically developing peers on a visuospatial WM measure. That's impressive, although on a second visuospatial WM measure, results were less positive.

Egeland et al. (2013) included no near-transfer EF measures (i.e., no WM measures) and found no benefit compared to controls on any of their six far-transfer EF measures.

Hovik et al. (2013) found that, after 5 weeks of Cogmed training, 10-year-olds with ADHD had improved more on all three WM indices they looked at (both verbal and visuospatial measures) than other ADHD children in the no-treatment group. That, too, is impressive.

Across all Cogmed studies with no-treatment or nonadaptive-Cogmed control conditions (regardless of the age of participants or whether they had ADHD), those trained on Cogmed improved more on 69% of the WM measures compared with control subjects, but performed better at posttest than controls on only 33% (and for studies reporting only posttest comparisons: 33%) and showed better change and posttest scores on 37% of WM measures (see Table 8.9).

Results on Near-Transfer EF Measures for Cogmed Training of School-Age Children versus Another Intervention

The two studies that compared Cogmed training to another intervention both found few differences between the benefits of Cogmed and the other intervention. Gray et al. (2012) compared Cogmed training to special math training for schoolchildren of 7 to 15 years diagnosed with both ADHD and a learning disorder. They found more improvement after Cogmed than math training on two measures very similar to Cogmed games (e.g., Backward Digit Span and Spatial Span), but not on three other WM measures, and not on five tests of other EF skills. Except for Backward Digit Span, on no measure was posttest performance significantly better in the Cogmed group than in the math-training group. Van der Donk et al. (2015) found similar benefits from Cogmed training and an intervention called "Pay Attention in Class" when these were delivered to 8- to

12-year-old schoolchildren with ADHD. Those who received Cogmed training improved more and performed better at posttest on a Forward Spatial Span task, but not on four other WM measures nor on five tests of other EF skills. Either the comparison condition in each of these two studies produced similar benefits to those from Cogmed, or—since there was no business-as-usual or no-treatment group in either study—it could be that neither program produced more benefit than would normally have occurred from practice effects plus 5 weeks of school.

If the programs indeed produced comparable benefits, given how different the three programs were, perhaps it was the increased attention from adults who expected benefits from the program or the excitement about a new program that produced the benefits, rather the content of the training programs per se. That would be consistent with de Jong's finding (de Jong, 2014) that the mentoring component of Cogmed might be more central to its benefits than the computerized training. Of course, one might plausibly expect EF benefits from math training or from the "Pay Attention in Class" intervention, so a failure to find a difference might be due to both interventions' also being beneficial due to their content, rather than the interaction with adults.

Results for Cogmed Training of Adults

Only one study has looked at possible EF benefits from Cogmed among older adults (Brehmer et al., 2012). Brehmer and colleagues studied both younger adults (20–30 years old) and older adults (60–70 years old). They found more improvement on all four (100%) of their near-transfer EF measures (backward digit span, Backward Span Board, Forward Span Board, and Paced Auditory Serial Addition Test—PASAT[5]) among both age groups for the Cogmed group versus the nonincrementing Cogmed control group, with larger differences for younger than older participants. The Cogmed trainees also showed more improvement on a more distal measure: the Cognitive Failures Questionnaire (CFQ). All the improvements were still evident 3 months later. (Brehmer et al., however, did not correct for multiple comparisons.)

However, even on near-transfer measures quite similar to the training tasks (e.g., Backward Digit Span), Brehmer et al. found only one significant posttest score difference between Cogmed trainees and active controls (young adults on Forward Span Board; the other three comparisons did not show a Cogmed benefit), although all four comparisons showed more improvement from Cogmed than from the nonadaptive-training active-control condition. The same was true for the CFQ (more improvement, but not better posttest scores than active controls). In general, the two Cogmed groups started off a bit worse than the two nonincrementing Cogmed groups (except for younger adults on Forward Span

[5] Brehmer et al. (2012) considered PASAT a sustained attention test and thus a far-transfer measure, but on this test participants hear single digits every 3 seconds and are to add each new digit to the one immediately prior to it. We consider this a WM measure.

Board and older adults on Backward Digit Span and CFQ), which helps explain why change scores were significantly different while performance after the intervention generally was not.

The only other study of Cogmed in adults meeting our inclusion criteria (besides Mawjee et al., 2014, 2015, discussed above, which only compared more versus less Cogmed) looked at Cogmed training versus no treatment for adults 19 to 52 years old who had ADHD (Gropper et al., 2014). They found that those who did Cogmed reported fewer cognitive failures in everyday life and fewer instances of off-task behavior (and a greater reduction in those) than did control subjects, and that was still true 2 months later. Cogmed trainees also improved more and performed better at posttest on very-near-transfer measures of spatial span and digit span than did no-treatment controls, although they showed no benefits on the PASAT (an auditory serial addition WM task), a CANTAB spatial WM measure, or a measure of selective attention.

Results for Cogmed Training of Children 4 to 6 Years Old

Among children 5 years old, Thorell et al. (2009) found lots of evidence of differential improvement. The experimental group improved more on Forward + backward word span (combined), Forward + backward visuospatial span (combined), and sustained attention than those who did a nonadaptive version of Cogmed or received no treatment. On no measure, however, did Cogmed trainees perform significantly better at posttest than those who trained on a nonadaptive version of Cogmed or those in the no-treatment group.

The other study that looked at Cogmed benefits in very young children was by Bergman Nutley et al. (2011). They studied 4-year-olds and found a benefit from Cogmed on a visuospatial WM task (similar to Corsi Blocks and similar to Cogmed games) compared with active controls. On STM measures, which do not assess WM—Block Design (visuospatial STM) and Forward Word Span (verbal STM)—and on a far-transfer reasoning measure (Odd One Out), they found no benefit from Cogmed WM training. Those trained in reasoning improved more on reasoning than did active controls. Benefits were narrow, not even generalizing from visuospatial to verbal (or even to all visuospatial memory measures), although perhaps if their measures had tapped WM, rather than STM, a benefit might have been found.

Discussion of Results from Cogmed Training Studies

Cogmed works well for improving WM, especially on measures similar to training games in Cogmed itself and on complex-span measures. The one study that looked at improving inhibitory control (Thorell et al., 2009) found disappointing results: Perhaps not enough pretesting, thought, and/or effort had gone into constructing the inhibitory-control training, or the children (ages 4–5 years) were too young, the training sessions were too short (15 min rather than the 30–45 min used for older children), or computer training may not be the best way to improve inhibition. Rueda and colleagues (Rueda, Checa, & Cómbita, 2012; Rueda, Rothbart, McCandliss, Saccomanno, & Posner, 2005) also found disappointing results in their attempt to improve inhibitory control in 4- and 5-year-olds using different computerized training. Blakey and Carroll (2015) administered both computerized WM and inhibitory-control training to 4-year-olds and found that WM improved but inhibitory control did not.

If someone has a circumscribed deficit specific to WM, we recommend Cogmed. The superb recent review by Simons et al. (2016) also concluded that "the randomized controlled trials provide strong evidence that Cogmed training improves performance on other working-memory tasks with similar processing demands" (pp. 147–148), although Simons et al. would be more circumspect in emphasizing benefits only on quite similar tasks, not benefits to WM (even visuospatial WM) generally. For benefits to last, we recommend engaging in continuing, ongoing challenges to WM, or else the WM benefits from Cogmed or anything else will likely disappear in months or a year or two. For benefits to WM more broadly, we recommend training and challenges also to other aspects of WM less emphasized in Cogmed games. If someone has deficits in inhibitory control or diverse EF components, however, we do not recommend Cogmed.

Surprisingly, only one study has looked at Cogmed with older adults. That is a topic crying out for research. WM is one of the first cognitive skills to deteriorate with aging and it often shows the greatest decline (e.g., Hedden & Park, 2001; Wang et al., 2011). Hence Cogmed might be an excellent option for older adults with WM decline, provided they enjoy the Cogmed games enough to keep working at them and have good mentors. In the one study that looked (Brehmer et al., 2012), older adults who trained on Cogmed showed more improvement than controls on all four (100%) of the near-transfer EF measures and on the more distal CFQ, and all improvements were still evident 3 months later. More studies with older adults are needed.

Sometimes the reason something works can be quite different from what anyone expected. Although most studies of Cogmed do not mention the mentoring component, to be certified to administer Cogmed, adults must be trained in, and commit to, mentoring those doing Cogmed. De Jong (2014) found that the mentoring may actually account for the benefits of Cogmed even more than the computerized games. That merits follow-up and further study. It also illustrates that the reason why a program is beneficial should be investigated, rather than assumed. (See the discussion above about the two studies that compared Cogmed to other interventions—the attention from adults might turn out to be more crucial than most people have thought.)

The nonincrementing version of Cogmed includes interaction with adults similar to what occurs with the standard, adaptive version of Cogmed, yet the standard version consistently produces better results than the nonincrementing version. Does this argue against the mentoring component potentially being critical for the benefits? Probably not, because it is unlikely that the mentors expect similar benefits from the control condition. It is probably critical that the mentor believes in the efficacy of the training and expects it to benefit the trainee.

N-Back Training

N-back tasks are explained in the section "EFs Explained" above. Unlike studies of Cogmed, where all but two of the 15 studies (87%) were with children, most studies of N-back training have been with adults (all but two out of 13, or 85%). Whereas 80% of Cogmed studies included an active control group, only 54% of N-back training studies did (albeit most Cogmed studies have used a control group not challenged as much nor presumably expecting as much improvement as those training on Cogmed). Most N-back studies included training for fewer weeks than Cogmed (all but one Cogmed study had 5–8 weeks of training; all but two N-back studies trained participants only 2–5 weeks) and had shorter training sessions—15 to 30 min in 10 out of 13 N-back studies (77%) versus 30 to 45 min for Cogmed sessions in 13 of the 16 studies (81%).[6]

Across all Cogmed studies, more improvement than in comparison conditions was reported on 69% of the near-transfer EF measures and better posttest results than in comparison conditions on 33% of near-transfer EF tasks for which a comparison of posttest results was reported (see Table 8.8a). (Looking only at the studies that included the requisite statistical analyses, the corresponding percents are 61% for more improvement than comparison conditions on WM measures and 38% for better posttest performance on WM measures than comparison conditions for Cogmed. See Table 8.8b.) Seventy-one percent of Cogmed studies found at least suggestive evidence of WM benefits.

Results for N-back training are more disappointing. For N-back training, more improvement than comparison subjects was reported on only 34% of near-transfer EF measures and better posttest performance than comparison subjects on 31% of near-transfer EF measures. (See Table 8.8a. Looking only at the studies that did the requisite statistical analyses, the corresponding percents are 28% for more improvement than comparison conditions on WM measures and 28% for better posttest performance on WM measures than comparison conditions for N-back training. See Table 8.8b.) Fifty percent of all N-back studies (only 17% of N-back studies with the requisite statistical analyses) found at least suggestive evidence of WM benefits (see Figure 8.2). Perhaps if N-back training sessions were longer or continued for more weeks (more like Cogmed), better results would be found. However, N-back studies with longer sessions (30–60 min) have not found EF benefits (Chooi & Thompson, 2012; Kundu, Sutterer, Emrich, &

[6] Although note that Mawjee et al. (2014, 2015) compared Cogmed training sessions of 15 and 45 minutes, holding everything else constant, and found no difference in benefits.

Postle, 2013; Lilienthal, Tamez, Shelton, Myerson, & Hale, 2013; Pugin et al., 2014; Thompson et al., 2013). On the other hand, Basak et al.'s (2008) findings with Rise of Nations (that 2–3 weeks was too short to see benefits, although benefits were seen after 4–5 weeks) suggest that continuing training for more weeks might well make a difference.

Results were somewhat better when N-back was compared to no treatment than when it was compared to active-control conditions. *Compared to active-control conditions*, N-back training produced more improvement on only 18% of all near-transfer measures across studies and better posttest performance on only 18% as well. *Compared to no-treatment controls*, N-back training produced more improvement on 42% of all near-transfer measures across studies and better posttest performance on 38%.

Perhaps it is not that surprising that no differences were found in benefits from N-back training versus playing Tetris (Kundu et al., 2013), since both would be expected to challenge EFs and hence improve them. That there were no differences in WM benefits from N-back training versus visual search training (Redick et al., 2013) or versus training multiple-object tracking (Thompson et al., 2013) is more surprising (see Tables 8.11 and 8.12). Both studies included no treatment controls; results were no better comparing N-back to no treatment than to the active-control condition in each of these studies. This suggests that benefits in these two studies seem to have been simply due to practice in taking the outcome measures (which were completed both before and after the weeks of training).

Two N-back training studies have been done with older adults (Li et al., 2008; Stepankova et al., 2014). For the study by Stepankova et al., where the mean age of participants was 68 years (range = 65–74), those trained on N-back showed more improvement and performed better at posttest than no-treatment controls on both (100%) of the EF near-transfer measures (Forward + backward combined digit span task and a challenging WM task [Letter-Number Sequencing, where a series of numbers and letters are presented orally in random order and then the participant is to repeat back the numbers in numerical order followed by the letters in alphabetical order]) regardless of whether participants were assigned to N-back practice more or less often (four vs. two times per week). Those trained on N-back also improved more and performed better at posttest than no-treatment controls on both visuospatial far-transfer measures (matrix reasoning and block design) that Stepankova and colleagues administered. Participants in the Li et al. study were 70 to 80 years old (mean age = 74). Those trained on N-back performed better and improved more on two very-near-transfer nontrained N-back tasks but not on either complex-span task tested compared to no-treatment controls (but, remember, the correlations between N-back and complex-span tests are low, so the lack of transfer may not be indicative of lack of benefits).

Table 8.11. Percentage of WM Measures on Which Persons Who Received N-Back Training Showed More Improvement and/or Better Posttest Results Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–17 YEARS OLD)											
1	Jaeggi et al., 2011[A]	Single N-back task	Computerized knowledge & vocabulary task												
2	Pugin et al., 2014[B]	Single N-back task	No treatment	1	2	50%	1	2	50%	1	2	50%	1	2	50%
				ADULTS (19–55 YEARS OLD)											
3	Chooi & Thompson 2012	Dual N-back task (8 or 20 sessions)	Nonincrementing version of training games	0	1	0%	0	1	0%	0	1	0%	0	1	0%
4	Lilienthal et al., 2013[C]	Dual N-back task	Nonincrementing dual N-back task task	1	4	25%	1	4	25%	1	4	25%	1	4	25%
5	Redick et al., 2013[B,D]	Dual N-back task	Visual search	1	2	50%	1	2	50%	1	2	50%	1	2	50%
6	Stephenson & Halpern, 2013[A,B]	Dual N-back task	Spatial matrix span												
7	Thompson et al., 2013	Dual N-back task	Multiple object tracking	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for adults who received N-back training compared with an active-control condition			2	10	20%	2	10	20%	2	10	20%	2	10	20%

(continued)

Table 8.11. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
8	Kundu et al., 2013[E]	Dual N-back task	Tetris	0	1	0%	0	1	0%	0	1	0%	0	1	0%
	Grand Totals and Percents for adults and children who received N-back training compared with an active-control condition (excluding Kundu et al., 2013)			2	10	20%	2	10	20%	2	10	20%	2	10	20%
	Grand Totals and Percents for adults and children who received N-back training compared with any active-control condition (including Kundu et al., 2013)			2	11	18%	2	11	18%	2	11	18%	2	11	18%
	Grand Totals and Percents for adults and children who received N-back training compared with an active-control condition or no treatment (excluding Kundu et al., 2013)			3	12	25%	3	12	25%	3	12	25%	3	12	25%
	Grand Totals and Percents for adults and children who received N-back training compared with any active control condition or no treatment (including Kundu et al., 2013)			3	13	23%	3	13	23%	3	13	23%	3	13	23%
	ADULTS (19–55 YEARS OLD)														
3	Chooi & Thompson, 2012	Dual N-back task (8 or 20 sessions)	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
9	Jaeggi et al., 2008[B]	Dual N-back task (8, 12, 17, or 19 sessions)	No treatment	1	2	50%	1	2	50%	1	2	50%	1	2	50%

10	Jaeggi et al., 2010	Dual N-back task	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
10	Jaeggi et al., 2010	Single N-back task	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
11	Li et al., 2008	Single N-back ask	No treatment	2	3	67%	2	3	67%	2	3	67%	2	3	67%
4	Lilienthal et al., 2013	Dual N-back task	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
5	Redick et al., 2013[B]	Dual N-back task	No treatment	1	2	50%	0	2	0%	0	2	0%	0	2	0%
12	Rudebeck et al., 2012[A]	Dual N-back task	No treatment												
6	Stephenson & Halpern, 2013[A,B]	Dual N-back task	No treatment												
7	Thompson et al., 2013	Dual N-back task	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for adults 19–55 years old who received N-back training compared with no treatment			5	17	29%	4	17	24%	4	17	24%	4	17	24%
	Grand Totals and Percents for adults and children who received N-back training compared with no treatment			6	19	32%	5	19	26%	5	19	26%	5	19	26%
	Grand Totals and Percents for Adults (19–55 years old)			7	28	25%	6	28	21%	6	28	21%	6	28	21%
				OLDER ADULTS (OLDER THAN 55 YEARS)											
11	Li et al., 2008	Single N-back task	No treatment	2	3	67%	2	3	67%	2	3	67%	2	3	67%

(continued)

Table 8.11. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
13	Stepankova et al., 2014[B]	Single N-back task (10 or 20 sessions)	No treatment	2	2	100%	2	2	100%	2	2	100%	2	2	100%
	Totals and Percents for older adults who received N-back training compared with no treatment			4	5	80%	4	5	80%	4	5	80%	4	5	80%
	Grand Totals and Percents across all ages who received N-back training compared with no treatment			10	24	42%	9	24	38%	9	24	38%	9	24	38%
	Grand Totals and Percents across all N-back studies (excluding Kundu et al., 2013)			12	34	35%	11	34	32%	11	34	32%	11	34	32%
	Grand Totals and Percents across all N-back studies (including Kundu et al., 2013)			12	35	34%	11	35	31%	11	35	31%	11	35	31%

Note. Results for outcomes other than WM are not included here.

[A] This study did not include any near-transfer measures. They only looked at reasoning/fluid intelligence (R/FL) measures.

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results, if any, would remain significant had that been done.

[C] Studies that varied the number of training sessions found no difference by number of sessions on anything relevant to this table, so results across those different conditions are collapsed here.

[D] The one significant difference here was because the control group mysteriously got worse at posttest on the running letter span task, while those who trained on N-back with difficulty increasing (as well as the no-treatment group) improved.

[E] One might plausibly expect EF benefits from playing Tetris, so a failure to find a difference here might be due to both interventions' being beneficial, rather than N-back training's being ineffectual, thus we have not included the null findings here when calculating totals or percentages, except for the last line.

Table 8.12. *Percentage of EF Measures (Except Reasoning/Fluid Intelligence) on Which Persons Who Received N-Back Training Showed More Improvement and/or Better Posttest Results Than Comparison Groups Across All Studies and Ages, Broken Down by Study*

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–17 YEARS OLD)											
1	Jaeggi et al., 2011[A]	Single N-back task	Computerized knowledge & vocabulary task												
2	Pugin et al., 2014[B]	Single N-back task	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
				ADULTS (19–55 YEARS OLD)											
3	Chooi & Thompson, 2012	Dual N-back task (8 or 20 sessions)	Nonincrementing version of training games	0	2	0%	0	2	0%	0	2	0%	0	2	0%
4	Lilienthal et al., 2013[C]	Dual N-back task	Nonincrementing dual N-back task	1	5	20%	1	5	20%	1	5	20%	1	5	20%
5	Redick et al., 2013[B,D]	Dual N-back task	Visual search	1	3	33%	1	3	33%	1	3	33%	1	3	33%
6	Stephenson & Halpern, 2013[A,B]	Dual N-back task	Spatial matrix span												
7	Thompson et al., 2013	Dual N-back task	Multiple object tracking	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for adults who received N-back training compared with an active-control condition			2	10	20%	2	10	20%	2	10	20%	2	10	20%

(*continued*)

Table 8.12. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
8	Kundu et al., 2013[B]	Dual N-back task	Tetris	0	2	0%	0	2	0%	0	2	0%	0	2	0%
	Grand Totals and Percents for adults and children who received N-back training compared with an active-control condition (excluding Kundu et al., 2013)			2	13	15%	2	13	15%	2	13	15%	2	13	15%
	Grand Totals and Percents for adults and children who received N-back training compared with any active-control condition (including Kundu et al., 2013)			2	15	13%	2	15	13%	2	15	13%	2	15	13%
	Grand Totals and Percents for adults and children who received N-back training compared with an active-control condition or no treatment (excluding Kundu et al., 2013)			3	18	17%	3	18	17%	3	18	17%	3	18	17%
	Grand Totals and Percents for adults and children who received N-back training compared with any active-control condition or no treatment (including Kundu et al., 2013)			3	20	15%	3	20	15%	3	20	15%	3	20	15%
				ADULTS (19–55 YEARS OLD)											
3	Chooi & Thompson 2012	Dual N-back task (8 or 20 sessions)	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
9	Jaeggi et al., 2008[B]	Dual N-back task (8, 12, 17, or 19 sessions)	No treatment	1	2	50%	1	2	50%	1	2	50%	1	2	50%

10	Jaeggi et al., 2010	Dual N-back task	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
10	Jaeggi et al., 2010	Single N-back task	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
11	Li et al., 2008	Single N-back ask	No treatment	2	3	67%	2	3	67%	2	3	67%	2	3	67%
4	Lilienthal et al., 2013	Dual N-back task	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
5	Redick et al., 2013[B]	Dual N-back task	No treatment	1	3	33%	0	3	0%	0	3	0%	0	3	0%
12	Rudebeck et al., 2012[A]	Dual N-back task	No treatment												
6	Stephenson & Halpern, 2013[A,B]	Dual N-back task	No treatment												
7	Thompson et al., 2013	Dual N-back task	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for adults 19–55 years old who received N-back training compared with no treatment			5	19	26%	4	19	21%	4	19	21%	4	19	21%
	Grand Totals and Percents for adults and children who received N-back training compared with no treatment			6	24	25%	5	24	21%	5	24	21%	5	24	21%
	Grand Totals and Percents for Adults (19–55 years old)			7	34	21%	6	34	18%	6	34	18%	6	34	18%
				OLDER ADULTS (OLDER THAN 55 YEARS)											
11	Li et al., 2008	Single N-back task	No treatment	2	3	67%	2	3	67%	2	3	67%	2	3	67%

(continued)

Table 8.12. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
13	Stepankova et al., 2014[B]	Single N-back task (10 or 20 sessions)	No treatment	2	2	100%	2	2	100%	2	2	100%	2	2	100%
	Totals and Percents for older adults who received N-back training compared with no treatment			4	5	80%	4	5	80%	4	5	80%	4	5	80%
	Grand Totals and Percents across all ages who received N-back training compared with no treatment			10	29	34%	9	29	31%	9	29	31%	9	29	31%
	Grand Totals and Percents across all N-back studies (excluding Kundu et al., 2013)			12	42	29%	11	42	26%	11	42	26%	11	42	26%
	Grand Totals and Percents across all N-back studies (including Kundu et al., 2013)			12	44	27%	11	44	25%	11	44	25%	11	44	25%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.12 (although they are mentioned in the text) but results for all other EF measures are included here.

[A] This study did not include any near-transfer measures. They only looked at R/FL measures.

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[C] Studies that varied the number of training sessions found no difference by number of sessions on anything relevant to this table, so results across those different conditions are collapsed here.

[D] The one significant difference here was because the control group mysteriously got worse at posttest on the running letter span task, while those who trained on N-back with difficulty increasing (as well as the no-treatment group) improved.

[E] One might plausibly expect EF benefits from playing Tetris, so a failure to find a difference here might be due to both interventions' being beneficial, rather than N-back training's being ineffectual, thus we have not included the null findings here when calculating totals or percentages, except for the last line.

Far-Transfer Results for N-Back Training

Every N-back study but one looked for evidence of far transfer. Of those 12 studies (of 13), three (25%) found clear evidence of far transfer (Jaeggi et al., 2010, on Raven's and less so on BOMAT; Rudebeck et al., 2012, on BOMAT; Stepankova et al., 2014, on Matrix Design from WAIS-III). Two found only the slightest hint (Jaeggi et al., 2008; Stephenson & Halpern, 2013). Note that neither Rudebeck et al., Stepankova et al., Jaeggi et al., nor Stephenson and Halpern corrected for multiple comparisons; it is unclear which of their findings, if any, would remain significant had they done that.

Table 8.12, which presents the percentage across all EF measures (except reasoning/fluid intelligence) on which persons trained on N-back showed more improvement and/or better posttest results than comparison groups across all studies and ages, broken down by study, appears online.

Although N-back performance has been found to be fairly highly correlated with performance on fluid-intelligence or reasoning tasks (Gray, Chabris, & Braver, 2003; Jaeggi et al., 2010; Kane, Conway, Miura, & Colflesh, 2007; Schmiedek, Hildebrandt, Lövdén, Lindenberger, & Wilhelm, 2009), of the 11 N-back training studies that looked for far transfer to tests of fluid intelligence or reasoning, most (55%) found none (Chooi & Thompson, 2012; Jaeggi, Buschkuehl, Jonides, & Shah, 2011; Kundu et al., 2013; Pugin et al., 2014; Redick et al., 2013; Thompson et al., 2013). Three of the five that found benefits looked at more than one fluid-intelligence/reasoning measure: One found clear benefits on both measures (Jaeggi et al., 2010). One found some benefit on one measure but none on the other (Jaeggi et al., 2008). The third found more improvement on two of four measures and no better posttest scores on any (Stephenson & Halpern, 2013).

Only one study (Stephenson & Halpern 2013) with an active control group (out of six) found more improvement or better posttest performance on any measure of fluid intelligence or reasoning after N-back training compared to controls. Of the five N-back studies with only a no-treatment control group that looked at fluid-intelligence or reasoning outcomes, four (80%) found suggestive evidence of such far transfer. Redick et al. (2013) found neither more improvement nor better posttest performance from N-back training on any of several fluid-intelligence/reasoning measures (including Raven's Matrices and a test of inferences and analogies) compared to controls trained on a visual search task. This is despite their N-back training continuing for longer and having lengthier and more frequent sessions than any other N-back training study, and even though they and Lilienthal et al. (2013) found the strongest near-transfer results of any N-back study with an active control group. They also found no benefits on measures of multitasking, although training had been with dual-task N-back.

Chooi and Thompson (2012) found neither more improvement nor better posttest performance on Raven's Matrices after N-back training than after sham N-back training or no treatment. However, Chooi and Thompson had a very high attrition rate (35%) and allowed participants to switch groups. Twenty-five percent of those assigned to N-back training or the active-control condition (nonadaptive N-back training) opted to transfer into the no-treatment group. Hence, one should view their results with caution.

Jaeggi et al. (2011) found no benefits on Raven's Matrices from N-back training versus computerized training on knowledge and vocabulary items. They did find, however, that those who improved most during the N-back training also improved most on Raven's and that subset of subjects improved more on Raven's than did subjects in the active-control condition. Pugin et al. (2014) found no benefits on the TONI test of nonverbal fluid intelligence compared to no-treatment controls.

Turning to the five studies with only a no-treatment control group that found some benefits to fluid intelligence/reasoning, Jaeggi et al. (2010) found both more improvement and better posttest performance on Raven's Matrices whether training was with the regular N-back task or a dual N-back paradigm (both groups were considered experimental groups by the authors) compared to no-treatment controls. On the Bochum Matrices Test (BOMAT), however, while those trained with the regular N-back task showed a benefit, those trained on the dual-task version did not.

Jaeggi et al. (2008) found the opposite: No benefit on Raven's (which was administered to those trained on the regular N-back task) but significant improvement on the BOMAT (which was administered to those trained with the dual N-back paradigm) compared to no-treatment controls.[7]

Rudebeck et al. (2012) administered only the BOMAT and found both more improvement and better posttest performance compared to no-treatment controls.[6] That was true for those who showed much improvement during N-back training and also for those who showed little.

Stephenson and Halpern (2013) administered Raven's, Beta-III Matrix Reasoning, Wechsler Abbreviated Scale of Intelligence (WASI) Matrix Reasoning, and Cattell. They found that those who trained on dual N-back improved more on Beta-III than those who trained on a single auditory N-back task (but not a single visual N-back), those who trained on a complex-span task, and no-treatment controls. Those who trained on the dual N-back, the visual single N-back, or the complex-span task improved more on Raven's than did no-treatment controls.[6] All other results for fluid-intelligence measures were

[7] Corrections for multiple comparisons were not made; not all their positive results might still be significant had those corrections been made.

negative: There were no group differences on WASI or Cattell, no group difference on posttest scores for any measures, and no other differences between groups on Beta-III or Raven's.

Stepankova et al. (2014) found both more improvement and better posttest performance on Matrix Design (from WAIS-III) than no-treatment controls whether the verbal N-back training was conducted twice a week (for a total of 10 sessions) or four times a week (for a total of 20 sessions).[6]

Results for Near Transfer to WM Tests After N-Back Training

Two N-back training studies with young adults found a benefit on one complex-span task but not on others (compared to no-treatment controls or active controls who trained on a nonadaptive version of the N-back task (Lilienthal et al., 2013) or on a visual-search task (Redick et al., 2013). Redick et al. included only two complex-span tasks, but Lilienthal et al. included four and found a benefit on only one. The one N-back training study with older adults to look at transfer to complex-span tasks found none. It has been found repeatedly that performance on N-back and complex-span tasks is only very weakly correlated (see Jaeggi et al., 2010; Kane et al., 2007; Oberauer, 2005; Roberts & Gibson, 2002; for a meta-analysis, see Redick & Lindsey, 2013). Hence, it is not too surprising that only two out of the eight studies that looked at whether N-back training improved complex-span performance found that it did.

Of the 10 N-back studies that looked at transfer to other WM measures, 60% found benefits. Stepankova et al. (2014) found older adults showed greater improvement and better posttest performance on both a Forward + backward combined digit span task and Letter-Number Sequencing.[7] Jaeggi et al. (2008) found both more improvement and better posttest scores from N-back training (whether regular or dual N-back) on backwards digit span compared to no-treatment controls regardless of whether the number of N-back training sessions was anywhere from 8 to 19.

In contrast, Pugin et al. (2014) found that adolescents (10–16 years old) showed no benefit on Letter-Number Sequencing from N-back training, although on a measure of very narrow transfer (training on a visuospatial N-back and testing on an auditory N-back) they showed more benefit than no-treatment controls. Likewise, Li et al. (2008) found very narrow transfer from N-back training. Li's group trained younger (age 21–30) and older adults (age 70–80) on N-back. Compared to no-treatment controls, those trained on N-back in both age groups improved more and performed better at posttest on a spatial N-back and numerical N-back task. The only memory measure Rudebeck et al. (2012) included assessed episodic memory (non-EF); they found no group difference on that when comparing N-back training to no treatment. They did find, however, that those who improved most during N-back training also improved the most from pre- to posttest on episodic memory. Jaeggi et al. (2008) found no benefit from N-back training (whether regular or dual N-back) on backward digit span compared to no-treatment controls regardless of the number of N-back training sessions (within the range of 8–19 sessions).

In general, results are not encouraging for N-back training's improving performance on complex-span tasks or other WM measures and decidedly mixed for whether it improves reasoning/fluid intelligence. It does improve performance on other N-back tasks. These conclusions apply both to training on a regular, single N-back task and to training on a dual-task variant.

Complex-Span Tasks

Complex-span tasks are explained in the section "EFs Explained" above. WM benefits after computerized complex-span training have generally been better than those after N-back training but not as good as those after Cogmed training (see Table 8.8a). Looking at all EF outcomes (including reasoning/fluid intelligence), computerized complex-span results look more on a par with those for N-back training but fall even farther short of Cogmed than when only WM outcomes are considered (see Tables 8.1 and 8.2).

The complex-span training administered by Loosli et al. (2012) was extremely brief and consisted of ten 12-min sessions (once per day, 5 days a week, for only 2 weeks). Participants were 10 years old. Loosli et al. tested no near-transfer measures and found no benefits on a measure of reasoning/fluid intelligence (TONI). Indeed, although all six complex-span studies looked at far transfer to reasoning/fluid intelligence, Borella et al. (2010) alone reported finding it. The study by Borella et al. is one of two papers reporting results from noncomputerized complex-span training. The other study is Carretti et al. (2013). (The two studies are discussed in the section below on noncomputerized cognitive training.) Except for these two studies from Borella and Carretti's group, no two studies trained on the same complex-span tasks. In sum, complex-span training improves performance on complex-span tasks. It does not improve reasoning/fluid intelligence.

Training on complex-span tasks was not much longer in other studies (15–20 sessions total—comparable to most N-back studies). Two had active-control conditions: Harrison et al. (2013) had two active control groups (simple span and visual search, both of which kept increasing in difficulty). Richmond et al. (2011) had one active control group (trivia learning). The Harrison et al. study had a high attrition rate (37%). None of the other complex-span studies provided data on attrition.

Harrison et al. found that young adults (mean age = 20 years) showed more improvement and performed better at posttest on two other complex-span tasks (reading span and rotation span) when compared to either active control group.

In addition, they performed better than the visual-search control group on the running letter, running spatial, and Keep Track complex-span tasks (although on the Keep Track Task this seems to be due to the visual-search group mysteriously getting worse). Harrison et al. found no transfer on the word or arrow span tasks. Thus, they found benefits on four of six untrained complex-span tasks (67%). They did not correct for multiple comparisons, however. It is unclear which results would have remained significant had they done that. See Tables 8.13 and 8.14.

Richmond et al. (2011) found more improvement and better posttest scores on a reading span task and on repetitions in the California Verbal Learning Task (CVLT) among older adults (mean age = 66 years) trained on complex-span tasks compared to active controls, but no benefit on CVLT accuracy or intrusion errors nor on Backward Digit Span or the Test of Everyday Attention.

Chein and Morrison (2010) found more improvement, but not better posttest scores, on the Stroop task in young adults (mean age = 20 years) trained on complex-span tasks than no-treatment controls.

Table 8.14, which presents the percentage across all EF measures (except reasoning/fluid intelligence) on which persons trained on N-back showed more improvement and/or better posttest results than comparison groups across all studies and ages, broken down by study, appears online.

Task Switching

Task switching, also called set-shifting, involves going back and forth between doing one task and doing another. Typically, although not always, all stimuli in a task-switching task are relevant to each task so participants must switch how they think about the stimuli or what aspect of a stimulus they focus on when switching between tasks.

For near transfer for task switching, we included all three core EFs (inhibitory control, WM, and cognitive flexibility) because all are required for task switching. Note that for Cogmed, N-back, and complex span we only included WM under near transfer. Even so, when studies with possibly spurious positive results are omitted, task switching shows better results for both the number of measures on which more improvement was found and the number of measures on which better posttest performance was found after training compared to the control group, and task switching comes in second, just behind Cogmed, in the percentage of studies finding at least suggestive evidence of near-transfer benefits (see Table 8.8a).

Three task-switching training studies were from Kray's lab (see Table 8.15). By far the best results were found in the first study: Karbach and Kray (2009) found task-switching training (vs. training on a single task with no switching) showed very-near-transfer improvements—there was improved task switching on other

Table 8.13. Percentage of WM Measures on Which Persons Who Received Complex-Span Training Showed More Improvement and/or Better Posttest Results Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
1	Loosli et al., 2012[A]	Complex Span	No treatment												
				ADULTS (18–55) YEARS OLD											
2	Harrison et al., 2013[B]	Complex Span	Adaptive simple span training	1	6	17%	1	6	17%	1	6	17%	1	6	17%
2	Harrison et al., 2013[B]	Complex Span	Adaptive visual-search training	4	6	67%	4	6	67%	4	6	67%	4	6	67%
3	Chein & Morrison, 2010[C]	Computerized adaptive verbal and spatial complex WM span task	No treatment												
	Grand Totals and Percents for adults who received complex-span training compared with an active-control condition			5	12	42%	5	12	42%	5	12	42%	5	12	42%
	Grand Totals and Percents for adults who received complex-span training compared with an active-control condition or no treatment			5	12	42%	5	12	42%	5	12	42%	5	12	42%

				OLDER ADULTS (OLDER THAN 55)											
4	Borella et al., 2010	Complex Span (noncomputerized)	Fill-in paper-and-pencil questionnaires	3	3	100%	3	3	100%	3	3	100%	3	3	100%
5	Carretti et al., 2013	Complex Span (noncomputerized)	Fill-in paper-and-pencil questionnaires	1	1	100%	1	1	100%	1	1	100%	1	1	100%
6	Richmond et al., 2011	Complex Span	Trivia Learning	2	6	33%	2	6	33%	2	6	33%	2	6	33%
	Grand Totals and Percents for older adults who received complex-span training compared with an active control condition			6	10	60%	6	10	60%	6	10	60%	6	10	60%
	Grand Totals and Percents for adults who received complex-span training compared with an active control condition			11	22	50%	11	22	50%	11	22	50%	11	22	50%
	Grand Totals and Percents for adults who received computerized complex-span training compared with an active-control condition or no treatment			7	18	39%	7	18	39%	7	18	39%	7	18	39%
	Grand Totals and Percents for adults who received complex-span training (computerized or noncomputerized) compared with an active-control condition or no treatment			11	22	50%	11	22	50%	11	22	50%	11	22	50%

Note. Results for far-transfer measures, such as reasoning/fluid intelligence are not included here.

[A] Loosli et al. (2012) did not include any near-transfer measures.

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[C] Chein and Morrison (2010) did not test the difference between posttest scores, and from their figure we are not able to tell if posttest scores were significantly different.

Table 8.14. Percentage of EF Measures (Except Reasoning Fluid Intelligence) on Which Persons Who Received Complex-Span Training Showed More Improvement and/or Better Posttest Results Than Comparison Groups Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
1	Loosli et al., 2012[A]	Complex Span	No treatment												
				ADULTS (18–55) YEARS OLD											
2	Harrison et al., 2013[B]	Complex Span	Adaptive simple span training	1	7	14%	1	7	14%	1	7	14%	1	7	14%
2	Harrison et al., 2013[B]	Complex Span	Adaptive visual-search training	4	7	57%	4	7	57%	4	7	57%	4	7	57%
3	Chein & Morrison, 2010[C]	Computerized adaptive verbal and spatial complex WM span task	No treatment	1	1	100%									
	Grand Totals and Percents for adults who received complex-span training compared with an active-control condition			5	14	36%	5	14	36%	5	14	36%	5	14	36%
	Grand Totals and Percents for adults who received complex-span training compared with an active-control condition or no treatment			6	15	40%	5	14	36%	5	14	36%	5	14	36%

					OLDER ADULTS (OLDER THAN 55)										
4	Borella et al., 2010	Complex Span (noncomputerized)	Fill-in paper-and-pencil questionnaires	3	3	100%	3	3	100%	3	3	100%	3	3	100%
5	Carretti et al., 2013	Complex Span (noncomputerized)	Fill-in paper-and-pencil questionnaires	1	1	100%	1	1	100%	1	1	100%	1	1	100%
6	Richmond et al., 2011	Complex Span	Trivia Learning	2	6	33%	2	6	33%	2	6	33%	2	6	33%
	Grand Totals and Percents for older adults who received complex-span training compared with an active-control condition			6	10	60%	6	10	60%	6	10	60%	6	10	60%
	Grand Totals and Percents for adults who received complex-span training compared with an active-control condition			11	24	46%	11	24	46%	11	24	46%	11	24	46%
	Grand Totals and Percents for adults who received computerized complex-span training compared with an active-control condition or no treatment			8	21	38%	7	20	35%	7	20	35%	7	20	35%
	Grand Totals and Percents for adults who received complex-span training (computerized or noncomputerized) compared with an active-control condition or no treatment			12	25	48%	11	24	46%	11	24	46%	11	24	46%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.14 (although they are mentioned in the text) but results for all other EF measures are included here.

[A] Loosli et al. (2012) only included measures of R/FL.

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[C] Chein & Morrison (2010) did not test the difference between posttest scores, and from their figures we are not able to tell if posttest scores were significantly different.

Table 8.15. Percentage of Near-Transfer Measures on Which Persons Who Received Task-Switching Training Showed More Improvement and/or Better Posttest Results Than Comparison Groups Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest only including measures where this was looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
			SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH NO CLINICAL DIAGNOSIS												
1	Dörrenbächer et al., 2014	Task switching in a low- or high-motivational setting	Single-task training in a low- or high-motivational setting	0	4	0%	0	4	0%	0	4	0%	0	4	0%
2	Karbach & Kray, 2009	Task switching	Single-task training	5	5	100%	1	5	20%	1	1	100%	1	5	20%
3	Zinke et al., 2012[A]	Task switching & task switching + exercise group	Exercise on a stationary bike only & no treatment	1	9	11%	1	9	0%	1	9	0%	1	9	0%
	Totals and Percents for typically developing children who trained on task switching compared with single-task training			5	9	56%	1	9	11%	1	5	20%	1	9	11%
	Totals and Percents for typically developing children who trained on task-switching compared with any active-control condition			6	18	33%	2	18	11%	2	14	14%	2	18	11%
3	Zinke et al., 2012[A]	Task switching & task switching + exercise group	No treatment	1	9	11%	1	9	0%	1	9	0%	1	9	0%
	Totals and Percents for typically developing children who trained on task switching compared with any active-control condition or no treatment			7	27	26%	3	27	11%	3	23	13%	3	27	11%

	SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH ADHD														
4	Kray et al., 2012	Task switching	Single-task training	2	4	50%	2	4	50%	2	4	50%	2	4	50%
	Grand Totals and Percents for school-age children (with or without a clinical diagnosis) who trained on task switching compared with single-task training			7	13	54%	3	13	23%	3	9	33%	3	13	23%
	Grand Totals and Percents for school-age children (with or without a clinical diagnosis) who trained on Task-switching compared with any active control condition			8	22	40%	4	22	20%	4	18	24%	4	22	20%
	Grand Totals and Percents for school age children (with or without a clinical diagnosis) who trained on task switching compared with any active-ontrol condition or no treatment			9	31	32%	5	31	18%	5	27	20%	5	31	18%
	ADULTS (18–55 YEARS OLD)														
2	Karbach & Kray, 2009	Task switching	Single-task training	5	5	100%	2	5	40%	2	3	67%	2	5	40%
5	Pereg et al., 2013[A,B]	Karbach & Kray's (2009) Task-switching training + verbal self-instruction + training variability	No treatment	2	6	33%	2	6	33%	2	6	33%	2	6	33%
	Totals and Percents for younger adults who trained on task switching compared with single-task training or no treatment			7	11	64%	4	11	36%	4	9	44%	4	11	36%
	OLDER ADULTS (OLDER THAN 55 YEARS)														
2	Karbach & Kray, 2009	Task switching	Single-task training	5	5	100%	1	5	20%	1	2	50%	1	5	20%

(continued)

Table 8.15. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest only including measures where this was looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Total and Percents across all task-switching studies that used single-task training as an active-ontrol condition			17	23	74%	6	23	26%	6	14	43%	6	23	26%
	Grand Total and Percents across all task-switching studies that used an active-ontrol condition			18	32	59%	7	32	22%	7	23	29%	7	32	22%
	Grand Total and Percents across all task-switching studies compared to no treatment			3	15	20%	3	15	20%	3	15	20%	3	15	20%
	Grand Total and Percents across all task-switching studies (including no-treatment controls)			21	47	45%	10	47	21%	10	38	26%	10	47	21%

Note. The only far-transfer EF measures in studies of task switching were of reasoning/fluid intelligence (R/FL); therefore, a second table that included near and far transfer excluding R/FL would be identical to this table and so is not included.

[A] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[B] The positive improvement and posttest result for the Manual Stroop task in Pereg et al. (2013) is due to the control group's mysteriously getting worse, not because of much improvement by the experimental group.

tasks (both reduced mixing costs and reduced switching costs) in 8– to 10-year-old children and younger and older adults. The training also produced greater improvements on the Stroop task and on Raven's Matrices for children, and on both verbal and spatial memory for children and young adults compared with controls (although it is unclear if posttest performance differences were significant). Therefore, children seemed to show the most benefits and older adults the least.

Study 2: Kray, Karbach, Haenig, and Freitag (2012) found task-switching training improved performance more than single-task training on task switching on other tasks (very near transfer), including switching on the Stroop task, among 8 to 12-year-old boys with ADHD, but benefits did not transfer to WM (re-ordering digits) or to matrix reasoning/fluid intelligence (Raven's Matrices; unlike what Karbach & Kray had found). Of concern is that there were only 10 subjects per group and attrition was high (33%).

Study 3: Dörrenbächer, Müller, Tröger, and Kray (2014) found that task-switching training improved switching costs on other tasks (very near transfer) more than did single-task training (though not mixing costs) for 8– to 11-year-olds trained in a highly motivating context but not in a low motivational context. Benefits did not transfer to WM (as assessed by an AX-CPT, N-back task, Backward Digit Span, and counting span) or to inhibitory control as indexed by the Stroop test (vs. Kray et al., 2012, who found a benefit on Stroop).

Two studies are from other labs. Pereg, Shahar, and Meiran (2013) found that benefits for young adults (mean age = 24 years) from task-switching training (vs. no treatment) on alternative-runs task switching did not transfer to cued task switching (very near transfer) nor to the Stroop or N-back tasks. (On the verbal Stroop task, there was clearly no benefit; on a manual Stroop task, the appearance of a benefit was due to the no-treatment group's mysteriously getting worse.)

Zinke, Einert, Pfenning, and Kliegel (2012) looked at task-switching training (modeled after Karbach and Kray) alone or preceded by cycling on a stationary bike versus just that aerobic exercise alone or no treatment in early adolescents (mean = 12 years; age range = 10–14 years). Results for the two task-switching conditions were similar. They found that task-switching training improved task switching on a nontrained task (mixing costs were more improved and better at posttest than for those who only exercised or had no treatment). However, task-switching training did not improve inhibitory control or WM as indexed by the N-back, Flanker, and Stroop tests.

Computerized Cognitive Training Using Commercial Brain-Training Products (other than Cogmed) and One Noncommercial Product ("BrainGame Brian")

We found two peer-reviewed studies each of Brainware Safari® and three of Lumosity®, and one each of Rise of Nations®, Wii Big Brain Academy®, Neuroracer, and Posit Science*. The two studies of Brainware Safari, the study of Rise of Nations, and one of the studies of Lumosity included only no-treatment controls. The studies of Brainware Safari were done in children, the two studies of Lumosity were done with young to middle-aged adults, and the studies of Neuroracer, Rise of Nations, Posit Science, and Wii Big Brain Academy were done with older adults.

Unfortunately, most of these studies looked at benefits only immediately after training, training was quite brief, and half had no active control group. Given those caveats, after only 10 to 12 weeks of only 15 to 30 min of practice 3 to 5 times a week, studies of Lumosity and Avtzon's (2012) study of Brainware Safari report some benefits worthy of note, as does Rise of Nations after a similar number of hours of training but over only 4 to 5 weeks. In more rigorous studies than all the others, after only 5 weeks of BrainGame Brian (done 35 to 50 min a day), several benefits were noted, and after only 4 weeks of Neuroracer (done 60 min three times a week), mixed results were found.

Basak et al. (2008) found that playing Rise of Nations (a real-time strategy videogame) for 24 hours (roughly 16 90-min sessions over 4–5 weeks), but not for 11 hours (roughly seven 90-min sessions over 2–3 weeks), improved cognitive flexibility (task switching), WM + inhibitory control (N-back task), and fluid intelligence/reasoning (Raven's Matrices), although not inhibition as assessed by the Stop-Signal task or WM + interference control as assessed by the Operation Span task, compared with no-treatment controls (mean age of 69 years in both groups).

Wii Big Brain Academy is a video game that presents puzzles meant to challenge logic, reasoning, math computations, and memory. Ackerman, Kanfer, and Calderwood (2010) found that 8 weeks of Wii Big Brain Academy (60 min, five times a week) produced no greater benefits to fluid intelligence/reasoning, crystallized intelligence, or speed of processing than reading in adults 50 to 71 years old.

Kesler et al. (2013) found that immediately after 12 weeks of Lumosity training, women with a mean age of 56 years who were breast cancer survivors (18 months post-chemotherapy) performed better and had improved more on the Wisconsin Card Sort and Verbal Fluency (both of which require multiple EF skills) than those in the no-treatment group. In another study with almost 5,000 adults (mean age of 39 years) all of whom wanted Lumosity but some of whom were assigned to do online crossword puzzles, on almost every outcome measure those who trained on Lumosity for 10 weeks improved significantly more than controls—which is hardly surprising with so many subjects (Hardy et al.02, 2015). The effect sizes for Backward Digit Span (WM) and Raven's Matrices (visuospatial reasoning) were tiny, and controls improved more on grammatical reasoning. Slighty larger effect sizes were found for go/no-go, which assesses inhibitory control (0.16, still minimal) and for arithmetic reasoning and a composite of all the outcome measures (~ 0.25). Whether the groups differed in posttest performance on any measure was not reported.

Just as we thought this was going to press (too late to be included in our tables or tabulations), a study of Lumosity with 128 young adults was accepted for publication (Kable et al., 2017). For 10 weeks, 30 min per day, 5 days a week, participants either trained on Lumosity or played video games. On no outcome measure did posttest performance differ significantly among the groups. However, those who trained on Lumosity showed more improvement than those who played video games or no-treatment controls on the N-back task (which requires WM plus inhibitory control) and reduced false-positives on a CPT (which requires sustained attention), although there was no difference in improvement in task switching, decision-making, delay discounting, or on the Stroop or Stop-Signal tests compared to business-as-usual or video-game participants.

Unlike Rise of Nations, Lumosity, and Wii Big Brain Academy, Brainware Safari is intended for children. Avtzon (2012) reports that 9-year-olds with learning disabilities who played Brainware Safari video games for 12 weeks (30 min per day, 5 days a week) improved more and achieved better posttest scores in verbal and visuospatial WM and on a composite index of EFs than did no-treatment controls. Helms and Sawtelle (2007), who did not report pre- or posttest scores (so we cannot tell if improvements simply reflect catching up to controls), found that 11-year-olds who played Brainware Safari showed more improvement on two measures of WM (Backward Digit Span and auditory WM), planning, and concept formation, although no better math or reading fluency or comprehension than their peers who did not play Brainware Safari.

Clearly, what is needed is a higher bar than no-treatment controls, assessment on more diverse skills, and assessments months and years after training (rather than only immediately after) to determine the potential benefits, and limits to benefits, of any of these commercial products. Also, Basak et al. (2008; Rise of Nations) and Hardy et al. (2015; Lumosity) neglected to correct for multiple comparisons in their data analyses.

By contrast, the study by Dovis et al. (2015) of the noncommercial BrainGame Brian (BGB) is one of the most rigorous studies reviewed here. BGB has WM, inhibitory control, and cognitive flexibility training components. One group of 8- to 12-year-old children with ADHD played BGB with the three components; Group 2 got a version with the latter two components, but the WM component did not increase in difficulty; and Group 3 received nonincrementing versions of all three components. Groups 1 and 2 improved more on the Stop-Signal task (a measure of inhibition) than Group 3 and maintained that benefit 3 months later. Group 1 improved more on Corsi Blocks (a measure of visuospatial WM) than Group 3, with that benefit slightly reduced three months later. Neither Group 1 nor Group 2 improved more than Group 3 on the Stroop task (an inhibitory control measure), Digit Recall (a composite WM-STM measure), Trail-Making (cognitive flexibility), Raven's Matrices (reasoning/fluid intelligence), or parent or teacher ratings of ADHD behaviors. No significant differences in benefits were found for Group 1 versus Group 2.

That followed a pilot study by van der Oord et al. (2014), also with medicated ADHD children, but comparing them only to wait-list controls. Parents, who were not blind to group assignment, felt the children who did BGB improved more on EFs in general. This study is not included in our tabulations because the researchers only used questionnaires and/or self-reports.

In the only test of BGB with children on the autism spectrum, one group was trained on just the WM component, another on just the cognitive flexibility component, and a third group on a mock training control condition where difficulty did not increase (de Vries et al., 2014). No greater gains were found for either BGB condition compared with the control condition.

Anguera et al. (2013) evaluated EF benefits for older adults (mean age = 67, range = 60–85 years) of multitask training using their Neuroracer video game. The two tasks in the video game were to drive a car on a winding road and to respond to a sign only when a green circle was also visible. The active control group did each of the tasks one at a time, dividing their time between the two. Thus, since both groups spent the same amount of time, the control group spent half as much time doing either task. Anguera et al. pointed out that "difficulty was maintained using an adaptive staircase algorithm to independently adjust the difficulty of the 'sign' and 'driving' tasks following each 3-min run based on task performance" and that task engagement on both the multi- and single-task versions "was motivated by rewards given only when both component tasks improved beyond 80% on a given run" (p. 98).

Anguera et al. (2013) found that after 4 weeks, those who played the multitasking version improved more in (a) RT variability, but not in RT, on the TOVA test (a measure of sustained attention) and (b) delayed recognition with distraction but not delayed recognition attending to distractors (both measures of WM plus selective attention) than those who played the single-task version or no-treatment controls. The group that trained on multitasking did not improve more in visual WM (Change Detection task), visuospatial WM (Filter task), selective attention (Useful Field of View), or on either of their measures of speed of processing. Posttest scores were not provided, so do not know if there was any group difference on those. Testing at 6 months after training is mentioned, but nothing about performance is provided for that time point.

Being able to drive on a winding road while also attending to traffic lights and road signs is an important real-world skill. Anguera et al. (2013) claim impressive gains in the video version of this from playing Neuroracer. An excellent study would be to put a dash cam on the car of each trainee and see if training on the video game transfers to real-world benefits when behind the wheel.

Barnes et al. (2013) trained older adults (mean age = 74 years) on visual and auditory processing tasks emphasizing both speed and accuracy from the Posit Science task battery. (The Posit Science Corporation was started by neuroscientists Merzenich and Mahncke [Mahncke, Bronstone, & Merzenich, 2006].) Barnes et al.'s active control group watched DVDs of educational lectures. Both the experimental and active-control conditions lasted 12 weeks, with three 60-min sessions per week. Half of each group was also assigned to an aerobic exercise program or stretching and toning, but they collapsed across the exercise conditions in reporting EF outcomes. On Verbal Fluency (whether letter or category), Trails B, or the Flanker task, there were no group differences. Only on the Useful Field of View test (on indices of both selective and divided attention) was there more improvement in the Posit Science group than in the control group.

Other Types of Cognitive Training, Both Computerized and Noncomputerized

Twenty-nine (29) studies fall into this "other" category. Over 50% of the studies (17 studies) found at least suggestive evidence of EF benefits (see Table 8.16). The most impressive results were found for noncomputerized complex-span training (Borella et al., 2010; Carretti et al., 2013). Also noteworthy are three other studies: Röthlisberger, Neuenschwander, Cimeli, Michel, and Roebers (2012) trained children on noncomputerized laboratory EF tests and found more improvement and better posttest results on three of their five untrained EF tasks compared to no-treatment controls. The Advanced Cognitive Training for Independent and Vital Elderly (ACTIVE) study trained older adults on reasoning using noncomputerized cognitive training (Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006). The study found that the benefits to reasoning on nontrained measures was still evident even 5 years later, and the effect size 1 year after training was an impressive 0.40. Johnstone et al. (2012) administered computerized training on self-ordered pointing and go/no-go to children. Although children without a clinical diagnosis improved on only two out of five of the EF outcome measures compared to no-treatment controls, and children with ADHD improved more, and performed better at posttest, on four of the five EF outcome measures (80%) compared to no-treatment controls.

Both studies that had people train on things directly related to their real-world activities found at least suggestive evidence of EF benefits (the ACTIVE study and Wang, Chang, & Su, 2011).

Table 8.16. Percentage of Near-Transfer EF Measures on Which Persons Who Received Other Cognitive Training Showed More Improvement and/or Better Posttest Results Across All Studies And Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
YOUNG CHILDREN (< 7 YEARS OLD) WITH NO CLINICAL DIAGNOSIS															
1	Blakey & Carroll, 2015	Training in WM and inhibitory control	Training in perceptual judgments	1	4	25%	1	4	25%	1	4	25%	1	4	25%
2	Kroesbergen et al., 2014	*Domain-general WM training*	Domain-specific WM training	0	2	0%	0	2	0%	0	1	0%	0	2	0%
3	Rueda et al., 2005	Inhibition training: younger children	Watched children's videos	0	2	0%	0	2	0%	0	2	0%	0	2	0%
3	Rueda et al., 2005	Inhibition training: older children	Watched children's videos	0	2	0%	0	2	0%	0	2	0%	0	2	0%
4	Rueda et al., 2012	Inhibition training	Watched children's videos	0	3	0%	0	3	0%	0	3	0%	0	3	0%
5	Wass et al., 2011	Visual-attention training of infants	Viewed TV clips and images	3	4	75%	3	4	75%	3	4	75%	3	4	75%
2	Kroesbergen et al., 2014	*Domain-general WM training*	No treatment	1	2	50%	0	2	0%				0	2	0%

2	Kroesbergen et al., 2014	*Domain-specific WM training*	No treatment	1	2	50%	0	2	0%				0	2	0%
6	Kyttälä et al., 2015[A]	*WM and counting training*	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
7	Röthlisberger et al., 2012: xxx5-year-olds	*Training on EF laboratory tasks (including Stroop, Card Sort, Trail-Making, and Grass-Snow)*	No treatment	3	5	60%	3	5	60%	3	5	60%	3	5	60%
7	Röthlisberger et al., 2012: xxx6-year-olds	*Training on EF laboratory tasks (including Stroop, Card Sort, Trail-Making, and Grass-Snow)*	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
			SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH NO CLINICAL DIAGNOSIS												
8	Mackey et al., 2011	Computerized & noncomputerized reasoning training	Computerized & noncomputerized speed training	1	1	100%	0	1	0%	0	1	0%	0	1	0%
9	Johnstone et al., 2012[B]	Computerized training on self-ordered pointing & go/no-go tasks	No treatment	2	5	40%	2	5	40%	2	5	40%	2	5	40%

(*continued*)

Table 8.16. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
10	Wong et al., 2014[B]	Computerized WM training (Visuospatial & Auditory)	No treatment	3	4	75%	1	4	25%	1	4	25%	1	4	25%
SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH ADHD OR A LEARNING DISABILITY															
11	Alloway et al., 2013	High-frequency Jungle Memory™ WM training	Low-frequency Jungle Memory™ WM Training	2	2	100%	0	2	0%	0	2	0%	0	2	0%
12	Tucha et al., 2011[B]	AixTent computerized attention training	Noncomputerized visual-perception training	1	8	13%	1	8	13%	1	8	13%	1	8	13%
11	Alloway et al., 2013	High-frequency Jungle Memory™ WM training	No treatment	2	2	100%	0	2	0%	0	2	0%	0	2	0%
11	Alloway et al., 2013	Low-frequency Jungle Memory™ WM Training	No treatment	0	2	0%	0	2	0%	0	2	0%	0	2	0%
9	Johnstone et al., 2012[B]	Computerized training on self-ordered pointing & go/no-go tasks	No treatment	4	5	80%	4	5	80%	4	5	80%	4	5	80%

13	Semrud-Clikeman et al., 1999	*Training on visual and auditory attention tasks*	No treatment (ADHD control group)	2	2	100%	2	2	100%	2	2	100%	2	2	100%
13	Semrud-Clikeman et al., 1999	*Training on visual and auditory attention tasks*	No treatment (typically developing)	2	2	100%	0	2	0%	0	2	0%	0	2	0%
14	Tamm et al., 2013[b]	*Pay Attention! noncomputerized intervention*	No treatment	4	8	50%	3	12	25%	3	9	33%	0	12	0%
			ADULTS (18–55 YEARS OLD)												
15	Owen et al., 2010	Web-based reasoning, planning, and problem-solving training	Web-based adaptive training in memory, attention, visuospatial processing, and math calculations	0	2	0%	0	2	0%	0	2	0%	0	2	0%
15	Owen et al., 2010	Web-based reasoning, planning, and problem-solving training	Answering obscure knowledge questions	0	1	0%	0	1	0%	0	1	0%	0	1	0%
15	Owen et al., 2010	Web-based training in memory, attention, visuospatial processing, and math calculations	Answering obscure knowledge questions	0	1	0%	0	1	0%	0	1	0%	0	1	0%

(*continued*)

Table 8.16. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
16	Penner et al., 2012[C]	Distributed-intensity BrainStim	High-intensity BrainStim WM training	3	7	43%	0	7	0%	0	7	0%	0	7	0%
17	Dahlin et al., 2008[B]	Letter memory, updating (verbal & nonverbal), and a complex-span task (Keep Track Task)	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
16	Penner et al., 2012[B,C]	High-intensity BrainStim WM training	No treatment	0	7	0%	0	7	0%	0	7	0%	0	7	0%
16	Penner et al., 2012[B]	Distributed-intensity BrainStim WM training	No treatment	3	7	43%	0	7	0%	0	7	0%	0	7	0%
18	Schmiedek et al., 2010, 2014[B,D]	Nonadaptive tasks of perceptual speed, episodic memory, WM updating, and complex-span tasks	No treatment	0	5	0%									

				OLDER ADULTS (OLDER THAN 55 YEARS)											
19	Borella et al., 2010	*Noncomputerized complex-span task*	Fill-in paper-and-pencil questionnaires	3	3	100%	3	3	100%	3	3	100%	3	3	100%
20	Buschkuehl et al., 2008	WM computerized training	Nonaerobic muscle training on recumbent bicycle	1	2	50%	1	2	50%	1	2	50%	1	2	50%
21	Carretti et al., 2013	*Noncomputerized complex-span task (categorization WM span)*	Fill-in paper-and-pencil questionnaires	1	1	100%	1	1	100%	1	1	100%	1	1	100%
22	Corbett et al., 2015	Web-based Reasoning and Problem-Solving Cognitive Training (ReaCT)	Web-based game where statements needed to be put in correct order	1	1	100%	0	1	0%				0	1	0%
22	Corbett et al., 2015	Web-based training in memory, attention, visuospatial processing, and math calculations	Web-based game where statements needed to be put in correct order	0	1	0%	0	1	0%				0	1	0%
23	Ball et al., 2002; **Rebok et al., 2014**, and Willis et al., 2006[D]	**Reasoning and problem-solving noncomputerized training including real-world tasks**	No treatment	1	1	100%									

(*continued*)

Table 8.16. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
24	Blieszner et al., 1981[E]	*Inductive reasoning training (noncomputerized)*	No treatment				1	1	100%	1	1	100%			
25	Cheng et al., 2012[F]	*Multidomain cognitive training (general EF training)*	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
25	Cheng et al., 2012[F]	*Single-domain cognitive training (reasoning)*	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
17	Dahlin, Nyberg, et al., 2008, Dahlin, Stigsdotter-Neely, et al. 2008[B]	Letter memory, updating (verbal & nonverbal), and a complex-span task (Keep Track Task)	No treatment	0	4	0%	0	4	0%	0	4	0%	0	4	0%
26	Plemons et al., 1978	*Visuospatial reasoning training (noncomputerized)*	No treatment	2	3	67%	1	3	33%	1	3	33%	1	3	33%

18	Schmiedek et al., 2010, 2014[a,b]	Nonadaptive tasks of perceptual speed, episodic memory, WM updating, and complex-span tasks	No treatment	2	5	40%								
27	Wang et al., 2011	<u>Cooking task computerized training</u>	Participated in other lab studies (no treatment)	1	2	50%	1	2	50%	1	2	50%	1	2	50%
28	Wilkinson & Yang, 2012	Stroop task computerized training	No treatment	0	2	0%	0	2	0%	0	2	0%	0	2	0%
29	Zinke et al., 2014	*WM and EF training*	No treatment	0	2	0%	0	2	0%	0	2	0%	0	2	0%
	Grand Total and Percents across all studies examining noncomputerized training compared with any active control condition (excluding Blakey & Carrol, 2015, and Mackey et al., 2011) ($N = 3$)			4	6	67%	4	6	67%	4	5	80%	4	6	67%
	Grand Total and Percents across all studies examining noncomputerized training compared with any active control condition (including Blakey & Carrol, 2015, and Mackey et al., 2011) ($N = 5$)			6	11	55%	5	11	45%	5	10	50%	5	11	45%
	Grand Total and Percents across all studies examining noncomputerized training compared with no treatment ($N = 10$)			19	43	44%	13	47	28%	13	40	33%	9	46	20%

(continued)

Table 8.16. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Total and Percents across all studies examining noncomputerized training (excluding Blakey & Carrol, 2015, and Mackey et al., 2011) compared with any active-control condition or no treatment ($N = 13$)			23	49	47%	17	53	32%	17	45	38%	13	52	25%
	Grand Total and Percents across all studies examining noncomputerized training (including Blakey & Carrol, 2015, and Mackey et al., 2011) compared with any active-control condition or no treatment ($N = 15$)			25	54	46%	18	58	31%	18	50	36%	14	57	25%
	Grand Total and Percents across all studies examining computerized training compared with any active-control condition ($N = 12$)			14	39	36%	7	43	16%	7	41	17%	7	43	16%
	Grand Total and Percents across all studies examining computerized training compared with no treatment ($N = 7$)			17	52	33%	8	42	19%	8	42	19%	8	42	19%
	Grand Total and Percents across all studies examining computerized training compared with any active-control condition or no treatment ($N = 19$)			31	91	34%	15	85	18%	15	83	18%	15	85	18%
	Grand Total and Percents across all studies for which participants trained on activities related to real-world activities compared with no treatment ($N = 2$)			2	3	67%	1	2	50%	1	2	50%	1	2	50%

Grand Total and Percents across all studies examining reasoning training compared with any active-control condition ($N = 3$)	2	5	40%	0	5	0%	0	4	0%	0	5	0%
Grand Total and Percents across all studies examining reasoning training compared with no treatment ($N = 4$)	4	8	50%	3	8	38%	3	8	38%	2	7	29%
Grand Total and Percents across all studies examining reasoning training compared with any active-control condition or no treatment ($N = 7$)	6	13	46%	3	13	23%	3	12	25%	2	12	17%
Grand Total and Percents across all studies examining inhibitory control training compared with any active-control condition ($N = 2$)	0	7	0%	0	7	0%	0	7	0%	0	7	0%
Grand Total and Percents across all studies examining inhibitory control training compared with any active-control condition or no treatment ($N = 3$)	0	9	0%	0	9	0%	0	9	0%	0	9	0%
Grand Total and Percents across all studies examining attention training compared with any active-control condition ($N = 2$)	4	9	46%	4	12	33%	4	12	33%	4	12	33%
Grand Total and Percents across all studies examining attention training compared with no treatment ($N = 2$)	8	12	67%	5	16	31%	5	13	38%	2	16	13%
Grand Total and Percents across all studies examining attention training compared with any active-control condition or no treatment ($N = 4$)	12	21	58%	9	28	32%	9	25	36%	6	28	21%

(continued)

Table 8.16. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Total and Percents across all studies examining WM training compared with any active-control condition ($N = 6$)			10	17	59%	5	17	29%	5	16	31%	5	17	29%
	Grand Total and Percents across all studies examining WM training compared with no treatment ($N = 9$)			13	47	28%	2	37	5%	2	33	6%	2	37	5%
	Grand Total and Percents across all studies examining WM training compared with any active-control condition or no treatment ($N = 15$)			23	64	36%	7	54	13%	7	49	14%	7	54	13%
	Grand Total and Percents across all studies examining training on multiple EFs compared with any active-control condition ($N = 4$)			2	8	25%	2	8	25%	2	7	29%	2	8	25%
	Grand Total and Percents across all studies examining training on multiple Efs compared with no treatment ($N = 4$)			11	26	42%	11	26	42%	11	26	42%	11	26	42%
	Grand Total and Percents across all studies examining training on multiple Efs compared with any active-control condition or no treatment ($N = 8$)			13	34	38%	13	34	38%	13	33	39%	13	34	38%

Grand Total and Percents across all studies compared with any active-control condition ($N = 15$)	18	45	40%	11	49	22%	11	46	24%	11	49	22%
Grand Total and Percents across all studies compared with no treatment ($N = 17$)	36	95	38%	21	89	24%	21	82	26%	17	88	19%
Grand Total and Percents across all studies compared with any active-control or no treatment condition ($N = 32$)	54	140	39%	32	138	23%	32	128	25%	28	137	20%

Note. For the condition of interest column: *Italic* font indicates a study that used noncomputerized cognitive training. Regular font indicates computerized cognitive training. An underline indicates training on "real-world" tasks. Red ink indicates reasoning training. Blue ink indicates inhibitory control training. Green ink indicates attention training. Brown ink indicates WM training. Violet ink indicates training on multiple EFs.

Borella et al. (2010) and Carretti et al. (2013) are included in this table because they used noncomputerized training. Their results are also presented in Tables 8.13 and 8.14, along with other studies of complex-span training.

[A] The WM training by Kyttälä, Kanerva, and Kroesbergen (2015) did not increase in difficulty (it was nonadaptive).

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[C] The authors call this an experimental group, but, for the purpose of this table, we are considering it an active control.

[D] This study did not test the difference between posttest scores.

[E] The difference in rate of improvement between groups was not tested.

[F] Cheng et al. (2012) noted that when they combined the two cognitive training groups, the results for two of the three measures (Stroop and Trails B) are true only for the roughly 55% who attended ≥ 80% of the training sessions.

Of the 12 studies of noncomputerized training, an impressive 75% found at least suggestive evidence of EF benefits,[8] as did one study that used both computerized and noncomputerized training (Mackey et al., 2011). The other study that used both types of training (Blakey & Carroll, 2015) found little evidence of improvement and no evidence of better performance at posttest compared to the active control group.

For studies of training on reasoning or on multiple EFs, stronger evidence of EF benefits was found if an intervention was compared to a no-treatment group than to an active control group. However, for studies of miscellaneous training on WM or studies using noncomputerized training, stronger evidence of EF benefits was actually found when an intervention was compared to an active control group than when it was compared to no treatment.

The most disappointing results were found by Owen et al. (2010). In a study with over 10,000 participants (> 4,000 in each of two experimental groups and > 2,700 in the active control group; mean age = 40 years, range = 18–60 years), they found no better results from 6 weeks of online training in reasoning, planning, and problem-solving or online training in attention, memory, math, and visuospatial processing than from 6 weeks spent finding answers online to obscure knowledge questions. Their outcome measures were a grammatical reasoning test, a visuospatial WM test where participants had to remember which boxes had already been searched, and two non-EF measures (digit span and paired associate learning).

Table 8.17, which presents the percentage across all EF measures (except reasoning/fluid intelligence) on which persons who received other cognitive training showed more improvement and/or better posttest results than comparison groups across all studies and ages, broken down by study, appears online.

Five years later, Corbett et al. (2015) conducted a similar study with better outcomes. They enrolled > 6,500 participants (> 2,400 in each of two experimental groups and > 1,700 in the active control group; mean age = 59 years, age range not given). They found that those who trained on reasoning and problem-solving online and those who trained on attention, memory, math, and visuospatial processing online showed more improvement on a grammatical reasoning test, a verbal recognition memory test, and in self-reported daily living activities than active controls who worked on placing a series of statements in the correct order. Those who trained on reasoning also improved more on the visuospatial WM test where participants had to remember which boxes had already been searched and on paired associate learning than

[8] Those nine studies are: Ball et al. (2002), Rebok et al. (2014), and Willis et al. (2006)—all on the ACTIVE study; Blieszner, Willis, & Baltes (1981), Borella et al. (2010), Carretti et al. (2013), Kroesbergen, van't Noordende, & Kolkman (2014), Plemons et al. (1978), Röthlisberger et al. (2012), Semrud-Clikeman et al. (1999), and Tamm, Epstein, Peugh, Nakonezny, & Hughes (2013).

Table 8.17. Percentage of All EF Measures (Except Reasoning/Fluid Intelligence) on Which Persons Who Received Other Cognitive Training Showed More Improvement and/or Better Posttest Results Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				YOUNG CHILDREN (< 7 YEARS OLD) WITH NO CLINICAL DIAGNOSIS											
1	Blakey & Carroll, 2015	Training in WM and inhibitory control	Training in perceptual judgments	1	4	25%	1	4	25%	1	4	25%	1	4	25%
2	Kroesbergen et al., 2014	*Domain-general WM training*	Domain-specific WM training	0	2	0%	0	2	0%	0	1	0%	0	2	0%
3	Rueda et al., 2005	Inhibition training: younger children	Watched children's videos	0	2	0%	0	2	0%	0	2	0%	0	2	0%
3	Rueda et al., 2005	Inhibition training: older children	Watched children's videos	0	2	0%	0	2	0%	0	2	0%	0	2	0%
4	Rueda et al., 2012	Inhibition training	Watched children's videos	0	3	0%	0	3	0%	0	3	0%	0	3	0%
5	Wass et al., 2011	Visual-attention training of infants	Viewed TV clips and images	3	4	75%	3	4	75%	3	4	75%	3	4	75%
2	Kroesbergen et al., 2014	*Domain-general WM training*	No treatment	1	2	50%	0	2	0%				0	2	0%
2	Kroesbergen et al., 2014	*Domain-specific WM training*	No treatment	1	2	50%	0	2	0%				0	2	0%
6	Kyttälä et al., 2015[A]	*WM and counting training*	No treatment	0	4	0%	0	4	0%	0	4	0%	0	4	0%

Table 8.17. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
7	Röthlisberger et al., 2012: 5-year-olds	*Training on EF laboratory tasks (including Stroop, Card Sort, Trail-Making, and Grass-Snow)*	No treatment	3	5	60%	3	5	60%	3	5	60%	3	5	60%
7	Röthlisberger et al., 2012: 6-year-olds	*Training on EF laboratory tasks (including Stroop, Card Sort, Trail-Making, and Grass-Snow)*	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
		SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH NO CLINICAL DIAGNOSIS													
8	Mackey et al., 2011	Computerized & noncomputerized reasoning training	Computerized & noncomputerized speed training	2	4	50%	0	4	0%	0	4	0%	0	4	0%
9	Johnstone et al., 2012[B]	Computerized training on self-ordered pointing & go/no-go tasks	No treatment	2	5	40%	2	5	40%	2	5	40%	2	5	40%
10	Wong et al., 2014[B]	Computerized WM training (Visuospatial & Auditory)	No treatment	3	11	27%	1	11	9%	1	11	9%	1	11	9%
		SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH ADHD OR A LEARNING DISABILITY													
11	Alloway et al., 2013	High-frequency Jungle Memory™ WM training	Low-frequency Jungle Memory™ WM training	2	2	100%	0	2	0%	0	2	0%	0	2	0%

12	Tucha et al., 2011[B]	AixTent computerized attention training	Noncomputerized visual-perception training	1	8	13%	1	8	13%	1	8	13%	1	8	13%
11	Alloway et al., 2013	High-frequency Jungle Memory™ WM training	No treatment	2	2	100%	0	2	0%	0	2	0%	0	2	0%
11	Alloway et al., 2013	Low-frequency Jungle Memory™ WM training	No treatment	0	2	0%	0	2	0%	0	2	0%	0	2	0%
9	Johnstone et al., 2012[B]	Computerized training on self-ordered pointing and go/no-go tasks	No treatment	4	5	80%	4	5	80%	4	5	80%	4	5	80%
13	Semrud-Clikeman et al., 1999	*Training on visual and auditory attention tasks*	No treatment (ADHD control group)	2	2	100%	2	2	100%	2	2	100%	2	2	100%
13	Semrud-Clikeman et al., 1999	*Training on visual and auditory attention tasks*	No treatment (typically developing)	2	2	100%	0	2	0%	0	2	0%	0	2	0%
14	Tamm et al., 2013[B]	*Pay Attention! noncomputerized intervention*	No treatment	12	27	44%	3	31	10%	3	19	16%	0	15	0%
					ADULTS (18–55 YEARS OLD)										
15	Owen et al., 2010	Web-based reasoning, planning, and problem-solving training	Web-based adaptive training in memory, attention, visuospatial processing, and math calculations	0	2	0%	0	2	0%	0	2	0%	0	2	0%
15	Owen et al., 2010	Web-based reasoning, planning, and problem-solving training	Answering obscure knowledge questions	0	2	0%	0	2	0%	0	2	0%	0	2	0%

(continued)

Table 8.17. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
15	Owen et al., 2010	Web-based training in memory, attention, visuospatial processing, and math calculations	Answering obscure knowledge questions	0	1	0%	0	1	0%	0	1	0%	0	1	0%
16	Penner et al., 2012[C]	Distributed-intensity BrainStim	High-intensity BrainStim WM training	3	8	38%	0	8	0%	0	8	0%	0	7	0%
17	Dahlin et al., 2008[B]	Letter memory, updating (verbal & nonverbal), and a complex-span task (Keep Track Task)	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
16	Penner et al., 2012[B,C]	High-intensity BrainStim WM training	No treatment	0	8	0%	0	8	0%	0	8	0%	0	3	0%
16	Penner et al., 2012[B]	Distributed-intensity BrainStim WM training	No treatment	3	8	38%	0	8	0%	0	8	0%	0	8	0%
18	Schmiedek et al., 2010, 2014[B,D]	Nonadaptive tasks of perceptual speed, episodic memory, WM updating, and complex-span tasks	No treatment	0	5	0%									

OLDER ADULTS (OLDER THAN 55 YEARS)

19	Borella et al., 2010	*Noncomputerized complex-span task*	Fill-in paper-and-pencil questionnaires	3	3	100%	3	3	100%	3	3	100%	3	3	100%
20	Buschkuehl et al., 2008	Working memory computerized training	Nonaerobic muscle training on recumbent bicycle	1	2	50%	1	2	50%	1	2	50%	1	2	50%
21	Carretti et al., 2013	*Noncomputerized complex-span task (categorization WM span)*	Fill-in paper-and-pencil questionnaires	1	1	100%	1	1	100%	1	1	100%	1	1	100%
22	Corbett et al., 2015	Web-based reasoning and problem-solving cognitive training (ReaCT)	Web-based game where statements needed to be put in correct order	2	2	100%	0	2	0%				0	2	0%
22	Corbett et al., 2015	Web-based training in memory, attention, visuospatial processing, and math calculations	Web-based game where statements needed to be put in correct order	0	1	0%	0	1	0%				0	1	0%
23	Ball et al., 2002; Rebok et al., 2014, and Willis et al., 2006[D]	Reasoning and problem solving noncomputerized training including real-world tasks	No treatment	1	1	100%									
24	Blieszner et al., 1981[E]	*Inductive reasoning training (noncomputerized)*	No treatment				1	1	100%	1	1	100%			

(continued)

Table 8.17. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
25	Cheng et al., 2012[F]	*Multidomain cognitive training (general EF training)*	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
25	Cheng et al., 2012[F]	*Single-domain cognitive training (reasoning)*	No treatment	1	4	25%	1	4	25%	1	4	25%	1	4	25%
17	Dahlin, Nyberg, et al., 2008, Dahlin, Stigsdotter-Neely, et al. 2008[B]	Letter memory, updating (verbal & nonverbal), and a complex-span task (Keep Track Task)	No treatment	0	5	0%	0	5	0%	0	5	0%	0	5	0%
26	Plemons et al., 1978	*Visuospatial reasoning training (noncomputerized)*	No treatment	2	3	67%	1	3	33%	1	3	33%	1	3	33%
18	Schmiedek et al., 2010, 2014[B,D]	Nonadaptive tasks of perceptual speed, episodic memory, WM updating, and complex-span tasks	No treatment	2	5	40%									
27	Wang et al., 2011	Cooking task computerized training	Participated in other lab studies (no treatment)	1	2	50%	1	2	50%	1	2	50%	1	2	50%
28	Wilkinson & Yang, 2012	Stroop task computerized training	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%

29	*Zinke et al., 2014*	*WM and EF training*	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Grand Totals and Percents across all studies examining noncomputerized training compared with any active-control condition (excluding Blakey & Carrol, 2015, and Mackey et al., 2011) ($N = 3$)			4	6	67%	4	6	67%	4	5	80%	4	6	67%
	Grand Totals and Percents across all studies examining noncomputerized training compared with any active-control condition (including Blakey & Carrol, 2015, and Mackey et al., 2011) ($N = 5$)			7	14	50%	5	14	36%	5	13	38%	5	14	36%
	Grand Totals and Percents across all studies examining noncomputerized training compared with no treatment ($N = 10$)			27	64	42%	13	68	19%	13	52	25%	9	51	18%
	Grand Totals and Percents across all studies examining noncomputerized training (excluding Blakey & Carrol, 2015, and Mackey et al., 2011) compared with any active-control condition or no treatment ($N = 13$)			31	70	44%	17	74	23%	17	57	30%	13	57	23%
	Grand Totals and Percents across all studies examining noncomputerized training (including Blakey & Carrol, 2015, and Mackey et al., 2011) compared with any active-control condition or no treatment ($N = 15$)			34	78	44%	18	82	22%	18	65	28%	14	65	22%
	Grand Totals and Percents across all studies examining computerized training compared with any active-control condition ($N = 12$)			16	45	36%	7	49	14%	7	46	15%	7	48	15%
	Grand Totals and Percents across all studies examining computerized training compared with no treatment ($N = 7$)			17	64	27%	8	54	15%	8	54	15%	8	54	15%
	Grand Totals and Percents across all studies examining computerized training compared with any active-control condition or no treatment ($N = 19$)			33	109	30%	15	103	15%	15	100	15%	15	102	15%
	Grand Totals and Percents across all studies for which participants trained on activities related to real-world activities compared with no treatment ($N = 2$)			2	3	67%	1	2	50%	1	2	50%	1	2	50%
	Grand Totals and Percents across all studies examining reasoning training compared with any active-control condition ($N = 3$)			4	10	40%	0	10	0%	0	8	0%	0	10	0%

(continued)

Table 8.17. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Totals and Percents across all studies examining reasoning training compared with no treatment ($N = 4$)			4	8	50%	3	8	38%	3	8	38%	2	7	29%
	Grand Totals and Percents across all studies examining reasoning training compared with any active-control condition or no treatment ($N = 7$)			8	18	44%	3	18	17%	3	16	19%	2	17	12%
	Grand Totals and Percents across all studies examining inhibitory control training compared with any active-control condition ($N = 2$)			0	7	0%	0	7	0%	0	7	0%	0	7	0%
	Grand Totals and Percents across all studies examining inhibitory control training compared with any active-control condition or no treatment ($N = 3$)			0	10	0%	0	10	0%	0	10	0%	0	10	0%
	Grand Totals and Percents across all studies examining attention training compared with any active-control condition ($N = 2$)			4	9	46%	4	12	33%	4	12	33%	4	12	33%
	Grand Totals and Percents across all studies examining attention training compared with no treatment ($N = 2$)			16	31	52%	5	35	14%	5	23	22%	2	19	11%
	Grand Totals and Percents across all studies examining attention training compared with any active-control condition or no treatment ($N = 4$)			20	40	50%	9	47	19%	9	35	26%	6	31	19%
	Grand Totals and Percents across all studies examining WM training compared with any active-control condition ($N = 6$)			10	18	56%	5	18	28%	5	17	29%	5	17	29%
	Grand Totals and Percents across all studies examining WM training compared with no treatment ($N = 9$)			13	59	22%	2	49	4%	2	45	4%	2	49	4%

Grand Totals and Percents across all studies examining WM training compared with any active-control condition or no treatment ($N = 15$)	23	77	30%	7	67	10%	7	62	11%	7	66	11%
Grand Totals and Percents across all studies examining training on multiple EFs compared with any active-control condition ($N = 4$)	2	8	25%	2	8	25%	2	7	29%	2	8	25%
Grand Totals and Percents across all studies examining training on multiple EFs compared with no treatment ($N = 4$)	11	27	41%	11	27	41%	11	27	41%	11	27	41%
Grand Totals and Percents across all studies examining training on multiple EFs compared with any active-control condition or no treatment ($N = 8$)	13	35	37%	13	35	37%	13	34	38%	13	35	37%
Grand Totals and Percents across all studies compared with any active-control condition ($N = 15$)	20	51	39%	11	55	20%	11	51	22%	11	54	20%
Grand Totals and Percents across all studies compared with no treatment ($N = 17$)	44	128	34%	21	122	17%	21	106	20%	17	105	16%
Grand Totals and Percents across all studies compared with any active-control or no-treatment condition ($N = 32$)	64	179	36%	32	177	18%	32	157	20%	28	159	18%

Note. Results for reasoning/fluid intelligence (R/FL) are included here only for those studies that specifically trained people on reasoning (Ball et al., 2002, Blieszner et al., 1981, Cheng et al., 2012, Mackey et al., 2011, and Plemons et al., 1978). For studies that did not specifically train people on reasoning, results for R/FL are not included in Table 8.16 (although they are mentioned in the text), but results for all other EF measures are included.

For the condition of interest column: Italic font indicates a study that used noncomputerized cognitive training. Regular font indicates computerized cognitive training. An underline indicates training on real-world tasks. Red ink indicates reasoning training. Blue ink indicates inhibitory control training. Green ink indicates attention training. Brown ink indicates WM training. Violet ink indicates training on multiple EFs.

Borella et al. (2010) and Carretti et al. (2013) are included in this table because they used noncomputerized training. Their results are also presented in Tables 8.13 and 8.14, along with other studies of complex-span training.

[A] The WM training by Kyttälä, Kanerva, and Kroesbergen (2015) did not increase in difficulty (it was nonadaptive).

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which results would remain significant had they done that.

[C] The authors call this an experimental group, but for the purpose of this table, it is considered an active control.

[D] This study did not test the difference between posttest scores.

[E] The difference in rate of improvement between groups was not tested.

[F] Cheng et al. (2012) noted that when they combined the two cognitive training groups, the results for two of the three measures (Stroop and Trails B) are true only for the roughly 55% who attended $\geq$ 80% of the training sessions.

did the active control group, although those who trained on memory, attention, etc., did not.

Why were the results so much more positive in the study by Corbett et al. (2015) than in that by Owen et al. (2010)? One reason might be the longer and more intensive training. Although sessions in both studies were only about 10 min, in the Corbett et al.study they extended over 26 weeks, versus over only 6 weeks in the Owen et al. study. Indeed, Corbett et al. found that effects for the training of attention, memory, etc., were dose dependent: Those who completed 4–5 sessions per week (112 sessions total) showed better outcomes than those who completed fewer sessions per week. For the reasoning training (which showed more benefits), dose-response effects were not found. Another possible reason for the more positive results in the Corbett et al. study is that the older adults in that study seemed more highly motivated and exhibited a higher level of engagement than the younger adults in the Owen et al. study. During the first 6 weeks of training in the Corbett et al. study, participants completed over twice as many sessions (an average of 51) as did participants over the 6 weeks of the training in the Owen et al. study (an average of 25).

Three of four studies of attention training (75%) found at least suggestive evidence of benefits to attention (two were noncomputerized training: Semrud-Clikeman et al., 1999; Tamm et al., 2013; and one used computerized training: Wass, Porayska-Pomsta, & Johnson, 2011). The fourth study (which used computerized attention training: Tucha et al., 2011) found benefits on only one out of eight near-transfer measures. Tamm et al. and Tucha et al. had not corrected for multiple comparisons.

Five of the seven studies of reasoning training (71%) found at least suggestive evidence of improved reasoning. Those seven studies included three with noncomputerized training (the ACTIVE study—Ball et al., 2002, Rebok et al., 2014, Willis et al., 2006; Blieszner et al., 1981; Cheng et al., 2011; Plemons et al., 1978), two with computerized training (Corbett et al., 2015; Owen et al., 2010) and one with both computerized and noncomputerized training (Mackey et al., 2011).

The Two Studies That Used Noncomputerized Training of Complex-Span Tasks

The two studies that used noncomputerized training of complex-span tasks were done by many of the same people (Borella et al., 2010; Carretti et al., 2013), the second being essentially a replication and extension of the first. The training used in these studies was exceptionally brief (only three 60-min sessions over 2 weeks). Both were with older adults (65–75 years old). Borella et al. (2010) reported both more improvement and better posttest scores after the three training sessions than for the control group (which filled out questionnaires) on all EF outcome measures used (two that assessed WM—Backward Digit Span and Dot Matrix; one that assessed inhibition—Stroop; and one reasoning/fluid intelligence—Cattell Culture Fair test). Benefits were still evident 8 months later on the Cattell test, although not on the other measures, compared to the control group. Carretti et al. (2013) found more improvement and better posttest performance on a near-transfer WM test among those who filled out questionnaires than among controls but no benefit for the Cattell test. The near-transfer WM benefit was still evident 6 months later. Such stellar results from so little training (only three 1-hour sessions over 2 weeks) calls out for replication attempts by others. No other complex-span training study has included follow-up testing. Note that these extremely impressive results were found with older adults.

The Nine Studies That Trained People on Miscellaneous WM Tasks

The most heroic and comprehensive training effort was conducted by Schmiedek, Lövdén, & Lindenberger (2010), who trained adults 7 days a week for 14 weeks, for a total of roughly 100 60-min sessions per participant. People were trained on 12 computerized tasks (six speed-of-processing tasks, plus a numerical memory updating task, a complex-span task [alpha span], a spatial N-back task, and one task each for memorizing lists of words, number-word pairs, and object positions in a grid). Schmiedek et al. intentionally varied content and procedures across tasks to emphasize learning cognitive skills rather than low-level strategies, although they did not dynamically increase difficulty.

They achieved impressive effect sizes. The effect sizes of benefits for young adults (20–31 years old) versus no-treatment controls on verbal episodic memory was > 0.50, numerical episodic memory ~ 0.45, N-back > 0.40, visuospatial reasoning > 0.35, and numerical reasoning > 0.30, although the training produced no significant benefits for young adults on complex-span tasks, memory updating, or Raven's Matrices. The effect sizes of benefits for older adults (65–81 years old) was 0.60 on rotation span, > 0.50 on Raven's Matrices, > 0.45 on episodic memory for word pairs, and > 0.40 on animal span, although the training produced no significant benefits for older adults on N-back, reading or counting span, memory updating, verbal, numerical, or visuospatial reasoning, or episodic memory.

Two years later, benefits to episodic memory and reasoning/fluid intelligence, but not WM, were still present for younger adults but no benefits were still evident for older adults (Schmiedek et al., 2014). The ages of 20–31 years are when most young people are living on their own for the first time, attending university, and/or starting careers and/or families. Perhaps younger adults continued to show benefits when older adults did not because their episodic memory, reasoning, and fluid intelligence continued to be challenged more than was true for older adults.

Buschkuehl et al. (2008) trained high-functioning 80-year-olds, many of whom had never used a computer before, on two computerized WM tasks twice a week for 45 min at a time over 12 weeks. The first task required remembering the order in which squares had been selected (the squares differed in color and spatial location). The second task required remembering the order in which stimuli (displayed one at a time) had been displayed, with an interposed task to indicate whether the displayed stimulus was right-side-up or upside-down. Those who went through the training improved more and performed better at posttest on visuospatial memory (Forward and Backward Spatial Span) but not on verbal memory (composite Forward and Backward Digit Span) compared to participants who did nonaerobic muscle training on a recumbent bicycle. Interestingly, the control group got worse from pre- to posttest.

Dahlin, Nyberg, Backman, and Neely (2008) and Dahlin, Stigsdotter-Neely, Larsson, Backman, and Nyberg (2008) trained younger and older adults (mean ages 24 and 68 years, respectively) on the Keep Track complex-span task and on five other tasks that required WM updating over a 5-week period (45-min sessions, three times a week). They found more improvement and better post-test performance by young adults who underwent this training than for no-treatment control subjects on the N-back test, but not on Backward Digit Span, Computation Span, Verbal Fluency, or fluid intelligence/reasoning (Raven's Matrices). The N-back benefit (the one benefit observed right after training) was no longer present 18 month later. Dahlin and colleagues did not find benefits from the training for older adults.

Wong, He, and Chan (2014) trained 6- to 12-year-old children with poor WM and ADHD (mean age = 8 years) on eight computerized WM tasks (five were visuospatial: indicate where objects had been in forward and backward order; three were auditory verbal: say back spoken letters or digits in forward and backward order) in 35- to 40-min sessions, 4 to 5 times a week over 5 weeks.[9] On very-near-transfer tasks (Span Board and Digit Span [forward + backward]), those who trained improved more than no-treatment controls. On the Span Board, trained children achieved higher scores than controls. On Digit Span, they simply caught up to controls. In follow-up testing 5 to 6 weeks later, the performance of trained children had deteriorated on Span Board so that it was no longer better than controls (but it was still better than their own pretest performance). On Digit Span, trained children maintained the gains they had achieved but still were no better than controls. There was no far transfer at posttest on any measure of inhibition or attention.

Kroesbergen et al. (2014) trained 6-year-old children who were poor at math on either verbal and visuospatial WM skills or on WM skills specific to numerical tasks. There were only eight 30-min sessions (two per week for 4 weeks) and the training was not computerized. Both groups improved more on visuospatial

[9] In analyzing their data, multiple comparisons were conducted, but no correction for that was made. It is unclear which, if any, results would remain significant had that correction been made.

WM than no-treatment controls but not on verbal WM. The WM benefit was comparable for both training groups. The group that trained on WM skills related to numeracy improved more in numeracy than controls.

Kyttälä et al. (2015) also used noncomputerized training with 6-year-olds. They trained some on counting and some on counting and WM, but without difficulty increasing. Here, too, there were only eight 30-min sessions over 4 weeks. No EF benefits in trained children were found compared to no-treatment controls.

Three studies varied characteristics of the training to see what is most helpful. Two varied frequency. Many studies over decades have documented that distributed or spaced practice usually yields better long-term results than massed practice (e.g., Landauer & Bjork, 1978; Rosenbaum et al., 2001; Shea & Morgan, 1979). Penner et al. (2012) showed this is also true for WM training. They had one group train 4 days a week over 4 weeks (massed practice) and one group train 2 days a week over 8 weeks (spaced practice). Sessions were 45 min long. On backward block span, Backward Digit Span, and the 3-back task, those who did spaced practice improved more than those who did massed practice and more than no-treatment controls. Improvement of the massed-practice and no-treatment groups did not differ. On other measures (Verbal Fluency and easier WM tasks: forward block span and 2-back) there were no group differences.

Alloway, Bibile, and Lau (2013) also varied frequency but kept duration constant so the spaced-practice group received fewer training sessions (one session a week for 8 weeks vs. four sessions a week for 8 weeks of online WM training [Jungle Memory®]). Whereas Penner et al. (2012) studied adults (mean age = 39 years), Alloway et al. studied children (mean age = 10.5 years). Alloway et al. found that the group with more sessions improved more on both WM measures (shape recall and a composite of Backward Digit Span and processing letter recall) compared to the group with less practice or no-treament controls, and those benefits were still evident 8 months later.

Prins, Dovis, Ponsioen, ten Brink, and van der Oord (2011) compared a modified version of Cogmed with elaborate gaming elements to the same modified version of Cogmed but without gaming elements (the way Cogmed is normally administered) for a very short time (once a week for 3 weeks). Their participants were 9½-year-olds with ADHD. Their only transfer measure was Corsi Blocks, on which there was no group difference, although those who trained with gaming elements improved significantly on that while those who trained without those elements did not. Children liked the version with gaming elements more and were more motivated to work at the modified Cogmed when those elements were present. Training sessions were 15 min, with the option of continuing for another 15 min. The group assigned to the version without gaming elements trained only for an additional 2.3 min on average; the group assigned to the version with gaming features trained for an extra 12.4 min on average.

The Four Studies That Trained People on Attention Tasks

Semrud-Clikeman et al. (1999) trained 10-year-old schoolchildren with ADHD of the inattentive type on a visual-attention task and an auditory-attention task in 60-min sessions twice a week for 18 weeks. On near-transfer measures (Brief Test of Attention and d2 Test of Attention), the children improved more than either no-treatment group (typically developing children and children with ADHD of the combined type). The intervention children performed better at posttest on both measures than the untrained ADHD children and, impressively, caught up to typically developing children on both measures.

Tamm et al. (2013) studied Pay Attention! noncomputerized training, which uses auditory stimuli and visual stimuli on cards to train sustained, selective, divided, and alternating attention, with difficulty increasing. They compared 30 min of this twice a week for 8 weeks to no intervention in children 7 to 15 years old (mean age = 9 years) with ADHD. (It appears that all, or most, participants were on medication for ADHD.) Nonblind observers (parents, clinicians, and the children themselves) reported some benefits, but teachers did not agree, and on objective neuropsychological tests the only benefit seen was on planning (which was probably the most difficult measure).

Tucha et al. (2011) studied possible benefits of AixTent computerized training for children 10 to 11½ years old with ADHD on medication compared to noncomputerized visual perception training. AixTent aims to train four domains of attention (vigilance and alertness, which do not involve EFs, and selective and divided attention, which do) using computer tasks in everyday or gamelike situations, with difficulty increasing as performance improves. On the Zimmermann and Fimm Test of Attentional Performance (2002), children trained on AixTent improved more than children trained on the Frostig Developmental Program of Visual Perception in commission errors during divided attention, but not on omission errors or speed during divided attention, nor on any of those three measures for selective attention, and not in cognitive flexibility.

Wass et al. (2011) trained 11-month-old infants to sustain their attention (ignoring distractions) and to shift their attention. After just over 2 weeks, the trained infants were better at sustaining, switching, and disengaging attention than were infants who just watched TV clips and still images.

The Three Studies That Trained People on Inhibitory Control

In two independent studies, Rueda and colleagues (2005, 2012) trained 4½- to 6½-year-old children on computer games emphasizing inhibitory control (selective attention and response inhibition) five times a week for 45 min over 2 to 3 weeks. Unfortunately, in both studies they found neither more improvement on any EF measure nor better EF posttest performance compared with peers who simply watched videos.

At the other end of the age spectrum, Wilkinson and Yang (2012) trained older adults (60–84 years old, mean = 71) using the Stroop task in six 30-min sessions spread over 2 weeks. Participants improved on what they practiced but there was no transfer of benefits to untrained tasks, such as go/no-go (inhibitory control), Flanker (selective attention), or tests of task switching or reasoning. The type or presence/ absence of, feedback during training did not affect the outcome.

Two Studies That Trained People on WM and Inhibitory Control

Blakey and Carroll (2015) trained 4-year-olds on a noncomputerized WM task (Six Boxes) and computerized WM and inhibitory control tasks. There were only four 20-min training sessions (one per week over 4 weeks). They found benefits to WM, but not to inhibitory control or cognitive flexibility. On their only WM measure (backward word span), the children who trained on WM and inhibitory control improved more and performed better than their peers who trained on making perceptual judgments, and the benefit was still evident 3 months later. No benefits were found on any of their three measures of inhibitory control and cognitive flexibility. Three months later, reasoning was also tested. Those who had trained on WM and inhibitory control performed better at reasoning 3 months later than those who had trained on perceptual judgments, but there were no pretest scores for this.

Johnstone et al. (2012) devised a computerized training regime for children of 7 to 14 years (mean age = 9.7), almost half of whom had ADHD. Of those with ADHD, 87% were on medication. The children trained 5 days a week for 4 to 5 weeks (15–20 min per day) on two computer games. One was a self-ordered pointing task (which the researchers dubbed the "Feed the Monkey" game), where children needed to find which box contained a hidden object, trying not to repeat a choice. The other was a go/no-go task. Difficulty on each was incremented after error-free performance and reduced when five or more errors were made. One group got this training while their attention was passively monitored using EEG; the presence or absence of EEG monitoring had no effect on the results. The computerized training yielded more improvement and better posttest performance than no treatment on two inhibitory control (selective attention) measures (the Flanker and oddball tasks) for children with ADHD (though these findings might not have survived correction for multiple comparisons), but produced no benefits for typically developing children. The training did not improve performance even for children with ADHD on a go/no-go task (though training had specifically included that kind of task) or on the counting span task (which requires WM and other EFs) compared to no-treatment controls.

The Seven Studies That Trained People on Reasoning

The huge online studies by Owen et al. (2010) and Corbett et al. (2015) with over 10,000 and over 6,000 participants, respectively, were discussed in the beginning of this section (just after Table 8.16). The large ACTIVE study, with 2,832 older adults (mean age = 74 years), used noncomputerized cognitive training of problem-solving and reasoning (Group 1), verbal episodic memory (Group 2), and speed of processing (Group 3; Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006). Participants were trained not just on laboratory measures but also using real-world activities (like food preparation and financial management for reasoning; organizing and recalling shopping list items and remembering details on prescription labels for memory training). Only change scores were compared, not levels of posttest performance, and the authors didn't compare the performance of Groups 1, 2, and 3 to one another, but only compared each to no-treatment controls.

Reasoning training improved reasoning more than no treatment, and that remained true at follow-up testing 1, 2, and even 5 years later (that's impressive; indeed, the effect size 1 year later was still 0.40). Benefits were no longer evident 10 years later. No far transfer to reasoning was found for the other two types of training, nor did training on reasoning transfer to improved memory or speed. Those who trained on memory (or speed) improved more on memory (or speed, respectively) at posttest, and performed better than no-treatment controls 1, 2, and 5 years later (even 10 years later for speed, but not memory). The benefits noted here for each group were seen only on laboratory tests fairly similar to the laboratory measures on which participants had been trained, not on real-life measures. Not surprisingly, for each group, by 5 years after training, benefits were much reduced, but booster sessions helped slow the decline for reasoning.

Blieszner et al. (1981) trained older adults (mean age = 70, range = 60–85 years) over a period of 2 weeks (5 days a week, 60 min per day) on inductive reasoning, such as figuring out the next letter or number in a series. Compared to no treatment, training resulted in better posttest performance on one inductive reasoning measure but not on another, and degree of improvement was no better than among no-treatment controls because there were sizeable test–retest effects.

Cheng et al. (2012) trained older adults (mean age = 70 years, range = 65–75 years) for 12 weeks (60-min sessions, twice a week) on either multiple EF domains as well as real-world skills or specifically on reasoning. Both groups improved more and performed better at posttest on reasoning than no-treatment controls, although neither training group showed benefits relative to controls on Stroop or Trail-Making. This was true even though only slightly more than 50% completed either training. Those who completed ≥ 80% of either training performed better at posttest on both Stroop and Trails B than those who attended less training. Six months later, those who had trained more were still better on Trails B and now they were also better on reasoning than those who had trained less, although initially even those who had received less training showed a benefit on reasoning. By 1 after training, the differential benefit to reasoning was even greater, while the other relative benefits had disappeared.

Mackey et al. (2011) trained children 7 to 10 years old (mean age of 8½) on either reasoning or speed of processing, in both cases using computerized and noncomputerized games with two sessions a week for 60 min over 8 weeks. Those trained on reasoning improved more on reasoning (the TONI test) than those trained on speed, but posttest scores on reasoning were not significantly different between the two groups. That could be perhaps because training on speed of processing also aided reasoning somewhat, but without no-treatment controls we cannot know if that was the case or if the reasoning training produced no better posttest scores than simply taking the test twice (pre- and posttest).

Plemons et al. (1978) trained older adults (mean age = 70 years) on items designed to closely resemble, but not be identical to, those on the Figural Relations Diagnostic Test and Cattell-Horn measures for Figural Relations. Not surprisingly, people who practiced the training items improved more on both of those tests than no-treatment controls (although their posttest performance was only significantly better than controls for the Figural Relations Diagnostic Test, which the training resembled more closely than the Cattell-Horn measures). There was no generalization to a measure of inductive reasoning or a measure of crystallized intelligence.

Three Studies That Trained People on Multiple EF Skills

None of the studies found clear EF benefits. Wang et al. (2011) gave older adults (mean age = 66 years) only five sessions, one per week, each less than an hour long, of training on a computerized task related to real life—cooking a meal. This required planning, prioritizing, multitasking, and other challenging EF skills. On Letter-Number Sequencing (a difficult WM task), they found more improvement and better posttest performance than older adults who did other computerized training, but on a less difficult test of WM (Backward Digit Span), they found no difference between the groups. We would like to see follow-up of this study with more sessions of the intervention, testing on some of the high-levels skills involved in the training, and testing to see if and how long benefits last.

Röthlisberger et al. (2012) trained 5- and 6-year-olds on a host of EF laboratory tasks (including Stroop, Card Sort, Trail-Making, and Grass-Snow) for 6 weeks (30 min per day, 5 days a week). This noncomputerized training improved performance on a complex-span task (that taxes multiple EFs) in both age groups, and on task switching (switching from classic to reverse Flanker) in 5-year-olds. Both the degree of improvement and posttest scores on these two measures were better than for no-treatment controls. The only measure on which there was no benefit relative to controls was on the classic Flanker test (which assesses selective attention and is easier than the two measures on which group differences were found).

Zinke et al. (2014) trained older adults (mean age = 77 years) in only nine 30-min sessions (three per week for 3 weeks) on the spatial memory subtest of the Kaufmann Assessment Battery for Children, a verbal WM task where participants heard a series of numbers and were to subtract two from each number and repeat the series of numbers, and the Tower of London. No benefits relative to no-treatment controls were found on the Tower of Hanoi or Corsi Block. Relative to controls, the training group improved more on Raven's Matrices, but there was no group difference in posttest scores. The training group improved more and performed better than controls at posttest on a measure of STM (Forward Letter Span) and that benefit was still evident 9 months later.

Neurofeedback

Neurofeedback uses scalp electrodes to monitor neural activity and gives participants continuous real-time feedback (e.g., by a visual-auditory display on a computer monitor) about whether they are getting closer to the goal for their neural activity. Participants typically have no clue how they are affecting their brain's electrical activity, but, despite that, are usually able to change their brain-wave activity in the desired direction.

The neurofeedback training of Wang and Hsieh (2013) increased the amplitude of theta brain waves (4–7 Hz), especially over frontal-midline electrodes in older and younger adults (mean ages 65 and 22, respectively). Becerra et al. (2012) recruited older adults with abnormally high theta power and their neurofeedback training decreased their theta absolute power. Vollebregt, van Dongen-Boomsma, Buitelaar, and Slaats-Willemse (2014) used neurofeedback to normalize brain-wave abnormalities among children with ADHD.

Wang and Hsieh (2013) found that those who got the theta band uptraining for only 15 min, three times a week for only 4 weeks (only 12 sessions), performed better and improved more on the Flanker task (which requires selective attention) and, in the older group, the Sternberg task (which taxes WM and inhibitory control) than those who got sham neurofeedback training. A continuing question with neurofeedback is how long benefits last, since there is evidence they fade quickly. It would be interesting to see if the benefits can be replicated and if there is any evidence that they last even weeks or months.

The theta band downtraining by Becerra et al. (2012) over 10 to 12 weeks (two or three 30-min sessions per week) produced no EF benefits relative to the sham training. Both groups improved. Vollebregt et al.'s (2014) individually customized neurofeedback produced no EF benefits.

Two meta-analyses of the effectiveness of neurofeedback for children with ADHD report that, when the raters and testers were blinded and/or a sham or active control group was used (rather than just no treatment), no benefits of neurofeedback were significant (Cortese et al., 2016; Sonuga-Barke et al., 2013). The meta-analysis by Cortese and colleagues examined all eight RCTs reviewed by Sonuga-Barke et al. plus five more recent ones. Cortese et al. found no significant benefits of neurofeedback on laboratory measures of inhibition or sustained attention or on ratings of ADHD symptoms in general or inattentive symptoms, though nonblinded raters indicated a small but significant benefit for hyperactivity/impulsivity. A third meta-analysis of the effectiveness of neurofeedback for children with ADHD looked only at parent ratings (almost all of which were not blinded; van Doren et al., 2018). It looked at 10 RCTs, and consistent with the findings of the other two meta-analyses, found that nonblinded raters report that neurofeedback improves inattention. Van Doren et al. report that raters indicated that the benefits increased during the follow-up period after the neurofeedback sessions ended and that by the end of the follow-up period, the benefits reported for neurofeedback were equal to those reported for medication.

Physical-Activity Training to Improve EFs

Aerobic exercise with and without cognitive or motor-skill challenges is the most studied physical activity for improving EFs. Aerobic exercise is exercise that requires the consumption of substantially more oxygen than at rest. It involves expending energy that maintains an increased heart rate and increased oxygen uptake. Hence, its most proximal benefit is improved cardiorespiratory fitness. The next most studied physical activities to improve EFs are resistance training (also called strength training or weight training) and yoga.

A very impressive study by Sink et al. (2015), with eight sites and over 1,500 participants, randomly assigned sedentary, cognitively intact older adults 70 to 89 years old to moderate physical activity (walking, resistance training, and flexibility exercises) or health education (educational workshops and upper extremity stretching). After 2 full years of the intervention, they found no group difference on any cognitive measure, including EFs. The disappointing results do not seem to be due to the participants' being too old; indeed the authors found that benefits to EFs from physical activity were greater for those ≥ 80 years old and those with poorer baseline physical performance.

Aerobic Exercise with Fewer Cognitive Demands (Plain Aerobic Exercise)

Sixteen studies looked for EF benefits from aerobic exercise with little or no cognitive component.[10] Of those studies, 12 (75%) were with older adults, two were with children of 8 to 12 years, one was with 6-year-olds, and one was with youths and adults 17 to 47 years old. None was done with children younger than 6 years old. Only one study (Stroth et al., 2010) looked specifically at teens or young adults.

Over half the studies of plain aerobic exercise found no EF benefit at all (56%) and another found almost no EF benefit (Fisher et al. 2011; see Table 8.18). Two studies (13%; Kramer et al., 1999; Moul, Goldman, & Warren, 1995) found strong evidence of EF benefits from plain aerobic training.

In the Kramer et al. (1999) study, adults 60 to 75 years old were assigned to aerobic walking or flexibility training (stretching and toning) for 24 weeks (dose and frequency not given). Although on one measure of response inhibition (incongruent Simon task trials) the aerobic-walking group seems to have just caught up to the flexibility group, on another measure of response inhibition (the Stop-Signal task) and on task switching, those who did aerobic walking improved more and achieved better posttest performance than the flexibility-training group.

Moul et al. (1995) had adults 65 to 72 years old (mean age = 69) do 30 to 40 min of aerobic walking five times a week for 16 weeks and administered subscales of the Ross Information Processing Assessment (RIPA). On one of the two EF subscales (Organization: semantic categorization and word fluency) but not on the Problem-Solving and Abstract Reasoning subscale, those who did aerobic walking improved more and performed better at posttest than those who did resistance training or flexibility exercises. This is considered strong evidence by our criteria because we do not include performance on reasoning/fluid intelligence measures in our calculations (except for studies that specifically targeted that). Although omitting the reasoning subscale leaves only one dependent measure, according to our criteria, finding both more improvement and better posttest performance on that one measure qualifies as strong evidence. (Our criteria are better change and better outcome scores than a control group on > 67% of EF measures, excluding reasoning/fluid intelligence unless that was targeted in training).

Seven studies (44%) found suggestive evidence of EF benefits (i.e., more EF improvement or better EF posttest performance than a control group on > 50% of measures). On only 17% of the 70 measures where studies compared EF improvement across groups was there evidence that plain aerobic exercise

[10] Not included in any calculations discussed here are studies of aerobic exercise plus other forms of exercise or aerobic exercise plus other activities that did not involve physical activity and studies that compared aerobic exercise to medication, although they appear in Table 8.18.

Table 8.18. Percentage of EF Measures (Except Reasoning/Fluid Intelligence) on Which Persons Who Did Plain Aerobic Exercise Showed More Improvement and/or Better Posttest Results Than Comparison Groups Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				YOUNG CHILDREN (3–6 YEARS OLD)											
1	Fisher et al., 2011[A]	Aerobically intense PE	Standard PE	3	10	30%	2	10	20%	2	10	20%	2	10	20%
				SCHOOL-AGE CHILDREN (7–17 YEARS OLD)											
2	Schmidt et al., 2015	Aerobic exercise: High aerobic & low cognitive condition	Standard PE: low aerobic & low cognitive condition	0	3	0%	0	3	0%	0	3	0%	0	3	0%
3	Tuckman & Hinkle, 1986[B]	Aerobic running	Standard PE				1	2	50%	1	2	50%			
	Totals and Percents for children who did plain aerobic exercise compared with those who did standard PE			3	13	23%	3	15	20%	3	15	20%	2	13	15%
				ADULTS (17–47 YEARS OLD)											
4	Stroth et al., 2010	Running training	No treatment	2	4	50%	0	4	0%	0	4	0%	0	4	0%
				OLDER ADULTS (OLDER THAN 55 YEARS)											
5	Albinet et al., 2010	Aerobic walking, circuit training, and running	Stretching	1	1	100%	0	1	0%	0	1	0%	0	1	0%

(continued)

Table 8.18. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
6	Blumenthal et al., 1989[A]	Aerobic cycling, walking, and jogging	Yoga & flexibility	0	8	0%	0	8	0%	0	8	0%	0	8	0%
7	Erickson et al., 2011, Leckie et al., 2014, and McAuley et al., 2011[C]	Aerobic walking	Toning & stretching	0	3	0%									
8	Kramer et al., 1999	Aerobic walking	Toning & stretching	3	3	100%	2	3	67%	2	3	67%	2	3	67%
9	Moul et al., 1995	Aerobic walking	Mild range-of-motion & flexibility exercises	1	1	100%	1	1	100%	1	1	100%	1	1	100%
10	Smiley-Oyen et al., 2008	Aerobic exercise using equipment (treadmill, elliptical machines, etc.)	Flexibility exercises & resistance training	0	4	0%	0	4	0%	0	4	0%	0	4	0%
11	Voelcker-Rehage et al., 2011	Aerobic walking	Relaxation & stretching	0	2	0%	0	2	0%	0	2	0%	0	2	0%
	Totals and Percents for older adults who did plain aerobic exercise compared with toning & stretching			5	22	23%	3	19	16%	3	19	16%	3	19	16%
12	Dustman et al., 1984[A]	Aerobic walking	Resistance training	1	2	50%	1	2	50%	1	2	50%	1	2	50%

	Totals and Percents for older adults who did plain aerobic exercise compared with any physical-activity active control			6	24	25%	4	21	19%	4	21	19%	4	21	19%
	Grand Totals and Percents for children and older adults who did plain aerobic exercise compared with any physical-activity active control			9	37	24%	7	36	19%	7	36	19%	6	34	18%
13	Fabre et al., 2002[D]	Aerobic training	Leisure activities: Painting & choral singing	0	1	0%	0	1	0%	0	1	0%	0	1	0%
14	Legault et al., 2011	Aerobic walking & flexibility training	Healthy aging education	0	6	0%	0	6	0%	0	6	0%	0	6	0%
	Totals and Percents for older adults who did plain aerobic exercise compared with any non-physical-activity active control			0	7	0%	0	7	0%	0	7	0%	0	7	0%
	Grand Totals and Percents for older adults who did plain aerobic exercise compared with any active control			6	31	19%	4	28	14%	4	28	14%	4	28	14%
6	Blumenthal et al., 1989[A]	Aerobic cycling, walking, and jogging	No treatment	0	8	0%	0	8	0%	0	8	0%	0	8	0%
12	Dustman et al., 1984	Aerobic walking	No treatment	1	2	50%	0	2	0%	0	2	0%	0	2	0%
15	Mortimer et al., 2012[A,C]	Aerobic walking	No treatment	0	5	0%									
16	Oken et al., 2006[E]	Aerobic walking and mild leg stretches	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for older adults who did plain aerobic exercise compared with no treatment			1	18	6%	0	13	0%	0	13	0%	0	13	0%

(continued)

Table 8.18. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Totals and Percents for adults of all ages who did plain aerobic exercise compared with no treatment			3	22	14%	0	17	0%	0	17	0%	0	17	0%
	Totals and Percents for older adults who did plain aerobic exercise compared with any active-control condition or no treatment			7	49	14%	4	41	10%	4	41	10%	4	41	10%
	Grand Totals and Percents for children and older adults who did plain aerobic exercise compared with any active-control condition			9	44	20%	7	43	16%	7	43	16%	6	41	15%
	Grand Totals and Percents for children and all adults who did plain aerobic exercise compared with any active-control condition or no treatment			12	66	18%	7	60	12%	7	60	12%	6	58	10%
	CHILDREN WHO TRAINED ON ENDURANCE, FLEXIBILITY, RESISTANCE, AND AEROBICS														
17	Gallotta et al., 2015[A,F]	Traditional PE Intervention	Coordinative PE Intervention	1	3	33%	0	3	0%	0	3	0%	0	3	0%
17	Gallotta et al., 2015[A,F]	Traditional PE Intervention	No treatment	3	3	100%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for children who trained on endurance, flexibility, resistance, and aerobics compared with any active control or no treatment			4	6	67%	0	6	0%	0	6	0%	0	6	0%
	ADULTS 40–72 YEARS OLD WITH CLINICALLY DIAGNOSED MAJOR DEPRESSIVE DISORDER (MDD)														
18	Hoffman et al., 2008[A]	Individualized aerobic training	Placebo pill	1	6	17%	1	6	17%	1	6	17%	1	6	17%

OLDER ADULTS (OLDER THAN 55 YEARS) WHO TRAINED ON AEROBICS PLUS RESISTANCE, BALANCE, AND FLEXIBILITY TRAINING

19	Langlois et al., 2013[C]	Aerobic exercise & resistance training using equipment (treadmill, elliptical machines, etc.)	No treatment	2	2	100%									
20	Williamson et al., 2009	Moderate-intensity physical activity: Aerobics, strength, balance, & flexibility	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
21	Sink et al., 2015[C]	Moderate-intensity physical activity training (brisk walking), strength, flexibility, & balance training	Health education training (unlikely to increase physical activity)	0	3	0%									
20	Williamson et al., 2009	Moderate-intensity physical activity: Aerobics, strength, balance, & flexibility	Health education	0	1	0%	0	1	0%	0	1	0%	0	1	0%
	Totals and Percents for older adults who did plain aerobic activity plus resistance training compared with any active control or no treatment			2	7	29%	0	2	0%	0	2	0%	0	2	0%

(continued)

Table 8.18. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
			ADULTS 40–72 YEARS OLD WITH CLINICALLY DIAGNOSED MAJOR DEPRESSIVE DISORDER (MDD)												
18	Hoffman et al., 2008[A]	Individualized aerobic training	Antidepressant medication: sertraline (Zoloft®)	0	6	0%	0	6	0%	0	6	0%	0	6	0%
22	Khatri et al., 2001	Aerobic walking and jogging	Antidepressant medication: sertraline (Zoloft®)	1	3	33%	0	3	0%				0	3	0%
	Grand Totals and Percents across all plain aerobic exercise studies (including studies where not just aerobic exercise was done) compared with any active control, medication, or no treatment			20	94	21%	8	83	10%	8	80	10%	7	81	9%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.18 (although they are mentioned in the text) but results for all other EF measures are included.

[A] The authors of this study did not include a correction for multiple comparisons. It is unclear which of their results would remain significant had they done that.

[B] Tuckman & Hinkle (1986) did not test the difference in rate of improvement between groups.

[C] This study did not test the difference between posttest scores.

[D] Fabre, Chamari, Mucci, Massé-Biron, & Préaut (2002) included only 8 participants per group and looked at outcomes after only 8 weeks.

[E] Oken et al. (2006) allowed people to enter the study—including the no-treatment group—who were doing ≤ 30 minutes of aerobic exercise a day.

[F] Gallotta et al. (2015) randomized by school but appear to have analyzed the data as if they randomized by individual children.

(aerobic exercise without motor skill or explicit EF demands) improved EFs more than any comparison condition. On only 11% of the 64 measures was EF posttest performance after weeks of plain aerobic exercise better than that of any comparison condition (see Table 8.18). Three studies compared plain aerobic exercise to standard physical education (PE; which presumably has some aerobic elements, hence potentially underestimating the benefits of aerobic exercise). Five compared plain aerobic exercise to no treatment (potentially overestimating the benefits of aerobic exercise). It ends up not mattering whether plain aerobic exercise was compared to standard PE, no treatment, or stretching and toning; in all cases, the mean percentage of measures on which the aerobic condition produced more EF improvement than the comparison condition was roughly 20% (23%, 14%, and 23%, respectively; see Table 8.18). The mean percentage of measures on which those who did plain aerobic exercises showed better EFs at posttest than a comparison condition varied from a low of 0% for the no-treatment control condition to 20% when standard PE was the comparison condition (16% when stretching and toning was the control condition). Fabre et al. (2002) included an excellent control condition (active, but not a physical activity; namely, leisure activities like painting and choral singing), but they included only eight participants per group (the minimum required to be included in this review) and looked at outcomes after only 8 weeks. They found no greater EF benefits from aerobic exercise than from the more sedentary leisure activities.

Studies of Plain Aerobic Exercise with ≥ 4 EF Measures That Found No EF Benefit

The most disappointing results come from five studies that looked at multiple EF measures and found no EF benefit at all from a training regimen of less cognitively demanding (i.e., plain) aerobic exercise. Blumenthal et al. (1989) had 60- to 83-year-olds (mean age = 67 years) do aerobic exercise for 16 weeks, in three 60-min sessions per week of which 45 min were aerobic, and found no EF benefits using design fluency, Verbal Fluency, Sternberg, Stroop, Backward Digit Span tasks, Trails B minus Trails A, and measures of selective attention and logical memory compared to active controls (who did yoga and flexibility exercises) or even no-treatment controls.

The same research group also looked at possible benefits of aerobic exercise (same frequency and duration as above) for sedentary, clinically depressed adults (ages 40 to 66, mean age of 52 years) compared with antidepressant medication (sertraline) or placebo pills (Hoffman et al., 2008). They found that those who did aerobic exercise improved no more than placebo controls on any of the same six neuropsychological EF measures used by Blumenthal et al. (1989). This was true even when looking only at the optimal exercisers. Exercisers also improved no more, and performed no better, than the medicated group on any neuropsychological measure except the Ruff 2 & 7 test (a measure of selective attention) and Trail-Making (a measure of cognitive flexibility), although the latter difference appears to be due to the medicated group inexplicably getting worse.[11]

The third study in this set of six had older adults free of dementia (mean age = 68, range = 60–79 years) do aerobic walking (Mortimer et al., 2012) for 30 min (during 50-min classes), three times a week for many weeks (40 weeks). Across all EF measures (Stroop, Trails B, Backward Digit Span, abstract verbal reasoning, category fluency, and an attention rating scale), the walking group showed zero benefits compared to no-treatment controls. When Mortimer and colleagues did a median split of the walking group, they found that the fast walkers improved more than the slow walkers on the Stroop task. They do not mention if the fast walkers improved more than the no-treatment group.

The fourth study (Legault et al., 2011) also had older adults (mean age = 76, range = 70–85 years) with normal cognitive functioning (and who had been exercising < 30 min a week) do aerobic walking (or stationary cycling for the few for whom walking was contraindicated). They, too, looked at Stroop and Trails B plus Flanker, task switching, self-ordered pointing, and a composite of all five. Participants did the aerobic exercise for 40 min (within 60-min classes) two times a week (plus 10–15 min each week on their own) for 17 weeks. No benefits were found on any EF measure, or the EF composite, for those who did aerobic exercise or those who did aerobic exercise plus recognition memory training compared to those who attended lectures on healthy aging.

The fifth study had adults 65 to 79 years old (mean age = 70) do 25–30 min of aerobic exercise (within 45-min classes) three times a week for 40 weeks and found no benefit to inhibitory control or cognitive flexibility as assessed by the Simon, Stroop, go/no-go, and Wisconsin Card Sort tests compared to other adults who did flexibility and resistance training exercises (Smiley-Oyen, Lowry, Francois, Kohut, & Ekkekakis, 2008).

Other Studies That Found Disappointing Results for EF Benefits From Aerobic Activities With Minimal Cognitive Demands

Six other studies, with fewer EF measures, also found little or no EF benefit from plain aerobic exercise. Oken et al. (2006) had healthy adults 65 to 85 years old

[11] Depression is often associated with poorer EFs. Hence, one might expect that anything that relieves depression (such as exercise or antidepressant medication) would improve EFs. However, depression is most likely to be associated with poorer EFs when the depression is severe (Mandelli, et al. 2006), recurrent (Paelecke-Habermann, Pohl, & Leplow, 2005), or treatment-resistant (Wroolie et al. 2006). In Hoffman et al.'s study, participants' depression tended to be mild to moderate (not severe), nonrecurrent, and responsive to treatment. Note this study and any other that compared an intervention program to medication are not included in any calculations we discuss.

(mean age = 72) do aerobic exercise in 60-min classes once a week and for an average of 56 min every day at home for 26 weeks. The researchers looked at the Stroop task, task switching, divided attention, and Letter-Number Sequencing. On none did the group that exercised improve more or perform better than no-treatment controls. However, people were allowed people to enter the study, including the no-treatment group, who were doing 30 min of aerobic exercise a day, and the authors speculate that there may have been ceiling effects on their cognitive measures.

Voelcker-Rehage, Godde, and Staudinger (2011) found no benefit in either speed or accuracy on the Flanker task for adults 63 to 79 years old (mean age = 70) from 35–50 min of aerobic walking three times a week for a full year (52 weeks) compared to stretching and relaxation exercises.

Another study compared 67-year-olds (age range: 55–80 years) assigned to aerobic walking to those assigned to stretching and toning on global and local switch costs (two EF measures). On neither was there a difference between groups in improvement or posttest performance (Erickson et al., 2011; Leckie et al., 2014; McAuley et al., 2011).

As mentioned above, Fabre et al. (2002) also found no EF benefit, although they included only one EF measure, their intervention was brief, and their number of subjects small. Their subjects were 60 to 76 years old (mean age = 66).

At the other end of the age spectrum, the one study of 6-year-olds (Fisher et al., 2011) included five behavioral measures of EFs plus the Conners Behavioral Rating Scale for Parents but found that 10 weeks of aerobically intense PE compared to standard PE yielded better improvement and better posttest performance on only two of the six EF tests (spatial span WM test in CANTAB®) and one subscale of Conners (inattentiveness). Fisher et al. found no benefit to planning, selective attention (on two different tests), memory of sequential order, or logical reasoning compared to standard PE.

The one study of adolescents and young adults (age range: 17–47; mean age = 23 years; Stroth et al., 2010) included three EF measures (Stroop, N-back, and the Hearts and Flowers task). The authors found no benefit from aerobic walking or running on the Stroop or N-back tasks, but more improvement (though not better posttest scores) than no-treatment controls on incongruent trials of the Hearts and Flowers task (which require inhibitory control).

Studies That Found Suggestive Evidence of EF Benefits From Aerobic Activities With Minimal Cognitive Demands

The studies that found suggestive evidence of EF benefits are Albinet et al. (2010), Dustman et al. (1984), Khatri et al. (2001), and Tuckman and Hinkle (1986). Albinet et al. used a challenging task (the Wisconsin Card Sort [WSCT]) to assess EF outcomes in sedentary seniors (65–78 years old, mean age of 71 years) after the relatively short period of 12 weeks of 60-min classes three times a week of either plain aerobic exercise or stretching. Those who did aerobic exercise showed a greater decrease in WCST errors than those who stretched (the control group got worse at posttest), although posttest scores did not significantly differ between the groups.

Dustman et al. (1984) assigned adults 55 to 77 years old to 60-min classes of aerobic walking or resistance training that met three times a week for 17 weeks, or to no treatment. On the Stroop test, those who did aerobic walking improved more and performed better than both comparison groups. On digit span (forward and backward combined) there was no difference between those who did aerobic walking and those who did resistance training; aerobic walkers showed a tendency to improve more and perform better on this than those in the no-treatment group, but that was not significant. Fluid intelligence/reasoning (as assessed by the Cattell Culture Fair test) was no better (nor more improved) after 17 weeks of aerobic walking than after resistance training or no treatment. Since three groups were pairwise compared on each of three outcome measures, Dustman et al. should have included a correction for multiple comparisons.

The other study (besides Hoffman et al., 2008) that compared exercise to antidepressant medication was by Khatri et al. (2001). They compared 16 weeks of three weekly 45-min sessions of plain aerobic exercise to antidepressant medication for clinically depressed middle-aged adults (mean age = 57 years). They found that aerobic exercise and medication each succeeded in reducing participants' depression. Exercise improved performance on one of their three EF measures (Stroop) *more* than antidepressant medication did. Trails B performance tended to improve more from exercise than medication, but that was not significant. Exercise did not benefit Backward-Digit-Span performance, while medication did slightly, but that difference, too, was not significant. Unfortunately, a no-treatment group was not included, so we do not know how much performance might have improved simply from practice taking the tests.

In a study by Tuckman and Hinkle (1986), children of 8 to 12 years did 30 min of aerobic running three times a week. After the relatively short period of 12 weeks, no benefit on maze tracing was found, but those who did aerobic exercise had better posttest scores on a measure of cognitive flexibility (the alternative uses task) than those in standard PE. We do not know if the training group started out better because neither pretest nor change scores are given.

Comparing Studies of Plain Aerobic Exercise Where an EF Benefit Was Observed on at Least Half the EF Measures to Studies Where an EF Benefit Was Observed on 30% or Less of the Measures

It is not obvious why some studies found at least a suggestion of benefit to EFs (and two found strong evidence of EF benefits) while other studies found no evidence of this or very little. A slightly larger percentage of the studies with older adults that found at least a suggestion of EF benefits included brisk walking as at least one component of their aerobic exercise program (100%) than studies that found little or no EF benefit (88%). However, half the studies that found an EF benefit on ≥ 50% of measures used fast walking as their sole aerobic activity, but also half the studies that found no EF benefit on any measure used fast walking as the intervention. The meta-analysis by Scherder et al. (2014) found that walking improved the EFs of previously sedentary older persons if they were without cognitive impairment but not if they had cognitive impairment. One recent study reports better psychological and health benefits from physical activity done outside in nature than from the same physical activities done inside (Calogiuri et al., 2015). When aerobic walking aided EFs, was it usually done outside while the other aerobic activities were usually done inside? It would be interesting to follow up on this intriguing finding.

Those who found at least a suggestion of benefits did not study programs that lasted longer. The mean length of the interventions in the five studies to find at least a suggestive benefit to EFs was 16 weeks and the longest length was 24 weeks (see Table 8.7). The mean length of interventions in the 10 studies that found virtually no EF benefit was 27 weeks and the longest length was 52 weeks. Half of the studies that found no EF benefit were longer than any study that found at least a suggestion of EF benefits. Perhaps there's some reason why continuing a plain aerobic-exercise intervention longer is not beneficial for improving EF outcomes.

Those who found at least a suggestion of benefits did not study programs that had longer sessions. The mean duration of sessions in the five studies to find at least a suggestive benefit to EFs was 46 min, and for just the aerobic portion it was 35 min (see Table 8.7). The mean duration of sessions in the 10 studies that found virtually no EF benefit was 57 min and for just the aerobic portion it was 42 min. Perhaps continuing aerobic exercise sessions beyond 45 to 50 min or the aerobic portion of those sessions beyond 30 to 40 min yields no additional benefits and perhaps is counterproductive.

Similarly, Gomes-Osman, Cabral, Morris, McInerney, Cahalin et al. (2018), who recently systematically reviewed RCTs examining the potential benefits of exercise on cognition in older adults (≥ 60 years), also concluded that neither dose, duration, nor frequency of physical activity affected cognitive outcomes.

They also looked at total time exercising. They report that a total of at least 52 hours of physical activity (aerobic, resistance training, or mind-body) was associated with improved speed of processing and EFs, but not working memory, in older adults with and without cognitive impairment. We calculated total time from Table 7 and find that more total time exercising conferred no EF advantage in the studies reviewed here. Indeed, among the studies reviewed here, those finding clear or suggestive evidence of EF benefits had 1/3–1/2 as many total hours as studies that found no or almost no EF benefit. That may be because, unlike Gomes-Osman et al., we included both children and adults (not just older adults), excluded most studies of adults with cognitive impairment, included quasi-experimental designs as well as RCTs, included working memory under EFs, and included only papers published through 2015. Note also that Gomes-Osman et al.'s recommendation of 52 hours of moderate-vigorous exercise is based on total session time; if the portion of the exercise session devoted to aerobic activity is used instead, the number of hours goes down to about 18–25 hours.

There was little difference in the number of participants per condition. Those who found at least a suggestion of EF benefit had a mean of 35 participants per group (range = 10–77). Those who found virtually no evidence of a benefit had a mean of 34 participants per group (range = 8–65; see Table 8.7). *There was little difference in the mean age of older adults in studies that found at least a suggestion of benefit to EFs and studies that found little or no evidence of an EF benefit* (67 vs. 70 years, respectively; age ranges were 55–78 and 55–85, respectively; see Table 8.7).

Two thirds (67%) of the studies using standard PE as the control condition found little or no benefit to EFs; 57% of the studies using stretching and toning as the control condition found little or no benefit; 60% of the studies using a no-treatment control group found little or no benefit. As mentioned above, the percentage of measures on which a greater EF improvement was found from plain aerobic exercise than in a control group was roughly 20%, regardless of whether the control condition was standard PE, stretching and toning, or no treatment. See Table 8.18.

Our earlier comment about monitoring and reporting compliance bears emphasizing here. How often did each participant attend his or her assigned sessions? Only 35% of plain aerobic activity studies reported this. How vigorously did each person participate in those sessions? Of course, people who rarely attended or rarely exerted themselves would not be expected to show much benefit. Similarly, if a person in a sedentary control group exercised vigorously during another part of the day, that too would minimize differences between groups. It would also complicate interpretation of results if people in a physical-activity condition exercised outside of (in addition to) that program. Very rarely did a study ask about, control for, or report monitoring participants' activities outside the intervention (this is less important for studies of sedentary adults, perhaps, but it is especially important for studies of children).

It could be that the critical difference between the studies where more or fewer EF benefits were found has to do with variables that few studies have reported, such as whether the group of participants developed significant camaraderie or not, whether the atmosphere created was one that fostered risking making a mistake versus one where participants worried about being embarrassed, characteristics of the activity leader (such as being supportive and not being punitive, having a strong conviction that EF benefits would be seen), or the physical environment in which the activity was done (e.g., outdoors in nature, outside in a city, or indoors). See the section entitled "Our Predictions About How to Most Effectively Improve EFs" for a fuller discussion of this.

Studies With Other Comparison Conditions or Additional Components to the Intervention Besides Plain Aerobic Exercise

Excellent results were achieved by Langlois et al. (2013). They studied adults 61 to 89 years old (mean age of 72 years, half of whom were frail) assigned to either business as usual or 60 min of physical exercise (aerobic exercise for 10–30 min plus resistance training for 10 min) three times a week for 12 weeks. They report only percent change and do not report posttest scores on any measures, so we do not know if there were any group differences in posttest performance. However, on both composite indices, one for inhibitory control (Stroop & Trails B minus Trails A) and one for WM (Backward Digit Span & Letter-Number Sequencing), those in the physical exercise group improved more. There was no difference on their measure of reasoning.

Two studies that looked at EF outcomes from aerobic exercise plus resistance, balance, and flexibility training found no evidence of any EF benefit at all compared to no treatment or health education (Sink et al., 2015; Williamson et al., 2009). See Table 8.18.

Masley et al. (2009) combined 30 to 45 min of aerobic exercise with training in stress management and a dietary intervention (so the benefits of aerobic exercise plus two additional components were investigated, not aerobic exercise alone) for adults 18 to 70 years old (mean age = 47). One group was assigned to aerobic exercise five to six times a week, another group to aerobic exercise three to four times a week, and a third group to no treatment. After 10 weeks, neither intervention group showed any benefit relative to no treatment for inhibitory control (Stroop test). Those who did aerobic exercise more often showed more improvement than controls on both composite EF indices. Those who exercised less often showed more improvement than controls on one of the composite EF indices. On no measure, however, was the posttest performance of either exercise group better than that of controls.

Relating the Results of This Review of EF Benefits From Plain Aerobic Activity to the Findings and Conclusions of Other Reviews

Consistent with the disappointing effects of plain, i.e., less cognitively demanding, aerobic exercise on EFs is the consistent finding that improvement in EFs and improvement in aerobic fitness are uncorrelated (for meta-analyses, see

Etnier et al., 2006; Young et al., 2015; for review, see Kramer & Erickson, 2007; also see Blumenthal et al., 1989; Davis et al., 2011; Smiley-Oyen et al., 2008).

Also consistent with our conclusion of disappointing EF benefits from aerobic exercise, a Cochran Review meta-analysis of 12 RCTs in older, cognitively healthy adults concluded that: "Overall *none* of our analyses showed a cognitive benefit from aerobic exercise even when the intervention was shown to lead to improved cardiorespiratory fitness. . . . Our analyses comparing aerobic exercise to any active intervention showed *no* evidence of benefit from aerobic exercise in any cognitive domain. This was also true of our analyses comparing aerobic exercise to no intervention" (Young et al., 2015, p.1, emphases added).

Older reviews have researched similar conclusions. Van Uffelen et al. (2008) reviewed five RCTs done with cognitively healthy older adults that looked at effects of aerobic exercise on EFs. Only one of those five studies (20%) found any benefit to EFs from aerobic exercise compared with control participants.

Kelly et al. (2014) reviewed 25 RCTs involving healthy older adults with no known cognitive impairment or any significant medical, psychiatric, or neurological problems. They concluded that "There is a lack of consistent evidence to show that aerobic interventions . . . result in improved performance on cognitive tasks for older adults without known cognitive impairment" (p.28). They found that on only 5% of EF measures did studies report significantly more EF improvement from aerobic exercise than from stretching/toning (positive results on only two out of 40 EF measures) or than from no-exercise active control conditions (positive results on only two out of 38 EF measures). Results were little better for aerobic exercise versus no treatment: Kelly et al. report that on only 12% of EF measures (five out of 12) did studies find more EF improvement from aerobic exercise than from no treatment.

Gates et al. (2013) reviewed 14 RCTs involving older adults with mild cognitive impairment. They concluded that only "trivial, nonsignificant effects were found for executive function" (p.1093) in their meta-analysis of aerobic exercise interventions.

Verburgh et al. (2014) conducted a meta-analysis of acute and chronic effects of exercise. Included in their analyses were three RCTs looking at the effects of chronic exercise in children and young adults (6 to 35 years old; Davis et al., 2011; Fisher et al., 2011; Stroth et al., 2010); they found "no significant overall effect of chronic physical exercise ($d = 0.14$, 95% CI -0.04 to 0.32, $p = 0.19$) on executive functions" (p. 973). Fisher et al. and Stroth et al. looked at plain aerobic exercise; Davis et al. looked at enriched.

Smith et al.'s (2010) meta-analysis in older adults found minimal benefits to EFs and none to WM. They included 16 RCTs that looked at the effect of aerobic exercise on EFs in cognitively healthy adults (in most, but not all cases, over 60 years old). Only one of those 16 studies found an effect size that was significant at $p < 0.05$ and that effect was significant at $p = 0.049$. Including the three studies on persons with mild cognitive decline brings the total number of studies to 19, but still only one had an effect size significant at $p < 0.05$. Two of the three studies showing the largest effects were not really studies of the benefits of aerobic exercise: Scherder et al. (2005) looked at slow walking (which is not aerobic) and Masley et al. (2009) looked at the benefits of stress management plus a dietary intervention plus aerobic exercise (which does not permit conclusions about the benefits of aerobic exercise per se). It's unclear what Smith et al.'s mean effect size for aerobic exercise benefits to EFs would have been with those two studies omitted, but it would certainly have been smaller. Twelve RCTs examined by Smith et al. looked at possible benefits of aerobic exercise to WM in adults (one of the 12 was with persons with mild cognitive decline); there were no WM benefits. The mean effect size across studies for the effect of aerobic exercise on WM was $g = 0.03$, ns. We, and most EF researchers, consider WM to be a component of EFs. If the studies Smith et al. included under WM had been combined with the studies they grouped under EFs, the mean effect size for that combined set of EF studies would not have been significant.

Consistent with these reviews are two cohort studies that followed people engaged in different activities. They found no protective role of physical activity in preventing cognitive decline. Wang et al. (2013) followed 1,463 adults in China without cognitive or physical impairment at baseline, age 65 at study entry, for 2.4 years. They found that cognitive activities, such as reading, playing cards, chess, or majiang, were associated with better EFs and less cognitive decline; social activities, such as visiting friends or family, were also associated with less cognitive decline. However, physical activities, whether they were walking or attending group exercise, were not associated at all with any reduced risk of cognitive decline.

Verghese et al. (2003) followed 469 adults in the United States living in the community and dementia-free at baseline, over age 75 at study entry, for 5.1 years. They found that reading, playing board games, playing a musical instrument, and especially social ballroom dance, were associated with a lower risk of dementia. However, physical activities such as walking, biking, swimming, or participating in group exercise, were not associated with any reduced risk of dementia.

One review concluded that aerobic activity does improve EFs of older, sedentary adults (Colcombe & Kramer, 2003). They included five studies of plain aerobic exercise reviewed here (Blumenthal et al. 1989; Dustman et al., 1984; Khatri et al., 2001; Kramer et al., 1999; Moul et al., 1995); we, too, found that four of those five studies found at least suggestive evidence of EF benefit. They also included a study by Williams and Lord (1997; which we discuss below because, in addition to aerobic exercise and resistance training, their exercise program included visuomotor coordination); we concluded that they, too, found suggestive evidence of EF benefits. On the small number of studies reviewed by Colcombe and Kramer (2003), we do not disagree with their conclusions. It is simply that a great many studies have been published since 2003.

Colcombe and Kramer also included two other studies that looked at EF outcomes that we have not included here (Emery, Schein, Hauck, & MacIntyre, 1998; Powell, 1974) because they both included clinical populations and we have included only very few such studies. Emery et al. looked at performance on Trails B and Verbal Fluency after 10 weeks of (Group A) aerobic exercise, education, and stress management, (Group B) just education and stress management, or (Group C) no treatment, among 67-year-olds with chronic obstructive pulmonary disease. They found more improvement in Verbal Fluency among those in Group A than those in no treatment, although there was no group difference in posttest performance or in improvement for Trails B. (Group B differed from neither Group A nor Group C on either measure in either improvement or posttest performance.)

Powell (1974) looked at fluid intelligence/reasoning (as assessed by Raven's Matrices) after 12 weeks of (Group A) what they termed "mild exercise" (brisk walking, calisthenics, and rhythmic movements), (Group B) a "social interaction" control condition (where participants played board games and did arts and crafts and music therapy together with others), and (Group C) business as usual among "institutionalized geriatric mental patients." They found more improvement on Raven's Matrices for Group A than Group C, although there was no significant difference at posttest. (Group B differed from neither Group A nor Group C in either improvement or posttest performance.) These two studies provide some support for an EF benefit from exercising.

In another review, Donnelly et al. (2016) included cross-sectional and longitudinal studies and studies of acute effects from a one-time exposure, which are not relevant to the present discussion. They also reviewed 10 reports of RCTs. All of those studies evaluated enriched aerobic exercise programs and are discussed in that section.

A Conundrum Concerning Aerobic Exercise and EFs

On the one hand, as just discussed, aerobic interventions do not seem to improve EFs. Many studies have found that whether or notEFs improve seems unrelated to whether aerobic fitness improves. On the other hand, people who are more physically active and have better aerobic fitness have been found repeatedly to have better EFs than those who are more sedentary (in children: Fedewa and Ahn, 2011; Gapin & Etnier, 2010; Hillman, Castelli, & Buck 2005; Scudder et al., 2014; Sibley & Etnier, 2003; in older adults: Boucard et al., 2012; Colcombe & Kramer, 2003; Voelcker-Rehage et al., 2011; at all ages: Etnier et al., 2006; Prakash, Voss, Erickson, & Kramer, 2015). Indeed, a computer simulation has estimated that a 5% reduction in physical inactivity among adults 45 or older in Australia would reduce dementia there by 11% (Nepal, Brown, & Ranmuthugala, 2010).

Perhaps people need to do aerobic activity for longer than has been investigated thus far in intervention studies (perhaps years vs. months). Many who are drawn to exercise have been physically active much of their lives, not just for the months or the year of a study.

People who freely choose to do aerobic activities probably enjoy them more than people who are randomly assigned to them. Evidence indicates that any cognitive benefit from physical activity may be proportional to how much the physical activity was enjoyed (Heyman et al., 2012; Raichlen, Foster, Gerdeman, Seillier, & Giuffrida, 2012). There is a biological reason why the ability of an activity to improve EFs may be proportional to how much joy the activity evokes (see the section "Our Predictions About How to Most Effectively Improve EFs" and Figure 8.7 in that section for elaboration of this point).

It may be that many who maintain better fitness do so by participating in physical activities that involve cognitive challenges and complex motor skills (such as ultimate Frisbee, squash, tennis, rock climbing, soccer, beach volleyball, social dance, or martial arts). Indeed, for people who regularly do what we have dubbed plain aerobic exercise, it is often not plain aerobic exercise for them. For committed runners or joggers, for instance, these activities are ripe with cognitive challenges as they strategically plan how, or if, they want to trade off speed and distance, minimize extra steps, etc. These activities can become exercises in mindfulness for them or provide the opportunity for exercising mindfulness. That is unlikely to be true for first-time exercisers assigned to an intervention. Thus, those who maintain a regular running regime by choice may do so more planfully or mindfully than those new to running (assigned to do it in some study). As we discuss under enriched aerobic exercise, however, interventions that have tried to specifically add cognitive and/or motor skill challenges have found results almost as disappointing as have studies of plain aerobic exercise.

For those who regularly engage in physical activities, these activities may be an important part of their social lives and/or an important source of pride and personal identity for them. The cognitive challenges, feelings of belonging, social benefits, feelings of pride and joy, and deep commitment to the activity and fellow teammates or exercise buddies may be critical to whether EFs benefit from these activities.

To the extent that aerobic exercise aids EFs, that might be because aerobic exercise improves mood (Khatri et al., 2001; Lane & Lovejoy, 2001; Williamson, Dewey, & Steinberg, 2001) and/or helps people sleep better (Foti, Eaton, Lowry, & McKnight-Ely, 2011; Loprinzi & Cardinal, 2011), given that EFs tend to be better when someone is happier and better rested (Borges et al., 2013; Hirt, Devers, & McCrea, 2008). As far as we know, those possibilities have not yet been investigated, except for mood in clinically depressed adults (Hoffman et al., 2008; Khatri et al., 2001).

Perhaps the correlation between better physical and cognitive fitness is due to one or more other variables and not to better fitness per se. Perhaps people who are more physically fit tend to eat better, get more sleep, or are healthier in general. Perhaps causality goes in the opposite direction: An individual probably needs good EFs, especially good inhibitory control and discipline, to maintain a regular exercise regimen.

In any case, the evidence so far seems to indicate that it is not aerobic fitness by itself that causes the cognitive benefit. Aerobic exercise interventions almost always improve aerobic fitness but less than half the time improve EFs. It is very possible, and we think probable, that engaging in physical activity does help EFs, but why that happens is not being consistently captured by physical-activity intervention studies. The section "Our Predictions About How to Most Effectively Improve EFs" discusses the possible aspects of physical activities important for improving EFs that have not be addressed in most intervention studies.

Physical Activity With More Cognitive and/or Motor Skill Demands (Enriched Aerobic Exercise)

General Comments

There have been a great many calls to move beyond an almost exclusive focus on plain aerobic exercise and resistance training to physical activities that tax EFs and motor coordination more (Best, 2010; Diamond, 2015; Ericsson, 2017; Ericsson & Karlsson, 2014, Moreau, 2015; Moreau & Conway, 2013; Myer et al., 2015; Pesce, 2012; Pesce, Leone, Motta, Marchetti, & Tomporowski, 2016; Pesce, Masci, et al., 2016; Sibley & Etnier, 2003; Tomporowski, Davis, Miller, & Naglieri, 2008; Tomporowski et al., 2015).

We had predicted that aerobic activity with EF demands would improve EFs more than aerobic activities with few EF demands (Diamond, 2015; Diamond & Ling, 2016; see also Best, 2010; Moreau, 2015; Pesce, 2012; Tomporowski et al., 2015). That prediction appears to have been confirmed. More studies of enriched aerobic exercise have yielded suggestive evidence of EF benefits than studies of plain aerobic exercise (see Figure 8.4a and Tables 8.1 and 8.2). In studies of enriched versus studies of plain aerobic exercise, more EF improvement than the control group was found on about twice as many measures (see Tables 8.1 and 8.2).

Three studies (Chuang, Hung, Huang, Chang, & Hung, 2015; Moreau, Morrison, & Conway, 2015; Schmidt et al., 2015) directly compared EF benefits from more versus less cognitively demanding aerobic activity (enriched vs. plain aerobic activity). Across the three studies, the evidence shows only a slight trend in the predicted direction. On only 57% of the seven measures across the three studies did enriched aerobic exercise improve EFs more than plain aerobic exercise. When Moreau et al. (2015) compared enriched aerobic exercise to WM training, they found comparable improvement and posttest scores; there were no significant differences in EF outcomes. That is quite impressive for enriched aerobic exercise. A study too new to be included in Tables 8.3 or 8.4 found more improvement on EFs from enriched than from plain aerobic exercise (Koutsandréou, Wegner, Niemann, & Budde, 2016).

Only two studies of enriched aerobic exercise (Chang et al., 2014; Williams & Lord, 1997) published by our cutoff date of 2015 found clear evidence (in both cases, more improvement and better posttest performance than the control group on 100% of their EF measures). Both studies, however, included only one EF measure. Williams and Lord also included a measure of reasoning/fluid intelligence (which we are considering far transfer for the WM training studies, so to be fair are not including it in calculations for other types of interventions). On that measure, they found neither more improvement nor better posttest performance among exercisers. These studies are discussed further in the section "Studies of Enriched Aerobic Exercise That Found Encouraging Results." (The study by Chang et al. is discussed under studies with children and the Williams and Lord study is discussed under studies with adults.)

Across all studies of enriched aerobic exercise, more improvement was found compared to control conditions on 36% of the 74 EF measures (twice as good as 18% of 66 EF measures for plain aerobic exercise) and better posttest performance than controls was found on 15% of the 41 EF measures for which data were reported (vs. 12% of the 60 measures for plain aerobic exercise). (See Tables 8.1, 8.18 and 8.19.)

Before proceeding further, we can mention one thing that is clear: Despite its widespread adoption by schools, there are no independent studies that have looked at whether Brain Gym® improves cognition in general or EFs in particular. That is, right now there is no evidence that Brain Gym improves either. An absence of evidence does not mean that Brain Gym might not, in fact, improve cognition, but it does mean that claims that such benefits have been established are untrue.

The percentage of measures on which more EF improvement was found in school-age children from enriched aerobic exercise than from the comparison condition was the same whether the comparison condition was PE or no-treatment (33%; see Table 8.19). For older adults, enriched aerobic exercise produced more improvement than any active control condition on 13% of EF measures. However, when enriched aerobic exercise was compared with no treatment for older adults, more benefit to EFs from enriched aerobic exercise was found on 45% of measures.

Weaker evidence of EF benefits from enriched aerobic activity was generally found for children than adults. For example, among children with no clinical diagnosis, enriched aerobic exercise resulted in more improvement on only 33% of 43 EF measures investigated and better posttest scores on only 8% of 25

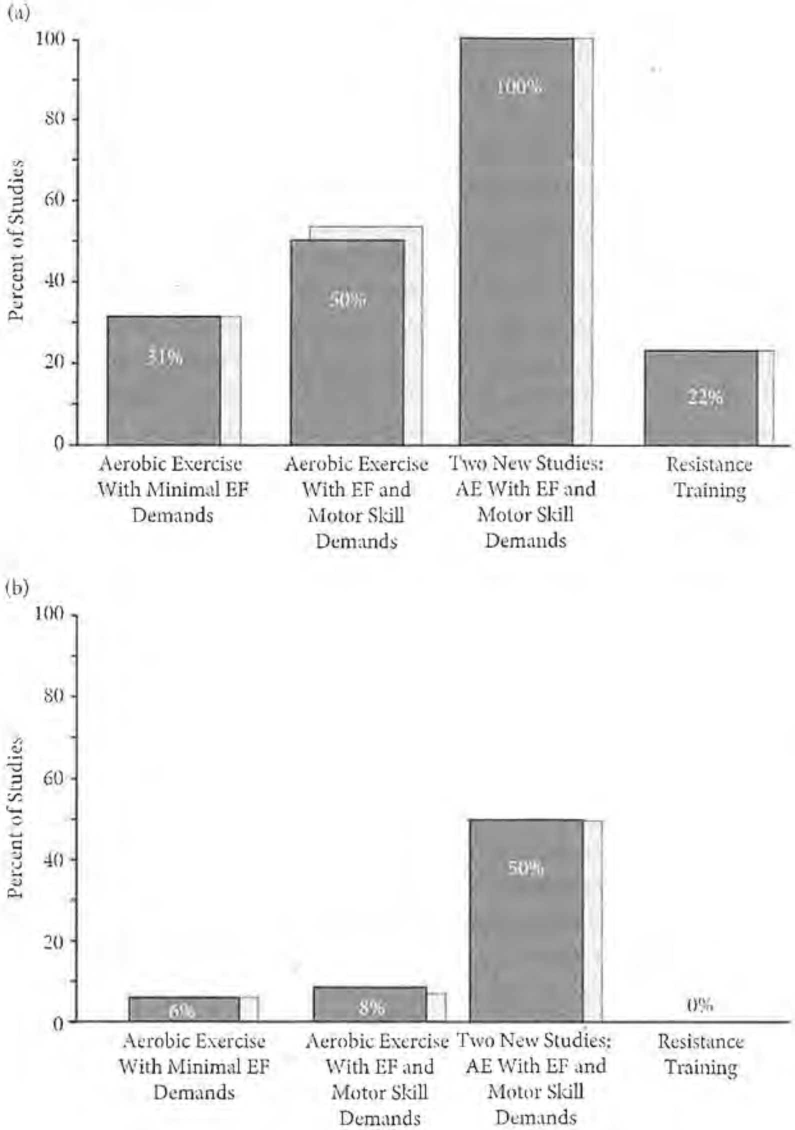

Figure 8.4. Success rates of plain and enriched aerobic exercise and resistance training for improving EFs. AE = aerobic exercise. The darker bars in the foreground present the results omitting studies with possibly spurious positive results. The lighter bars in the background present the results for all studies. Studies omitted for having positive results that might not have held up were those that had not corrected for multiple comparisons or had not conducted data analyses reflecting the level at which they randomized. Figure 8.4a: Percentage of studies finding at least suggestive evidence of physical activity benefiting any EFs, including reasoning (i.e., studies

measures, compared to control conditions. For adults with no clinical diagnosis, however, enriched aerobic exercise resulted in more improvement on 40% of 30 EF measures and better posttest scores on 15% of 13 measures compared to control conditions. (See Table 8.19.)

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where the experimental group showed either more improvement or better posttest performance than a comparison group on > 50% of the EF measures). Plain aerobic exercise ($N = 16$): Albinet et al. (2010), Kramer et al. (1999), Moul et al. (1995), Stroth et al. (2010), and Tuckman and Hinkle (1986). Enriched aerobic exercise ($N = 18$): Chang et al.(2014) Chuang et al. (2015), Kim et al. (2011), Maillot et al. (2012) Moreau et al. (2015), Predovan et al. (2012), Staiano et al. (2012), Williams and Lord (1997). Newer studies ($N = 2$): Alesi et al. (2016) and Koutsandréou et al. (2016). Resistance training ($N = 9$): Liu-Ambrose et al. (2008) and Molloy, Beerschoten, Borrie, Crilly, and Cape (1988). For all studies: Plain aerobic exercise ($N = 16$): Albinet et al. (2010), Dustman et al. (1984), Kramer et al. (1999), Moul et al. (1995), Stroth et al. (2010), Tuckman and Hinkle (1986). Enriched aerobic exercise ($N = 19$): Chang, Hung, Huang, Hatfield, and Hung (2014), Chuang et al. (2015), Gallotta et al. (2015), Kim et al. (2011), Maillot, Perrot, and Hartley (2012), Moreau et al. (2015), Predovan et al. (2012), Staiano, Abraham, and Calvert (2012), Williams and Lord (1997). Newer studies ($N = 2$): Alesi et al. (2016) and Koutsandréou et al. (2016). Resistance training ($N = 9$): Liu-Ambrose et al. (2008) and Molloy, Beerschoten, Borrie, Crilly, and Cape (1988). Figure 8.4b: Percentage of studies finding clear evidence that physical activity benefits any EFs, including reasoning (i.e., studies where there was both more improvement and better posttest performance by the experimental group than by a comparison group on ≥ 67% of the EF measures used). Whenever a study reported > 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations of strong evidence because it is possible the results of that study might have met our criteria had the results not reported been included. For studies with the needed statistical analyses: Plain aerobics ($N = 16$): Kramer et al. (1999). Enriched aerobic exercise ($N = 13$): Chang et al. (2014) and Williams and Lord (1997). Newer studies ($N = 2$): Koutsandréou et al. (2016). Resistance training ($N = 8$): none. For all studies: Plain aerobics ($N = 16$): Kramer et al. (1999). Enriched aerobic exercise ($N = 14$): Chang et al. (2014) and Williams and Lord (1997). Newer studies ($N = 2$): Koutsandréou et al. (2016). Resistance training ($N = 8$): none. If the FitKids studies are counted as three separate, independent studies, then for enriched aerobic exercise, 47% of 19 enriched aerobics studies showed suggestive evidence, and 13% of 15 studies showed clear evidence. A caveat about Hillman et al. (2014) and Kamijo et al. (2011): Their suggestive evidence for enriched aerobic exercise was due to greater improvement in the enriched aerobic group on ≥ 50% of their measures, but in all cases for those two studies that greater improvement might be an artifact of the group's starting out with worse EFs, since posttest scores were quite similar across groups.

Table 8.19. Percentage of Measures on Which Persons Who Were Trained on Aerobic Activity With Cognitive and/or Motor Skill Demands Showed More Improvement and/or Better Posttest Results Than a Comparison Group on Measures of Executive Functions, Except Reasoning/Fluid Intelligence, Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (5–17 YEARS OLD)											
1	Gallotta et al., 2015[A,B]	Coordinative-training PE intervention	Traditional PE intervention	2	3	67%	0	3	0%	0	3	0%	0	3	0%
2	Krafft, Pierce, et al., 2014, Krafft, Schaeffer, et al., 2014[C]	Jump rope, tag basketball, and soccer skills	Sedentary activities: Art and board games	2	10	20%	0	10	0%	0	4	0%	0	10	0%
3	Pesce et al., 2013[D]	PE with more cognitive demand taught by specialists	Standard PE (business as usual)	1	3	33%									
3	Pesce et al., 2013[D]	PE with more cognitive demand taught by specialists	Standard PE taught by specialists	1	3	33%									
4	Schmidt et al., 2015[E]	Team games: High aerobic & high cognitive condition	Aerobic exercise: High aerobic & low cognitive condition	1	3	33%	0	3	0%				0	3	0%

4	Schmidt et al., 2015	Team games: High aerobic & high cognitive condition	Standard PE: Low aerobic & low cognitive condition (business as usual)	1	3	33%	0	3	0%				0	3	0%
Totals and Percents for children who did aerobic exercise with cognitive and/or motor skill demands compared with any standard PE				3	9	33%	0	3	0%				0	3	0%
Totals and Percents for children who did enriched aerobic exercise compared with any physical-activity active-control condition				6	15	40%	0	9	0%	0	3	0%	0	9	0%
Totals and Percents for children who did enriched aerobic exercise compared with any active-control condition				8	25	32%	0	19	0%	0	7	0%	0	19	0%
SCHOOL-AGE CHILDREN (7–17 YEARS OLD)															
5	Chaddock-Heyman et al., 2013, Hillman et al., 2014, and Kamijo et al., 2011[F]	FITKids (aerobic exercise & motor skills)	No treatment	3	8	38%	1	8	13%	1	8	13%	1	8	13%
	Chaddock-Heyman et al., 2013[G]	FITKids (aerobic exercise & motor skills)	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Hillman et al., 2014[G]	FITKids (aerobic exercise & motor skills)	No treatment	1	4	25%	0	4	0%	0	4	0%	0	4	0%
	Kamijo et al., 2011[E,G]	FITKids (aerobic exercise & motor skills)	No treatment	2	3	67%	1	3	33%	1	3	33%	1	3	33%

(continued)

Table 8.19. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
6	Dalziell et al., 2015	Better Movers and Thinkers (BMT): Physical literacy, personal qualities, and thinking skills = performance	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
7	Davis et al., 2007, 2011[C]	Jump rope, basketball, and soccer skills: 40-min sessions	No treatment	1	3	33%	1	3	33%	1	3	33%	1	3	33%
7	Davis et al., 2007, 2011[C]	Jump rope, basketball, and soccer skills: 20-min sessions	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
1	Gallotta et al., 2015[A,B]	Coordinative-training PE intervention	No treatment	2	3	67%	0	3	0%	0	3	0%	0	3	0%
8	Reed et al., 2010[I]	Integrated physical activity into the teaching of language arts, math, and social studies	No treatment												

	Totals and Percents for children who did enriched aerobic exercise compared with no treatment			6	18	33%	2	18	11%	2	18	11%	2	18	11%
	Grand Totals and Percents for typically developing children who did enriched aerobic exercise compared with any active-control condition or no treatment			14	43	33%	2	37	5%	2	25	8%	2	37	5%
	CHILDREN (5–10 YEARS OLD) WITH A LEARNING DISORDER OR ADHD														
9	Chang et al., 2014	Aquatic coordination exercise program	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
	Grand Totals and Percents for children (with or without a clinical diagnosis) who did aerobic exercise with cognitive and/or motor skill demands compared with no treatment			7	19	37%	3	19	16%	3	19	16%	3	19	16%
	SCHOOL-AGE CHILDREN (7–17 YEARS OLD) WITH A LEARNING DISORDER OR ADHD														
	Westendorp et al., 2014[J]	Dynamic ball skills	Standard PE	0	2	0%	0	2	0%	0	2	0%	0	2	0%
	ADOLESCENTS (15–19 YEARS OLD)														
10	Staiano et al., 2012[C,K]	Competitive exergames	Business as usual				1	1	100%	1	1	100%			
10	Staiano et al., 2012[G,K]	Cooperative exergames	Business as usual				0	1	0%	0	1	0%			
	Totals and Percents for adolescents 15–19 years old who did enriched aerobic exercise compared with no treatment						1	2	50%	1	2	50%			
	Totals and Percents for participants less than 18 years old who did enriched aerobic exercise compared with no treatment			7	19	37%	4	21	19%	4	21	19%	3	19	16%
	Totals and Percents for participants less than 18 years old who did enriched aerobic exercise compared with any active-control condition or no treatment			15	44	34%	4	40	10%	4	28	14%	3	38	8%

(continued)

Table 8.19. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				ADULTS (18–52 YEARS OLD)											
11	Moreau et al., 2015[E]	Designed wrestling sport with EF demands	Aerobic exercise	2	2	100%	1	2	50%	1	2	50%	1	2	50%
				OLDER ADULTS (OLDER THAN 55 YEARS)											
12	Chuang et al., 2015	Dance Dance Revolution	Brisk walking	1	2	50%	0	2	0%				0	2	0%
13	Legault et al., 2011	Aerobic walking and flexibility training plus computerized cognitive training	Healthy aging education	0	6	0%	0	6	0%	0	6	0%	0	6	0%
	Totals and Percents for older adults who did enriched aerobic exercise compared with any active-control condition			1	8	13%	0	8	0%	0	6	0%	0	8	0%
	Grand Totals and Percents for adults (any age) who did enriched aerobic exercise compared with a vigorous physical-activity active-control condition			3	4	75%	1	4	25%	1	2	50%	1	4	25%
	Grand Totals and Percents for adults (any age) who did enriched aerobic exercise compared with any active-control condition			3	10	30%	1	10	10%	1	8	13%	1	10	10%

12	Chuang et al., 2015	Dance Dance Revolution	No treatment	1	2	50%	0	2	0%				0	2	0%
14	Kim et al., 2011	Latin dance—the Cha Cha (aerobic, social, partner dance)	Business as usual	1	2	50%	0	2	0%				0	2	0%
15	Klusmann et al., 2010[D]	Aerobic endurance, strength, coordination, flexibility, and balance training	No treatment	1	3	33%									
16	Maillot et al., 2012[D]	Exergames	Business as usual	3	4	75%									
17	Marmeleira et al., 2009	Aerobic activity plus task switching	Business as usual	0	4	0%	0	4	0%				0	4	0%
18	Predovan et al., 2012	Fast walking & aerobic dance exercise	No treatment	2	4	50%	0	4	0%	0	4	0%	0	4	0%
19	Williams & Lord, 1997	Aerobic, balance, & coordination exercise	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
	Totals and Percents for older adults who did aerobic exercise with cognitive &/or motor skill demands compared with no treatment			9	20	45%	1	13	8%	1	5	20%	1	13	8%
	Grand Totals and Percents for older adults who did aerobic exercise with cognitive &/or motor skill demands compared with any active-control condition or no treatment			10	28	36%	1	21	5%	1	11	9%	1	21	5%
	Grand Totals and Percents for adults (any age) who did enriched aerobic exercise compared with any active-control condition or no treatment			12	30	40%	2	23	9%	2	13	15%	2	23	9%

(continued)

Table 8.19. Continued

Study #	Study Name	Condition of Interest	Comparison Condition	Significantly Better Improvement # Sign.	# of Measures	% Sign.	Significantly Better Posttest # Sign.	# of Measures	% Sign.	Significantly Better Posttest Only Including Measures Where This Was Looked at # Sign.	# of Measures	% Sign.	Both Significantly Better Change and Posttest # Sign.	# of Measures	% Sign.
	Grand Totals and Percents across all ages comparing aerobic exercise with cognitive and/or motor skills demands to any physical-activity active-control condition			9	19	47%	1	13	8%	1	5	20%	1	13	8%
	Grand Totals and Percents for all ages of aerobic exercise with cognitive and/or motor skills demands compared with any non-physical-activity active-control condition			2	16	13%	0	16	0%	0	10	0%	0	16	0%
	Grand Totals and Percents across all ages of aerobic exercise with cognitive and/or motor skills demands compared with any active-control condition			11	35	31%	1	29	3%	1	15	7%	1	29	3%
	Grand Totals and Percents for all ages who did aerobic exercise with cognitive and/or motor skills demands compared with no treatment			16	39	41%	5	34	15%	5	26	19%	4	32	13%
	Grand Totals and Percents across all ages comparing aerobic exercise with cognitive and/or motor skills demands to any active-control condition or no treatment			27	74	36%	6	63	10%	6	41	15%	5	61	8%
	COORDINATION TRAINING WITH MINOR AEROBIC COMPONENT (NOT AEROBIC EXERCISE + COORDINATION TRAINING)														
	OLDER ADULTS (MEAN AGE OF 70 YEARS OLD)														
	Voelcker-Rehage et al., 2011	Coordination training	Relaxation & stretching	0	2	0%	0	2	0%	0	2	0%	0	2	0%

SCHOOL-AGE CHILDREN (7–17 YEARS OLD)														
Koutsandréou et al., 2016	Coordinative exercise	Aerobic exercise	1	1	100%	0	1	0%	0	1	0%	0	1	0%
Alesi et al., 2016	Soccer program	No treatment	1	2	50%	1	2	50%	1	2	50%	1	2	50%
Koutsandréou et al., 2016	Coordinative exercise	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
Totals and Percents for newer studies with children who did aerobic exercise with cognitive and/or motor skill demands compared with any active-control or no treatment			3	4	75%	2	4	50%	2	4	50%	2	4	50%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.19 (although they are mentioned in the text) but results for all other EF measures are included.

[A] Gallotta et al. (2015) randomized by school but appear to have analyzed the data as if they randomized by individual children.

[B] The authors of this study did not include a correction for multiple comparisons. It is unclear which of their results would remain significant had they done that.

[C] Participants were overweight.

[D] This study did not test the difference between posttest scores.

[E] One might plausibly expect EF benefits from aerobic exercise, so a failure to find a difference here might be due to both interventions' being beneficial, but instead results here were at least as promising as for other comparison conditions.

[F] In Kamijo et al. (2011) on the 3-Letter condition of the Sternberg test, the exercise group performed more poorly at pretest than controls. At posttest, both groups performed comparably. It's unclear whether the greater improvement by the children who exercised than controls reflects simply normal individual variation in developmental timetables, regression to the mean, or a benefit from the experimental condition. On the super-easy 1-Letter condition of the Sternberg test, the wait-list controls inexplicably got worse, making the difference in posttest scores between those who exercised and controls significant.

[G] These are the results if the FITKids studies are counted as three separate, independent studies. They are not included in the calculations for this table.

[H] The authors of this study did not conduct the needed multilevel data analysis. It is unclear how many of their results would remain significant had they done that.

[I] Reed et al. (2010) only included R/FL measures.

[J] Westendorp et al. (2014) randomized by class and had only three classes in each group; sample sizes of three gave them extremely low power to find any effect. All children had learning disorders and were attending special needs schools. It is unclear how much of an aerobic component this coordination training had, especially for the children who did not progress to the dynamic ball skills component. It is not included in our calculations.

[K] The difference in rate of improvement between groups was not tested.

The percentage of studies that involved children was only 18% for studies of plain aerobic exercise but 47% for enriched aerobic exercise. Only four of the nine studies that investigated enriched aerobic exercise in children less than 18 years old included an active control group. Only one study of possible EF benefits of enriched aerobic exercise has been done with adolescents and only one with adults younger than 55 years old. No study of enriched aerobic exercise has been done with children 3 to 6 years old.

Studies of Enriched Aerobic Exercise With Children That Found Encouraging Results

Chang et al. (2014) found that children with ADHD (5–10 years old, mean age = 8½) who did aquatic exercise (aerobic exercise plus motor coordination) for 40 min in a 90-min class, two times a week, for only 8 weeks improved more and achieved better posttest performance on the one EF test administered (an inhibitory control measure: go/no-go) than did wait-list controls.

Staiano et al. (2012) randomly assigned overweight, low-income African American adolescents (mean age: 16.5 years, range: 15–19 years) to competitive or cooperative exergames (e.g., Wii Sports) or no treatment. Those who did exergames did them for 30 min, an average of once a week (the sessions were offered every day of the school week), for 10 weeks. On their EF measure (a composite of Trail-Making and Design Fluency), those who did competitive exergames improved more than those who did cooperative exergames or no treatment; those in the cooperative condition did not improve significantly more than the no-treatment group. Whether there were group differences in posttest performance was not reported.

Three papers reported on outcomes of the FITKids intervention. Two of the three studies (Hillman et al., 2014; Kamijo et al., 2011) reported somewhat encouraging results. The FITKids afterschool program includes aerobic exercise, resistance, endurance, motor skills training (e.g., practice dribbling), games requiring cooperating with teammates, healthy snacks, and health education. It takes place for 120 min (of which 75–95 min is aerobic exercise) five times a week. Children were evaluated after 36 weeks in the program. On average, children attended 82% of the 180 FITKids sessions. All three FITKids' studies were with children roughly 8 to 9 years old from the same program; the Hillman et al. study included children also reported in the other two studies.

Hillman et al. reported more improvement in accuracy, although not speed, on both selective attention (Flanker) and cognitive flexibility (task switching) in children who had been through FITKids versus wait-list controls. There were no group differences in posttest scores on either measure, however. Wait-list controls had started out with better accuracy and by the end of the intervention the accuracy of both groups on both tasks was comparable. Kamijo et al. reported on another EF outcome measure, the Sternberg task. Controls started out better; the children in FITKids caught up. Most of the catching-up was due to the wait-list controls' inexplicably getting worse on the easiest condition (see Figure 8.3 in Diamond & Ling, 2016). Because the EF benefits reported in the two papers appear to reflect catch-up, we are concerned that they could be due to differences in developmental timetables rather to the FITKids program per se.

Studies of Enriched Aerobic Exercise With Adults That Found Encouraging Results
Williams and Lord (1997) reported both more improvement and better posttest performance among exercisers on their one EF measure. Women, whose mean age was 72, who did aerobic exercise plus exercises for balance and for eye–hand and eye–foot coordination plus resistance training for 42 weeks (twice a week for 50–55 min) improved more and performed better at posttest on a composite WM-STM measure (Forward and Backward Digit Span combined) than their peers in the no-treatment group. Attrition was high, however; 24% for the exercise group and 16% for the control group. No benefit to fluid intelligence/reasoning (Cattell Culture Fair Test) was found.

Chuang et al. (2015) found that women (mean age: 68 years) who did *Dance, Dance Revolution* (aerobic exercise + coordination + cognitive demands) for 30 min three times a week for 12 weeks improved more in speed but not accuracy on their one EF measure (Flanker task) than their peers who did brisk walking for the same amount of time (posttest scores were not given).

Maillot et al. (2012) had sedentary adults (mean age: 74 years; range: 65–78) do exergames (e.g., Wii Sports, Wii Fit) for 60 min two times a week for 12 weeks and compared them to no-treatment sedentary adults. On two measure of inhibitory control (Trails B and Stroop) and two measures of reasoning (matrix reasoning and verbal reasoning), those who did exergames improved more than those who remained sedentary (whether there was a group difference in posttest performance was not reported). The only EF measure on which there was no group difference in improvement was spatial span (a WM measure).

Moreau et al. (2015) randomly assigned adults (mean age: 30 years; range: 18–52 years) to (a) training on a "designed sport" based loosely on freestyle wrestling with added EF, sensory, and motor coordination demands, (b) WM training, or (c) aerobic exercise. All occurred three times a week for 60-min sessions for 8 weeks. Training on the designed sport produced outcomes as good as targeted WM training on WM (Backward Digit Span and Letter-Number Sequencing). As mentioned above, that is impressive. The designed-sport group also improved more on those two tasks than those who just did aerobic exercise. Their posttest scores were better than the aerobic-exercisers for Letter-Number Sequencing but not for Backward Digit Span. It appears that there were ceiling effects for Backward Digit Span, limiting its sensitivity to group differences.

Studies of Enriched Aerobic Exercise That Found Ambiguous Results

Two studies (Kim et al., 2011, and Klusmann et al., 2010) found EF benefits from enriched aerobic exercise on one EF measure but not on another. Kim et al. (2011) found that men and women (mean age: 68 years) who learned the Cha Cha (aerobic exercise + coordination + cognitive demands + social interaction [eye contact and touch] with partners) in 60-min classes (with 45 min of aerobic dance), two times a week for 26 weeks improved more in Verbal Fluency but not on Trails B than no-treatment controls.

Klusmann et al. (2010) randomly assigned older women (mean age: 74; range: 70–93) to (Group A) a physical exercise condition that included coordination, aerobic exercise, and work on balance, endurance, strength, and flexibility, (Group B) a mental exercise condition, where people did computer games that challenged memory and creativity, or (Group C) no treatment. Both the mental and physical exercise groups (Groups A and B) participated in 90-min sessions, three times a week, for 26 weeks. Both exercise groups (whether physical or cognitive) improved more than no treatment on Trail-Making (B-A) but not on Stroop or semantic Verbal Fluency (whether there was a group difference in posttest performance was not reported).

Predovan et al. (2012) had adults 57 to 80 years old (mean age = 68) do fast walking and aerobic dance exercise (within 60-min classes, where the aerobic portion gradually increased from 15 to 40 min) three times a week for a relatively short period (12 weeks). Aerobic exercise that included dance movements produced no relative benefits compared to no treatment for Stroop interference (saying the color of the ink of color words rather than reading them), which is the most commonly used Stroop outcome measure. However, on a more sensitive Stroop condition (where subjects had to switch between saying the ink color and reading the word), those who exercised improved more in both speed and accuracy. There were no posttest performance differences; indeed, the accuracy of aerobic exercisers at posttest was not as good as the pretest accuracy of controls: exercisers improved from a mean of 4.8 errors to 3.0, while controls improved from a mean of 2.5 errors to 2.2. The increase in aerobic capacity correlated with posttest RT on the more sensitive Stroop condition for the exercise group.

Davis et al. (2007, 2011) studied overweight, sedentary 7- to 11-year-olds who did aerobic games (which included basketball and soccer skills, jumping rope, and other activities that were mentally challenging and fun), 5 days a week for 13 weeks. When the aerobic games sessions were 40 min (but not when they were 20 min), children improved more in planning (their most demanding EF measure) and were better on planning at posttest than their peers who had not exercised, although there was no benefit to attention or sequence recall. The latter two may have been too easy to detect an effect. Those who got the 20-min sessions showed no significant cognitive benefit relative to the non-exercise group, yet they improved as much in physical fitness as did those assigned to 40-min exercise sessions (as assessed by a treadmill test of endurance).

Gallotta et al. (2015) studied children in Grades 3 to 5 randomly assigned to specially designed coordinative-exercise PE, traditional PE, or no PE. The two PE programs each consisted of two 60-min classes per week for 22 weeks. The middle 30 min of each class for both conditions consisted of moderate-to-vigorous activity. The traditional PE intervention focused on endurance, flexibility, resistance, and aerobic training. The coordinative-exercise PE intervention included skills used in sports games, rhythmic activities, and gymnastics. The one cognitive measure used was a measure of selective attention (the d2-R Test of Attention), for which three dependent measures were derived. Children in the coordinative-exercise group improved more than those in traditional PE and in the no-treatment group on both concentration (the number of letters correctly marked minus number of errors of commission [letters incorrectly marked]) and on percentage of errors (the number of errors of omission and commission divided by the total number of items). On neither measure was their posttest performance significantly better than either group. On percentage of errors, those in traditional PE started off extremely well and the coordinative-exercise group simply caught up.

Speed on the d2-R test (total number of items processed) improved more among those who did traditional PE than among children in either of the other two groups. The traditional PE group showed marginally better concentration at both pre- and posttest. Although their concentration score remained the best of all three groups at the end, the coordinative-exercise group showed the most improvement. Randomization for this study was done by school, but it appears the data were analyzed as if randomization had been done by child. It is unclear which, if any, of the findings would be significant if multilevel analyses had been done.

Westendorp et al. (2014) assigned children 7 to 10 years old with learning difficulties to either a ball skills intervention or standard PE (40-min sessions, two times a week for 16 weeks). The ball skills were first practiced in simple, static settings (e.g., playing catch with another child) in the hope of automatizing them. Once automatized, they were applied to dynamic sports settings (e.g., team games), where children needed to pay attention to teammates, opponents, game rules, and time, and where EF skills, including action planning, problem-solving, and cognitive flexibility, are thought to be critical, and where the aerobic component was greater.

Westendorp et al. found no group difference in improvement or posttest performance on any EF or academic skills measure (but they randomized by class and had only three classes in each group; sample sizes of three gave them extremely low power to find any effect). They did find that those who improved most in ball skills also showed the greatest improvement on the Tower of London task 6 months later. Westendorp's team speculates that those with weaker ball skills never progressed to practicing in dynamic sports settings where EFs are challenged; since their EFs were not challenged, they did not improve. Without progressing to the dynamic component, they were probably not aerobically challenged either, and either coordination exercise or aerobic exercise without the other might be less effective in improving EFs (see Marchetti et al., 2015).

Pesce et al. (2013) studied children 5 to 10 years old. Some were randomly assigned to PE that was intentionally more cognitively demanding and taught by a PE specialist, others were assigned to the standard PE curriculum taught by a PE specialist (the active control group), and still others were assigned to regular PE. These three PE conditions occurred for 60 min once a week for 26 weeks. They found no benefit to planning and no benefit from cognitively demanding PE on inhibition, as assessed by the Stroop task, but typically developing children who had been in cognitively demanding PE improved more than those in either of the other two groups in cognitive flexibility (task switching). Neither pre- nor posttest scores were reported, however, so we do not know if their task switching was better at posttest than the other two groups or if they simply caught up to the others. For children with developmental coordination challenges, the cognitively demanding PE condition was too challenging.

In a beautifully designed study by Schmidt et al. (2015), children 10 to 12 years old were randomly assigned to either exercise with high demands on both cognitive engagement and physical exertion (enriched aerobic exercise), exercise with high physical exertion demands but low cognitive demands (plain aerobic exercise), or exercise with low demands on both cognitive engagement and physical exertion (standard PE). That is the best study design of any of the extant investigations of the possible benefits of aerobic activity with cognitive demands. Unfortunately, the investigators found no differential benefits on the N-back or Flanker tests. The group that did enriched aerobic exercise improved more in cognitive flexibility (global switch costs) than the other two groups, but they started out worse than those groups and there was no significant difference in posttest switching performance (just in degree of change; see Figure 8.5). Thus, as with FITKids, the significant change reflects one group of children catching up

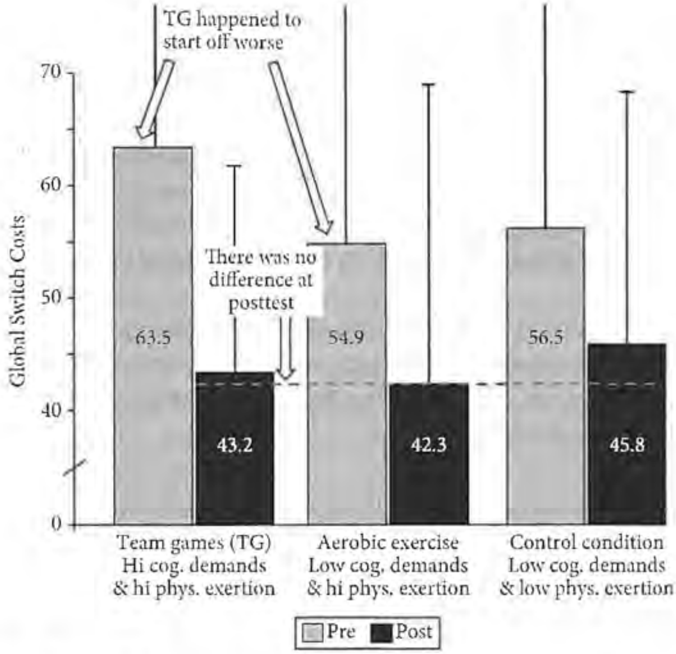

Figure 8.5. On one EF measure, Schmidt et al. (2015) found greater improvement from enriched aerobic exercise, but that seems to reflect catching up to the other children.

to the other children. It is important to note that Schmidt et al.'s (2015) intervention included only 12 sessions (two sessions per week for only 6 weeks). Perhaps if it had been more intensive or longer, greater differential benefits would have emerged.

Better Movers and Thinkers (BMT; Education Scotland Foghlam Alba, n.d.) is an approach to physical education developed in Scotland that specifically targets improving not only aerobic fitness, but also balance, gross motor coordination, rhythm, and timing, as well as EFs, confidence, determination, perseverance, and learning to work together with others in ways that children thoroughly enjoy. So far there is just an exploratory study on its feasibility (Dalziell, Boyle, & Mutrie, 2015). One class ($N = 25$) in one school received BMT and one class ($N = 21$) in another school received standard PE twice a week for 16 weeks. The children were 9 to 10 years old. Two EF measures were administered: nonverbal reasoning and visuospatial WM, both from the Lucid Assessment System for Schools (LASS). For analyses, the WM score was combined with the score on a STM measure (Forward Digit Span) also from LASS.

There were no significant group differences on either measure, although the BMT group improved significantly on the combined WM + STM composite and the control group did not. Phonological awareness improved more in the BMT group than among controls, and BMT children reported that they felt more confident and were better able to focus and concentrate when it came to their schoolwork. Since in this pilot study assignment to condition was by class and there was only one class per condition, if Dalziell et al. had analyzed their data reflecting that, they would have had no statistical power to detect any group difference.

Chang et al. (2013) found exactly the same EF results whether children did moderate- or low-intensity soccer practice. Since those were the only two groups in the study, we have no way of knowing if soccer practice benefitted both groups equally or benefitted neither.

Studies of Enriched Aerobic Exercise That Found No EF Benefits

Krafft, Schaeffer, et al. (2014) and Krafft, Schwarz, et al. (2014) had overweight 8- to 11-year-olds do 40 min of aerobic games every school day for 34 weeks. They found no improvement in inhibitory control (anti-saccade task) or selective attention (Flanker task) compared to peers assigned to sedentary activities, such as art and board games. For a small subset of participants (who also underwent neuroimaging), Krafft, Schaeffer, et al. also reported teachers' ratings on the BRIEF. Teachers rated those who exercised as more improved on the Global Executive Composite and Metacognition Indices (but not the Behavioral Regulation Index) than those who did sedentary activities (posttest scores for the BRIEF are not given, so we do not know if the exercisers were scored higher than the other group or simply caught up to them).

A third report on EF outcomes from FITKids (Chaddock-Heyman et al., 2013), in addition to the two reports discussed above, looked at two inhibitory control outcome measures (Flanker and go/no-go) and found neither more improvement nor better posttest scores on either in the FITKids group versus controls. Both groups made fewer errors on incongruent Flanker trials on posttest than they had on the pretest (i.e., both groups improved in inhibitory control). This improvement was significant for the FITKids group but not for controls. However, when the improvement of the two groups on incongruent Flanker trials was compared, there was no significant difference.

Marmeleira, Godinho, and Fernandes (2009) randomly assigned sedentary older adults (mean age: 68 years, range: 60–82 years) to aerobic exercise that intentionally included demands on inhibitory control, WM, planning, and processing speed (done three times a week for 60 min at a time for 12 weeks) or to no treatment. They found no EF benefits.

Two Studies Too Recent To Be Included in Tables 8.3 or 8.4 Or Our Tabulations, But That Deserve Mention

Alesi et al. (2016) found that children who practiced soccer showed more improvement and better posttest scores on the Tower of London than did sedentary control subjects, although on the easier Corsi Block spatial WM test there was no group difference. The mean age in both groups was 9 years.

Koutsandréou et al. (2016) found that children 9 to 10 years old who did coordinative exercise improved more in WM (as assessed by digit span) than those who did aerobic exercise, but again that might be only because the former group started behind (posttest scores were comparable for both groups). However, compared to a control group that did homework assignments while the other two groups were exercising, the coordinative-exercise children both improved more and had better posttest scores. That was not true for the plain-aerobic-exercise group.

In addition, it is worth mentioning the study by Ishihara et al. (2017), which looked at the EF benefits for 6- to 12-year-old children of learning to play tennis the traditional way (by practicing individual skills, such as the forehand swing) versus learning to play by actually playing tennis (in a modified, age-appropriate way).[12] They found greater EF benefits from playing tennis than from practicing tennis skills in isolation. Those who learned tennis by playing tennis improved on all three EF measures and on all three core EF skills (inhibitory control [Stroop task], WM [2-back task], and cognitive flexibility [task switching on a global-local task]). Those who learned tennis by practicing individual skills improved only on WM (2-back task). On a composite of the three EF measures, the tennis-playing group showed more improvement than the skill-practicing group. On posttest scores, however, there were no group differences. Children were not randomly assigned, so the children in the two groups might have differed a priori in ways that affected the results. Also, although the children in each group had been receiving tennis instruction for some time, pretesting was done before one tennis lesson and posttesting was done after that one lesson.

A Study of Coordination Training With Less of an Aerobic Component

Voelcker-Rehage et al. (2011), as mentioned above, found no greater benefit on the Flanker task in either speed or accuracy from aerobic walking versus stretching exercises. They also found no greater benefit on the Flanker task from

[12] The authors' rationale for their study was that since playing tennis requires more top-down cognitive control and puts greater demands on the abilities to override automatic response tendencies (inhibitory control) and to flexibly adjust in real time (cognitive flexibility) than doing repetitive exercises to work on tennis techniques (e.g., ball feeding), playing tennis should improve EFs more than repetitive exercises.

coordination exercises that addressed fine and gross motor skills, including balance, eye–hand coordination, leg–arm coordination, spatial orientation, and reacting to moving objects. The coordination group showed more improvement than those who did stretching exercises on the only non-EF cognitive measure (a visual-search task). The participants in the study were older adults (mean age = 70 years) and their exercise sessions were 35–50 min, three times a week. These results are consistent with neither coordinative exercise nor aerobic exercise being very effective in improving EFs without the other. Of course, the critical evidence missing here are results from a combined aerobic exercise + coordination training group.

Discussion of Results for Studies of Enriched Aerobic Exercise

Just as with studies of plain aerobic exercise, it is *not* that the studies that found clear or suggestive evidence of EF benefits from enriched aerobic exercise investigated programs that lasted longer, had longer sessions, were more frequent, or had more participants than studies that found few if any EF benefits (see Table 8.7 above).

Also as mentioned for plain aerobic exercise, perhaps studies have not found more EF benefits because participants do not do the activities for long enough. That might be even more critical for enriched aerobic exercise because it takes time to develop motor skills to the point where serious EF challenges can be added in a dynamic context. For example, Westendorp et al. (2014) speculate that participants with weaker ball skills never progressed to practicing in dynamic sports settings where EFs were challenged; since their EFs were not challenged, they did not improve.

Both studies of exergames (Maillot et al., 2012; Staiano et al., 2012) found encouraging results. This deserves further investigation.

While combining aerobic exercise with coordination training does not guarantee EF benefits, since several studies of enriched AE have failed to find EF improvements, it does seem clear that either coordination training or aerobic exercise alone, without the other, is even less likely to improve EFs. No benefit to EFs have been found from coordination training with less of an aerobic component (Voelcker-Rehage et al., 2011; Westendorp et al., 2014). Fewer EF benefits are found from plain aerobic exercise than from aerobic exercise with more cognitive and motor skill demands (see Table 8.7).

Too many studies of enriched aerobic exercise included only one EF measure. To seriously investigate EF benefits, more than one or even two measures should be used.

It may be that the participants need to engage in a sport, or an activity like dance, rather than do exercises drawn from that sport or activity done out of context. Certainly, correlational studies consistently show that athletes show better

EFs than non-athletes. For example, basketball players show particularly good selective attention (Kioumourtzoglou, Kourtessis, Michalopoulou, & Derri, 1998) and baseball players are particularly good at response inhibition (Kida, Oda, & Matsumura, 2005).

This hypothesis is consistent with the findings of Pesce, Masci, et al. (2016), who found that when children used the ball skills on which they were trained in a lot of their own outdoor play, more of an EF benefit was seen. The ball skills became something the children needed for something they wanted to do. If people learn something far better when they need it for something they want to do, as has repeatedly been shown (e.g., Cordova & Lepper, 1996; Freeman et al., 2014; Olson, 1964), training them in skills isolated from their use in a real-world activity seems ill-advised.

Participants are more likely to be emotionally invested in a sport than in decontextualized exercises, and their emotional investment may be key to whether that activity, even if it challenges EFs, ends up improving EFs (see the section "Our Predictions About How to Most Effectively Improve EFs"). One way to increase emotional investment is to give participants even a small decision-making role in the training activity, giving them some say in how the activity is organized or done (Ackerlund Brandt, Dozier, Juanico, Laudont, & Mick, 2015; Khan, Nelson, & Whyte, 2014; Williams, Cox, Kouides, & Deci, 1999).

Moderate- or high-intensity aerobic exercise may be more beneficial for EFs than low-intensity exercise (Hsu, Best et al., 2018). If so, it is not clear that high intensity yields better EF outcomes than moderate intensity (e.g., high- and lower-intensity soccer practice were found to produce similar EF gains by Chang et al., 2013). On the other hand, Coe, Pivarnik, Womack, Reeves, and Malina (2006) report that children who engaged in vigorous physical activity outside of school did better academically than those who exercised at only a moderate level (EFs were not assessed). Marchetti et al. (2015) report that motor-skill training improved inhibitory control only of those teenagers who were stronger and more aerobically fit. The role of intensity in directly affecting EF improvement or in moderating the effect of other features of an intervention on EFs might be worth further study.

An alternative perspective is provided by Tomporowski and Pesce (personal communication, September 23, 2017), who argue that while moderate-to-vigorous exercise yields more physical fitness benefits than lower-intensity exercise, aerobic intensity might not be central to cognitive benefits. They argue that what is driving the EF benefits from enriched aerobic exercise, such as sports, are the cognitive demands of the activity and that, during much of the skills training, aerobic demands are relatively low.

Based on their systematic review of cross-sectional, longitudinal, cohort, RCT, and acute-effects (from a one-time exposure) studies in children, Donnelly and colleagues (2016) concluded, "Based on the evidence available . . . PA has a positive influence on cognition" (p. 1197). Relevant here are the 10 reports on RCTs they reviewed. They counted each report as an independent datapoint (repeatedly saying that the 10 papers represent 10 studies). However, the two papers by Davis et al. (2007, 2011) were about the same RCT (the first paper contained a subset of the participants included in the second), the three papers by Krafft were about the same RCT (Krafft, Pierce et al., 2014; Krafft, Schaeffer, et al., 2014; Krafft, Schwarz, et al., 2014; with the first two papers reporting on a subset of the participants included in the third paper), and the four papers on FITKids were about the same implementation of that program (Chaddock-Heyman et al., 2013; Hillman et al., 2014; Kamijo et al., 2011; Monti, Hillman, & Cohen, 2012; the paper by Hillman et al. included participants included in the three other papers). The tenth paper was the soccer study by Chang et al. (2013), which, as already mentioned, compared more- to less-intensive soccer practice and found no difference in EF outcomes.

Eight of the 10 papers included in Donnelly et al.'s (2016) review are included in the present review. The study by Krafft, Pierce, et al. (2014) is not included here because it reported brain imaging outcomes, not cognitive outcomes. The paper by Monti et al. (2012) is not included because the only cognitive outcome they looked at was relational memory, and it is not clear that falls under EFs. In any case, Monti et al. found no benefits to relational memory, so that would only further reinforce our point that there are few EF benefits from enriched or plain aerobic exercise interventions.

Donnelly et al. (2016) wrote that multiple measures of cognition were included in all 10 of the papers on RCTs they reviewed. That is incorrect. Chang et al. (2013) looked at just the Flanker test, Kamijo et al. (2011) at just the Sternberg test, and Monti et al. (2012) at just a single measure of relational memory. Krafft, Pierce, et al. (2014) included no posttest measure of cognition. Donnelly et al. counted Kamijo et al. as providing evidence of the cognitive benefits of physical activity, but that is based on the wait-list control group inexplicably getting worse at posttest on the easiest condition. Chaddock-Heyman et al. found a benefit to speed of processing but none to EFs. Donnelly et al. note that, on incongruent trials on the Flanker task (which assess the EF ability inhibitory control), Chaddock-Heyman et al. found significant improvements in accuracy for those in FITKids but not for wait-list controls, but Donnelly et al. neglected to note that when Chaddock-Heyman et al. directly compared those change scores, they were not significantly different between the two groups. Donnelly et al. also do not mention that Chang et al. found no difference in cognitive outcomes for their two groups, making it impossible to draw a conclusion about any benefits. In sum, we find Donnelly et al.'s conclusion that, "Overall, the results of studies using RCT designs have consistently demonstrated significant improvements in the treatment groups, particularly for EF tasks" (p. 1204) to be unjustified.

In their systematic review of physical-activity intervention studies in children (most of which involved enriched aerobic exercise), Singh, Saliasi, et al. (2018) found five studies that they rated as high in methodological quality and that looked at the effects of physical activity on cognitive performance. They found that only 10 out of the total of 21 analyses (48%) conducted by the five studies showed a significant beneficial effect of physical activity on cognition. The authors concluded, "There is currently inconclusive evidence for beneficial effects of PA interventions on cognitive and overall academic performance in children."

Northey, Cherbuin, Pumpa, Smee, and Rattray (2017) do not report statistical analyses for EFs but only for "global cognition" (which included attention and memory in addition to EFs), so it is not possible to draw conclusions about possible benefits of physical activity specifically for EFs from that review.

The review by Vazou, Pesce, Lakes, and Smiley-Oyen (2019) included many studies that looked at other aspects of cognition, not EFs. Since studies with and without EF outcome measures were combined in their analyses, as well as EF and non-EF outcomes within a study, it is not possible to draw any conclusion about possible benefits of physical activity specifically for EFs from that review.

Resistance Training

Resistance training (also known as strength training or weight training) involves moving your limbs against resistance (anything that makes movement more difficult). Such resistance can be provided by your body weight, gravity, stretch bands, or weights. The most proximal benefits expected from resistance training are improved muscle strength and tone and improved endurance. There is little evidence that resistance training improves EFs. No study has found strong evidence of resistance training's aiding EFs. Only two studies of resistance training (Liu-Ambrose et al., 2008, and Molloy et al., 1988) have found EF benefits on 50% of assessment measures and none on more than that (see Figure 8.4 above and Table 8.20 below). Only one study of resistance training (Liu-Ambrose et al., 2008) has found better posttest performance on any EF measure when comparing those who did resistance training to any control group (and that study found better posttest scores on only two of its four measures [50%] using the low bar of a no-treatment control group; see Table 8.20).

Across the four studies that compared resistance training to toning and stretching, resistance training improved EFs more on four of 14 EF measures (29%) and produced no instances of better posttest performance. Across the four studies that compared resistance training to no treatment, resistance training

Table 8.20. Percentage of EF Measures (Except Reasoning/Fluid Intelligence) on Which Persons Who Did Resistance Training Showed More Improvement and/or Better Posttest Results Than Comparison Groups Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				OLDER ADULTS (OLDER THAN 55 YEARS)											
1	Cassilhas et al., 2007[A]	Resistance training	Toning & stretching	1	3	33%									
2	Liu-Ambrose et al., 2010, 2012	Resistance training + balance (1 time per week)	Toning & stretching	1	5	20%	0	5	0%	0	5	0%	0	5	0%
2	Liu-Ambrose et al., 2010, 2012	Resistance training + balance (2 times per week)	Toning & stretching	2	5	40%	0	5	0%	0	5	0%	0	5	0%
3	Moul et al., 1995	Resistance training	Mild range-of-motion & flexibility exercises	0	1	0%	0	1	0%	0	1	0%	0	1	0%
	Totals and Percents for older adults who did resistance training compared with toning and stretching			4	14	29%	**0**	**11**	0%	**0**	**11**	0%	**0**	**11**	0%
4	Kimura et al., 2010	Resistance training	Health education	0	2	0%	0	2	0%	0	2	0%	0	2	0%

5	Smiley-Oyen et al., 2008	Resistance training + flexibility exercises	Aerobic exercise using equipment	0	4	0%	0	4	0%	0	4	0%	0	4	0%
6	Brown et al., 2009	Resistance training + balance	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
7	Dustman et al., 1984	Resistance training	No treatment	0	2	0%	0	2	0%	0	2	0%	0	2	0%
8	Liu-Ambrose et al., 2008	Resistance training	No treatment	2	4	50%	2	4	50%	2	4	50%	2	4	50%
9	Molloy et al., 1988	Resistance training	No treatment	1	2	50%	0	2	0%	0	2	0%	0	2	0%
	Totals and Percents for older adults who did resistance training compared with no treatment			3	11	27%	2	11	18%	2	11	18%	2	11	18%
	Grand Totals and Percents for older adults who did resistance training compared with any active-control condition			4	20	20%	0	17	0%	0	17	0%	0	17	0%
	Grand Totals and Percents across all resistance training studies compared with any active-control condition or no treatment			7	31	23%	2	28	7%	2	28	7%	2	28	7%

Note. Results for reasoning/fluid intelligence are not included in Table 8.20 (although they are mentioned in the text) but results for all other EF measures are included.

[A] This study did not report the difference between posttest scores.

improved EFs more on three of 11 measures (27%) and produced better posttest performance on two of 11 measures (18%). It should be noted, though, that three of the eight studies that looked at EF benefits from resistance training had hoped not to find them, since resistance training was the active control condition, not the condition of primary interest.

Similar conclusions have been reached by Fedewa and Ahn (2011), Snowden et al. (2011), Gates et al. (2013), and Kelly et al. (2014) from their meta-analyses.

Fedewa and Ahn concluded, "No significant effects of physical-activity program [on children's academic achievement or general cognitive outcomes (not EF-specific)] were found when resistance training or combined training was applied" (p. 527). Snowden et al., who looked at studies of community-dwelling older adults, concluded, "None of the intervention categories had sufficient or strong evidence of effectiveness in maintaining or improving cognition. . . . [Two of these categories consisted of] studies of strength training on general cognition and executive function" (p. 706).

Gates et al. stated: "Resistance training was provided in two trials and produced . . . nonsignificant results on executive function" (p.1093). Kelly et al. reported finding no EF benefits in their meta-abalysis from resistance training versus no-exercise active controls (3 RCTs), although they found significantly more improvement in reasoning, but not in WM, when comparing resistance training to stretching/toning (3 RCTs). Studies included in Kelly et al.'s review reported significantly more improvement from resistance training than no-exercise active controls on only two out of 11 separate EF measures (18%) and significantly more improvement from resistance training than stretching/toning on only four out of 18 separate EF measures (22%).

For the present review, we found nine studies that examined EF performance before and after a regimen of resistance training. All nine were with older adults. In two studies (Dustman et al., 1984; Smiley-Oyen et al., 2008), resistance training was the active control condition, not the activity of primary interest; no EF benefits were found from resistance training in these studies.

In the study by Moul et al. (1995), both aerobic walking and resistance training were the activities of interest. On neither subscale of RIPA, nor on problem-solving and abstract reasoning, nor on word fluency and semantic categorization, did those who did resistance training outperform those who did flexibility training.

The most encouraging findings for resistance training were found by Liu-Ambrose et al. (2010) and Liu-Ambrose, Nagamatsu, Voss, Khan, and Handy (2012). They found that after a year of resistance training (52 weeks), women (mean age = 69 years) improved more on two different measures of inhibitory control than their peers who had done balance and toning exercises 60 min twice a week for the same 52 weeks. The two measures were Stroop interference (RT difference on a block of incongruent trials vs. a block of colored x's) and the Flanker Effect ([RT difference on incongruent minus congruent trials] divided by RT on congruent trials). The benefit on Stroop was true whether participants did resistance training once or twice a week. The benefit on Flanker was only true for those who did resistance training two times weekly. Posttest performance did not differ across groups on either the Stroop or Flanker task; only change scores differed. On EF measures that less often show treatment effects (RT on incongruent Flanker trials, Backward Digit Span minus Forward Digit Span, and Trails B minus Trails A), there were no group differences in either improvement or posttest scores.

Cassilhas et al. (2007) found greater improvement on visuospatial WM (Backward Corsi Blocks) and verbal STM (Forward Digit Span) after men (mean age = 68 years) did 24 weeks of resistance training (whether high or low intensity) than after 24 weeks of stretching and warm-up exercises. Posttest scores were not given. No benefit to verbal WM (Backward Digit Span) or spatial STM (Forward Corsi Blocks) was found from resistance training.

With older adults (mean age = 82 years) who had fallen and were at risk of further falls, Liu-Ambrose et al. (2008) investigated the benefits of 26 weeks of a home-based balance and strength retraining program. Note that this involved not just resistance training but also training in balance. They found more improvement and better posttest performance in speed and accuracy on the Stroop test from this program than business as usual, but no group difference in either change or posttest on Backward Digit Span or Trails B.

Molloy et al. (1988) found less decline in Verbal Fluency in 82-year-old women who did 13 weeks of resistance training compared with no-treatment controls—but posttest scores on Verbal Fluency did not differ between the groups (controls started out somewhat better but by posttest had declined to almost the level the exercise group was at). On digit span, again the controls started out slightly better, but both groups held their own, so that pre- and posttest scores were comparable within group and between groups.

Kimura, Obuchi et al. (2010) failed to find a benefit on task switching from 3 months of resistance training. They randomly assigned participants to either resistance training (combination of leg press, knee extension, hip abduction, and rowing) or a health-education course. There were no significant between-group differences in RT or in accuracy. Kimura et al. speculated that this result might be due to an insufficiently long or intense resistance-training program.

Mindfulness Training (Including More Sedentary Mindfulness As Well As More Physically-Active Mindfulness, Such as Yoga or Taekwondo)

All mindfulness approaches have in common quieting and focusing the mind, inhibiting internal and external distraction, so that one stays fully present to the current moment. Thirty-nine (39) studies looked at possible EF benefits from training involving some form of mindfulness. After one initial study each of Taekwondo and yoga showed promising results, follow-up studies of Taekwondo and yoga have found more disappointing outcomes (with the notable exception of a study of yoga by Gothe, Kramer, & McAuley, 2014, 2017). Very promising results have been found for two different mindfulness training methods both based on Chinese mind–body practices (Chan et al., 2013; Tang et al., 2007) and for a school program called MindUP. Quadrato Motor Training holds some promise and is worth further study. Indeed, mindfulness practices involving movement have yielded extremely promising results for improving EFs, better than for mindfulness practices primarily done seated and considerably better than for many movement activities without a mindfulness component.

More EF benefits from sitting mindfulness might be found if initially stressed individuals were the study participants. Mindfulness practices reduce stress and stress impairs EFs. Thus, helping severely stressed people feel calmer and less stressed should improve their EFs. Four studies discussed below report results consistent with this prediction (one by Bilderbeck, Farias, Brazil, Jakobowitz, & Wikholm [2013]; one by Gothe, Keswani, & McAuley [2016] and Gothe et al., [2014, 2017], and one each by Jensen, Vangkilde, Frokjaer, & Hasselbalch [2012] and Jha et al.[2015]).

Yoga

Yoga might be considered a dynamic form of mindfulness or a mindful physical activity. We decided to tabulate the results separately for yoga and other mindfulness activities involving movement before looking at the results, based solely on there being enough studies of yoga for it to merit its own table. There are many forms of yoga, but all involve physical movement and postures that emphasize flexibility and balance (asanas), breathing (pranayama), and meditative exercises (dhyana). Unfortunately, yoga intervention studies rarely report the amount or proportion of time spent doing each of the individual components.

Also, it is difficult to discern from written reports how faithful any program has been to the centuries-long mindfulness tradition behind yoga. Indeed, Sullivan et al. (2018, p. 6) noted that "Much of modern yoga practice focuses primarily on physical postures and movement sequences, [but] the traditional roots are centered on a philosophical path towards understanding the causes of suffering and its alleviation (Mallinson & Singleton, 2017; Stoler-Miller, 1998)." That is quite important, for as Trulson (1986) found for taekwondo, when the traditional mindfulness practice is emphasized, positive outcomes are found, but when just the physical exercise aspect is emphasized, positive outcomes are not found. The best EF outcomes from yoga are reported by the most recent and the earliest studies.

The most recent study (Gothe et al., 2014) had an excellent experimental design. Older adults with a mean age of 62 years (range = 55–79) were randomized to either hatha yoga with poses like warrior pose and sun salutations plus deep breathing exercises and meditative exercises or to stretching and resistance training without yoga poses or breathing or meditative exercises. Both conditions were delivered by certified trainers and included comparable levels of social interaction with the trainer, research staff, and others in the group. The conditions only lasted a short time (8 weeks) but both were given in 60-min classes three times each week. It is not known if the trainers for the control group knew the experimental hypotheses or if the testers were blind to the hypotheses or group assigments of the participants.

It is exciting that benefits from yoga were found on all three of their EF outcome measures: task switching (which assesses cognitive flexibility), N-back (which requires WM and inhibitory control), and the running memory complex-span task (which also requires WM and inhibitory control).[13] Impressively, for all three tasks, although the group that did yoga performed better on the experimental condition, both groups performed comparably on the control condition. For task switching there was no difference between the groups on the single task block (the control condition). Those who did yoga needed to slow down less to preserve their accuracy on both kinds of trials in the mixed block (switch trials and repeat trials). On both of those trial types, their speed improved more and was faster at posttest than those who did stretching and resistance training. Accuracy was high on both trial types in both groups, so it was not that the two groups chose different speed–accuracy trade-offs. The percentage of correct responses on both types of trials was marginally higher and showed marginally more improvement among those who did yoga; the differences in accuracy were probably not significantly different between the two groups because of ceiling effects.

Again, on the N-back task, there was no difference between the groups on the control condition, 1-back. In the 2-back condition, those who had done yoga improved more than those who had done stretching and resistance training. Indeed, the latter group showed no posttest improvement on the task. For the running span task, on the trials where there were no distractors, the two groups

[13] Enthusiasm should be tempered a bit, however, because they did not correct for multiple comparisons. Not all their positive findings might still be significant had they done that.

performed comparably. On the key trials where one or two distractors appeared at the beginning of the string of numbers to be recalled, accuracy improved more among those who had done yoga than among control subjects. Indeed, the latter group showed no posttest improvement on the task.

In a follow-up paper too recent to be included in Table 8.3 or our calculations, Gothe et al. (2017) report on two other EF outcome measures from their study. Their results for Trail-Making follow the same pattern as above—no difference between groups on the control condition (Trails A) but noticeable improvement by the yoga group on the EF-demanding condition (Trails B) and on the difference (Trails B minus Trails A), with no improvement on either by the control group, hence significantly more improvement by the yoga practitioners. On the Flanker task, those who had done yoga improved more in their speed on all types of trials, the EF-demanding ones (incongruent trials, that tax inhibitory control) and control trial types (neutral and congruent trials), than did those who had done stretching and resistance training. Results for the Flanker Effect ([RT difference on incongruent minus congruent trials] divided by RT on congruent trials) were not reported. The positive results across all five EF measures reported in the two papers is impressive indeed. Importantly, the yoga group showed more reduced stress and anxiety according to both self-report and cortisol measures than did the control group (Gothe et al., 2016).

In a small study lasting only 4 weeks, Manjunath and Telles (2001) found that 12-year-old girls assigned to yoga (which included relaxation and awareness training in addition to exercises) improved more, and performed better at posttest, on a measure of planning and inhibitory control (the Tower of London, their only EF measure) than did girls who had been assigned to regular physical training.

A follow-up study by Telles, Singh, Bhardwaj, Kumar, and Balkrishna (2013) with almost five times as many children, both boys and girls (a bit younger; mean age of 10½ years), and lasting three times longer, found more disappointing results (see Table 8.21). On the only EF outcome measure (Stroop) there was no difference between those who did yoga (including breathing techniques, postures, guided relaxation, and chanting) and those who did standard PE. Indeed, they found no significant benefit of any kind from the yoga.

With incarcerated youth and adult women, Bilderbeck et al. (2013) found that only 10 weeks of hatha yoga once a week for 2 hours reduced stress and psychological distress and increased positive affect. Their one cognitive measure was a go/no-go task (which assesses inhibitory control). They found that those who did yoga were more accurate on go trials of the go/no-go task at posttest than no-treatment controls. Fewer commission errors (fewer incorrect presses on no-go

Table 8.21. Percentage of Measures on Which Persons Who Did Yoga Showed More Improvement and/or Better Posttest Results Than Comparison Groups on Measures of EFs, Except Reasoning/Fluid Intelligence, Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
1	Manjunath & Telles, 2001	Yoga: Physical training + relaxation + awareness training	Physical training	3	3	100%	2	3	67%	2	3	67%	2	3	67%
2	Telles et al., 2013 [A]	Yoga: Physical training + relaxation +, awareness training	Physical exercise	0	2	0%	0	2	0%	0	2	0%	0	2	0%
	Totals and Percents for school-age children who did yoga compared with a physical-activity active-control condition			3	5	60%	2	5	40%	2	5	40%	2	5	40%
	Purohit & Pradhan, 2017 [B]	Yoga	No treatment	2	3	67%	0	3	0%	0	3	0%	0	3	0%
				ADULTS (21–68 YEARS OLD) IN PRISON											
3	Bilderbeck et al., 2013 [C]	Hatha yoga	No treatment				1	3	33%	1	3	33%			
				OLDER ADULTS (OLDER THAN 55 YEARS) WITH NO CLINICAL DIAGNOSIS											
4	Gothe et al., 2014 [D]	Hatha yoga	Stretching and resistance training	5	6	83%	2	6	33%	2	6	33%	2	6	33%

(continued)

Table 8.21. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Gothe et al., 2017[B]	Hatha yoga	Stretching and resistance training	5	5	100%	5	5	100%	5	5	100%	5	5	100%
5	Oken et al., 2006[A]	Iyengar yoga	Aerobic exercise	0	3	0%	0	3	0%	0	3	0%	0	3	0%
6	Blumenthal et al., 1989	Yoga and flexibility	No treatment	0	8	0%	0	8	0%	0	8	0%	0	8	0%
7	Hariprasad et al., 2013[D,E]	Yoga	No treatment	4	7	57%	3	7	0%	3	7	43%	3	7	0%
5	Oken et al., 2006	Iyengar yoga	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Totals and Percents for older adults who did yoga compared with an active-control condition			5	9	56%	2	9	22%	2	9	22%	2	9	22%
	Totals and Percents for older adults who did yoga compared with no treatment			4	18	22%	3	18	17%	3	18	17%	3	18	17%
	Grand Totals and Percents for older adults who did yoga compared with an active-control condition or no treatment			9	27	33%	5	27	19%	5	27	19%	5	27	19%
	Grand Totals and Percents for adults (any age) who did yoga compared with no treatment			4	18	22%	4	21	19%	4	21	19%	3	18	17%

	Grand Totals and Percents for adults (any age) who did yoga compared with any active-control condition or no treatment			14	32	44%	11	35	31%	11	35	31%	10	32	31%
	Grand Totals and Percents across all ages comparing yoga to any active-control condition			13	19	68%	9	19	47%	9	19	47%	9	19	47%
	Grand Totals and Percents across all ages comparing yoga to any active-control condition (except aerobic exercise) or no treatment			12	29	41%	8	32	25%	8	32	25%	7	29	24%
	Grand Totals and Percents across all ages comparing yoga to any active-control condition or no treatment			12	32	38%	8	35	23%	8	35	23%	7	32	22%
	ADULTS 18–55 YEARS OLD WITH CLINICALLY DIAGNOSED WITH MAJOR DEPRESSIVE DISORDER (MDD)														
8	Sharma et al., 2006	Sahaj yoga + antidepressant medication	Antidepressant medication	0	3	0%	0	3	0%	0	3	0%	0	3	0%
	Grand Totals and Percents across all ages compared with any active control, medication, or no treatment			12	35	34%	8	38	21%	8	38	21%	7	35	20%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.21 (although they are mentioned in the text) but results for all other EF measures are included.

[A] One might plausibly expect EF benefits from physical exercise or aerobic exercise, so a failure to find a difference here might be due to both interventions' being beneficial, rather than practicing yoga's being ineffectual, thus we have not included the null findings here when calculating totals or percentages, except where otherwise noted.

[B] This study was published after the 2015 cutoff date. We include it here because we think it is important, but it is not included in any calculations of percentages or totals.

[C] Bilderbeck et al. (2013) did not do pretesting. Participants were incarcerated.

[D] The authors of this study did not correct for multiple comparisons. It is unclear which of their results would remain significant had they done that.

[E] The authors of this study did not conduct the needed multilevel data analysis. It is unclear how many of their results would remain significant had they done that.

trials) would have indicated better inhibitory control. Although on average those who did yoga were considerably better at not pressing on no-go trials than were controls, there was so much variability among participants that that difference was not significant. Also, it should be noted that Bilderbeck et al. administered the go/no-test only after the intervention, not before.

At the other end of the age spectrum, yoga was the control condition in the Blumenthal et al. (1989) study of 67-year-olds discussed above under aerobic exercise. Their EF findings for yoga were as disappointing as their EF findings for aerobic exercise (see Table 8.21). Yoga was the primary condition of interest in the Oken et al. (2006) study of 72-year-olds also discussed above under aerobic exercise. Here, too, their EF findings for yoga were as disappointing as their EF findings for aerobic exercise, although, as mentioned above, there may have been ceiling effects that limited the ability to find group differences, and the researchers allowed people to enter the study, including the no-treatment group, who were doing up to 30 min of exercise a day.

Yoga was also the primary condition of interest in the study by Hariprasad et al. (2013) of 75-year-olds. Hariprasad and colleagues used seven EF tasks and found more improvement on four—Backward and Forward Spatial Span, Verbal Fluency (controlled oral word association), and response time on the Stroop test—among those who did yoga than no-treatment controls, but no difference in improvement on Backward Digit Span, accuracy on the Stroop test, or Trails B, and no difference in posttest scores except on Forward Spatial Span and Verbal Fluency. The study had a high dropout rate of over 25%, and the investigators did not correct for multiple comparisons and did not do multilevel data analyses, although they had done block randomization of elderly homes. Had their data analyses taken into account their method of randomization and had they corrected for their many group comparisons on EF and non-EF measures, their four significant EF results would probably not have held up.

With adults suffering from major depression, Sharma, Pomeroy, and Baron (2006) found that Sahaj yoga in addition to antidepressant medication produced no greater benefits on any of their three EF measures compared to medication alone. These results are reminiscent of those reported by Hoffman et al. (2008), who found that clinically depressed adults who did aerobic exercise showed no greater EF benefits than those who took placebo pills.

In a study too recent to be included in Table 8.4 or our calculations, Purohit and Pradhan (2017) randomized adolescents in the Bangalore area of India who had been orphaned (and hence stressed) to a yoga program that ran for a long time with frequent sessions (3 months of 90-min sessions 4 days a week) or to a waiting list. The yoga group improved more on Stroop and Backward Digit Span than wait-list controls, although not more on Trails B. Posttest scores did not differ between groups.

There is considerable evidence that stress impairs EFs (Arnsten, 2015; Arnsten & Goldman-Rakic 1998; Arnsten, Mazure, & Sinha, 2012; Liston, McEwen, & Casey, 2009; Morgan, Doran, Steffian, Hazlett, & Southwick, 2006). Yoga reduces feelings of stress and anxiety and decreases physiological indices of stress (Li & Goldsmith, 2012; Pascoe & Bauer, 2015; Tyagi & Cohen, 2016; West, Otte, Geher, Johnson, & Mohr, 2004). Thus, it makes sense that one mechanism by which yoga might improve EFs is via reducing stress, which would be consistent with benefits found by Bilderbeck et al. (2013), Gothe et al. (2014, 2016, 2017), and Purohit and Pradhan (2017). However, there is also evidence that other forms of physical activity, including aerobic exercise, reduce stress and anxiety (Herring, O'Connor, & Dishman, 2010; Salmon, 2001; von Haaren et al., 2016), yet aerobic exercise interventions have had disappointing results for benefiting EFs. Yoga is also a mindfulness exercise and as such should train attention and cognitive inhibition. It is likely that the direct training of EFs together with reducing stress might account for EF benefits from yoga for people who are experiencing stress. Note that most studies did not select for stressed participants and did not assess stress levels before and after, and four out of the seven studies found weak evidence of EF benefits (see Table 8.21).

In their review that included 15 RCTs that Gothe and McAuley (2015) judged to have examined the effect of yoga on EFs, the authors concluded there was a moderate effect on EFs ($g = 0.27$, $p = .001$). They included studies excluded here either because participants were likely to have brain damage (e.g., patients with multiple sclerosis) or because no dependent measure qualified as an EF measure in our judgement.

Chinese Mind–Body Practices

Both methods based on Chinese mind–body practices eschew struggling or trying to control one's thoughts; instead they see this coming naturally, without much effort, once stress is reduced and one's mind and body are more relaxed and in balance. Trying to force things is seen as counterproductive. Both studies were done in China; it is unclear if such promising results from such short exposure would be found in the West, but both studies found clear evidence of EF benefits, so the methods deserve follow-up study.

Chan et al. (2013) studied Nei Yang Gong, which thought only recently developed is based on traditional Chinese Chan-based mind-body exercises (from the *Chan* tradition named *Chanwuyi* from Sanhuang monastery; Chan, 2010). Like

Tai Chi and Qijong, Nei Yang Gong involves sets of slow, smooth, gentle, and calm movements.[14] It has two primary purposes:

> First, it aims to foster self-awareness and mental self-control to help restore a calm and relaxed state. Second, it helps to reduce stress, increase flexibility of the limbs, and improve the circulation of Qi and blood. . . . Nei Yang Gong has been developed on the basis of the Chan medical model, which emphasizes the maintenance of a natural and relaxed attitude to achieve smooth circulation of Qi and blood. In this way, Nei Yang Gong differs from some of the other mind–body techniques, such as mindfulness and meditation, which require a high degree of conscious mental awareness and self-control. (Chan et al., 2013, p. 2).

The control condition was progressive muscle relaxation (PMR), a well-established behavioral technique selected because of well-replicated empirical evidence that it alleviates anxiety and physiological arousal (Lohaus & Klein-Hessling, 2003; Omizo, Loffredo, & Hammett, 1982; Singh, Rao, Prem, Sahoo, & Keshav, 2009).

Participants attended sessions where they practiced for 1 hour in 5-min chunks, twice weekly for 4 weeks (i.e., only a short time), but also practiced at home roughly 20 min a day, 4 days a week on average (according to parents' logs). Participants were children with Autism Spectrum Disorder 6 to 17 years old (mean = 11.9 years) who were grouped into closely matched pairs, with one member of each pair randomly assigned to the experimental condition and one to the control condition.

Results are summarized in Table 8.22. Children in the Nei Yang Gong group improved more and performed better at posttest on the Tower of London (which requires planning and inhibitory control) than children in the PMR group; indeed, the reduction in rule violations was four times greater in the experimental group than in the control group. On another challenging EF measure (the Color Trails test), however, there were no differences between groups. Parents rated children in the Nei Yang Gong group has having fewer temper tantrums at

[14] For the purposes of their study, five types of movements were used: "tranquil stand, shoulder relaxation, nasal bridge massage, Qi-circulating movement, and passive Dan Tian breathing. The movements were arranged in a fixed sequence and incorporated with specific pieces of music to facilitate the children's mastery of the technique and to keep them engaged. . . . To foster self-awareness and self-control, the children were also encouraged to practice some forms of Nei Yang Gong that served as self-guided massages for relaxing and calming oneself whenever they feel distressed and frustrated, e.g., rolling their hands slowly up and down between the chest and the abdomen, resting their hands on their abdomen while quietly observing their breathing. The selected Nei Yang Gong movements involved simple bodily actions (e.g., moving hands/fingers up and down, and bending the knees) and the children were only asked to perform the movements in a relaxed and natural manner" (Chan et al., 2013, p. 5).

Table 8.22. Percentage of Measures on Which Persons Who Practiced Mindfulness Practices Involving Movement (Other Than Yoga) Showed More Improvement and/or Better Posttest Results on Near-Transfer Measures of EFs, Except Reasoning/Fluid Intelligence, Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest only including measures where this was looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
1	Lakes et al., 2013[A]	Leadership Ed. thru Athletic Development (LEAD): Taekwondo with added features	Standard PE	1	2	50%	0	6	0%	0	6	0%	0	6	0%
2	Lakes & Hoyt, 2004[A, B, C]	Taekwondo martial arts	Standard PE	3	5	60%	4	5	80%	4	5	80%	3	5	60%
	Totals and Percents for school-age children who practiced Taekwondo compared to standard PE			4	7	57%	4	11	36%	4	11	36%	3	11	27%
				OLDER ADULTS (OLDER THAN 55 YEARS)											
3	Taylor-Piliae et al., 2010[D]	Tai Chi	Aerobic, resistance + flexibility training	1	2	50%									
3	Taylor-Piliae et al., 2010[D]	Tai Chi	Course on healthy aging	1	2	50%									
4	Mortimer et al., 2012[C, D]	Tai Chi	No treatment	3	5	60%									

(*continued*)

Table 8.22. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest only including measures where this was looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
5	Nguyen & Kruse, 2012	Tai Chi	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
	Totals and Percents for older adults who practiced Tai Chi compared to any active-control condition			2	4	50%									
	Totals and Percents for older adults who practiced Tai Chi compared to no treatment			4	6	67%	1	1	100%	1	1	100%	1	1	100%
	Totals and Percents for older adults who practiced Tai Chi compared to any active-control condition or no treatment			6	10	60%	1	1	100%	1	1	100%	1	1	100%
	Grand Totals and Percents across all ages comparing Taekwondo or Tai Chi to any active-control condition or no treatment			10	17	59%	5	12	42%	5	12	42%	4	12	33%
	CHILDREN (6–17 YEARS OLD) WITH AUTISM SPECTRUM DISORDER														
6	Chan et al., 2013	Nei Yang Gong (based on traditional Chinese mind–body practices)	Progressive muscle relaxation	2	2	100%	1	2	50%	1	2	50%	1	2	50%
	Totals and Percents for children who practiced mindfulness involving movement (other than yoga) compared to any active-control condition			6	10	60%	1	1	100%	1	1	100%	1	1	100%

						ADULTS (18–55 YEARS OLD)									
7	Ben-Soussan et al., 2015[D]	Quadrato Motor Training	Simple motor and verbal training	2	2	100%									
8	Venditti et al., 2015	Quadrato Motor Training	Walking	2	2	100%	2	2	0%	2	2	100%	2	2	100%
	Totals and Percents for adults of all ages who practiced mindfulness involving movement (other than yoga) compared to any active-control condition			6	8	75%	2	2	100%	2	2	100%	2	2	100%
	Grand Totals and Percents across all ages comparing mindfulness involving movement (other than yoga) to any active-control condition			12	17	71%	7	15	47%	7	15	47%	6	15	40%
	Grand Totals and Percents across all ages comparing mindfulness involving movement (other than yoga) to any active-control condition or no treatment			16	23	70%	8	16	50%	8	16	50%	7	16	44%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.22 (although they are mentioned in the text) but results for all other EF measures are included.

[A] The authors of this study did not conduct the needed multilevel data analysis. It is unclear how many of their results would remain significant had they done that.

[B] Lakes & Hoyt (2004) used participants who were 5 to 11 years old.

[C] The authors of this study did not correct for multiple comparisons. It is unclear which of their results would remain significant had they done that.

[D] This study did not report the difference between posttest scores.

posttest than control children and as having a greater reduction in the intensity of such outbursts than control children.

After a short exposure to integrative body–mind training (IBMT) of only five 20-min sessions, university students who did IBMT performed better and had improved more on the only near-transfer EF measure used (Flanker) than those who did relaxation training. Far transfer to Raven's Matrices was not significantly better than for controls, but those in the IBMT group improved significantly on Raven's, whereas the improvement of the relaxation-training group was not significant. (These results are summarized below in Table 8.23 for more sedentary mindfulness interventions.)

In a follow-up study published only in Chinese (and so not included in our tables or our tabulations) middle- and high-school students in China were trained on IBMT in 30-min sessions daily over 6 weeks (Tang, 2005, 2009). According to Tang et al. (2012) what they found was that those who did IBMT improved more in sustained and selective attention, Raven's Matrices, positive emotions, and academic test scores than an unspecified active control group.

Taekwondo

Lakes and Hoyt (2004) found that children 5 to 11 years old assigned to 16 weeks (45-min sessions 2–3 times per week) of a variant of the traditional martial art, Taekwondo, showed benefits (more improvement and better posttest scores) on all dimensions of EFs studied (e.g., cognitive [focused vs. distractible] and affective [persevere vs. quit] and emotion regulation) compared to their peers assigned to standard PE. Better posttest performance but not more improvement was also seen on the freedom from distraction subscale of the WISC-III and on the arithmetic subtest of that, although not on the digit span subtest component. These are encouraging results, but the data were not analyzed taking into account that cluster randomization was used nor correcting for multiple comparisons. It is unclear which results if any would have been significant had the data analyses reflected the method of randomization and corrected for multiple comparisons. As usual, greater benefits were seen for the children starting out with worse EFs. Greater benefits were also seen in older children than in younger ones; that should be further investigated to see if there is a lower limit to the age when Taekwondo might be practiced with the goal of improving EFs.

The martial arts program Lakes and Hoyt (2004) investigated is called Leadership Education through Athletic Development (LEAD). Developed by Pasquinilli (2001), LEAD is a program born out of the Korean Moogong Ryu martial arts tradition and incorporates not only Taekwondo, but also Hapkido and Gumdo. Pasquinilli delivered the in-school program studied by Lakes and Hoyt; it is unclear if someone else's delivering the program would produce the same impressive gains. A primary goal of LEAD (and most traditional martial arts) is self-improvement and character development. In LEAD this is achieved through iteratively evaluating one's thoughts and actions and then working to improve them. At the start of each class, students spend a few minutes sitting in meditation. They are instructed to clear their minds of thoughts and worries and to focus solely on their breathing. Deep-breathing techniques are taught and reinforced during meditation. Then each student is to ask him- or herself three questions that emphasize self-monitoring and planning: (a) Where am I? (i.e., focus on the present moment), (b) What am I doing? and (c) What should I be doing? The latter two questions direct children to select specific behaviors, compare their current behavior to their goal, and generate concrete plans to improve their behavior. The values of respect, humility, responsibility, honor, perseverance, discipline, focus, and self-control are emphasized.

Lakes and colleagues (2013) next studied a Taekwondo program (not LEAD) with seventh and eighth graders (vs. K–5 in Lakes & Hoyt, 2004), in a public school (Lakes & Hoyt had been in a private school), with larger classes (50 per class vs. 16 in Lakes & Hoyt), and over a longer period (9 months vs. 3 in Lakes & Hoyt), but with assessments on far fewer participants per group (30 per condition vs. 104 in Lakes & Hoyt). PE classes were five times a week (vs. three in Lakes & Hoyt), but martial arts was still done in only two of those PE periods. Results were distinctly less positive than in Lakes and Hoyt. On the only behavioral EF outcome measure (Hearts and Flowers), the Taekwondo group performed no better than those who did standard PE on either block of the task requiring EFs. Parents rated the behavior, although not the attention, of those who did Taekwondo as more improved than those who did standard PE.

T'ai Chi

Three studies of t'ai chi met criteria for inclusion here. The best of the three is by Taylor-Piliae et al. (2010), who randomly assigned healthy older adults (mean age = 69, range = 60–84 years) to 6 months (26 weeks) of t'ai chi, "Western exercise" (WE), or a "healthy aging" curriculum. The t'ai chi classes were taught by a t'ai chi grand master. Twelve postures were taught over the first 12 weeks and were practiced with continuous movement from one to the next, in a slow and rhythmic motion. In addition to the t'ai chi postures, participants were taught other elements of t'ai chi, including breathing, relaxation, attention to feeling, inattention to thoughts, upright and relaxed posture, and a slow and relaxed pace. WE consisted of aerobic, resistance, and flexibility training. Classes in t'ai chi and WE were twice a week for 60 min. Participants in t'ai chi were encouraged to practice on their own every day and at least three times a week. Those in WE were encouraged to practice 3 days a week (each time, at least 30 min of walking followed by 10–25 min of resistance and flexibility training). The healthy aging classes met once a week for 90 min and covered a variety of topics, such as healthy eating and medical and legal advice, and included visits to markets to learn about reading food labels and selecting produce.

The study by Taylor-Piliae et al. (2010) included two EF measures (semantic fluency and Backward Digit Span). On the latter, those who did t'ai chi improved more than those who did WE or attended classes in healthy aging. It is not possible to tell from the report if posttest scores differed between groups. At follow-up 6-months after the classes ended, the t'ai chi group not only had maintained its superiority on the Backward Digit Span but also demonstrated further improvements. (Those in the t'ai chi and WE groups had been instructed to continue to attend one class a week and do three home-based sessions each week during the 6 months between the end of the interventions and when the follow-up assessments occurred.) People seemed to enjoy the t'ai chi more than the other two conditions, because attendance was higher for the t'ai chi classes than for WE or healthy aging during the 6 months of the intervention and higher than for WE during the 6 months following that (healthy aging classes were not offered after the intervention period).

Mortimer et al. (2012) compared 40 weeks of t'ai chi (50 min per day, three times a week) to no treatment for older adults (mean age = 68, range = 60–79 years). Those who did t'ai chi improved more than their peers who did not receive an intervention on Trails B, category fluency, and a rating scale of attention, although with correction for multiple comparisons these results might not have remained significant. There were no group differences on Stroop, Backward Digit Span, or abstract verbal reasoning. A control group that met just to engage in social interaction (to control for possible social benefits of t'ai chi) improved more than the no-treatment group on only one EF measure (category fluency). They were never compared to the Tai Chi group, but the change score for Tai Chi appears to be significantly better on Trails B than for the social group.

Nguyen and Kruse (2012) compared 26 weeks of t'ai chi to no treatment for older adults (mean age = 69 years). The t'ai chi lessons were twice a week for 60 min each (of which 15 min was warm-up and 15 min was cool-down). The only EF measure was Trails B. Those who did t'ai chi showed more improvement and better posttest scores than control participants.

Wayne, Walsh, et al. (2014) conducted a meta-analysis with these three studies plus one not included here because its participants suffered from depression (Lavretsky et al., 2011) and they included a measure under EFs (Forward Digit Span) that we consider a measure of STM, not EFs. Nonetheless, they concluded that *compared to no-treatment*, the effect size of t'ai chi's benefits for EFs is a whopping 0.90 (Hedges' g) and *compared to active control groups*, its effect size is $g = 0.51$. All these studies were conducted with older adults. It would be wonderful to see t'ai chi studies with children and younger adults.

Kelly et al. (2014) included two studies of t'ai chi in their meta-analysis. Those studies were Mortimer et al. (2012) and Nguyen and Kruse (2012). They concluded that t'ai chi produced significant benefits to attention compared to no-exercise control groups and particularly impressive benefits to speed of processing, a non-EF ability. They report, however, better results for the t'ai chi participants versus controls on only five out of the 15 measures (33%) that they considered to be EF measures across the two studies.

Quadrato

Quadrato Motor Training, developed by Patrizio Paoletti, is a mindful movement activity where a person starts in one corner of a 0.5-m × 0.5-m square and is to keep moving to corners of the square in response to recorded verbal instructions calling out the number associated with the corner to move, for example, from Position 2 to 3, from 3 to 1, 1 to 4, 4 to 4 (i.e., don't move), 4 to 2, 2 to 4, 4 to 3, etc., while looking straight ahead, rather than down at the square. Young women (mean age 24 years) who did Quadrato every day for only 7 min for only 4 weeks improved more than did others who did verbal training or simple motor training on both outcome measures from the alternative uses task (the only EF task administered), which assesses cognitive flexibility (Ben-Soussan, Berkovich-Ohana, Piervincenzi, Glicksohn, & Carducci, 2015; for a review, see Ben-Soussan et al., 2015). Those who did Quadrato improved more in ideational fluency (number of uses generated) and ideational flexibility (number of different categories from which answers were generated). Whether posttest scores differed is not indicated. The same benefits were found in a second study with young women (mean age 30 years) who did Quadrato with the same dose, duration, and frequency—except here both more improvement and better posttest scores were reported for both outcomes measures of the alternative uses task (Venditti et al., 2015). This is worth following up. Quadrato is simple, takes very little time, and can be done anywhere.

Mindfulness-Based Stress Reduction (MBSR)

MBSR was developed by Kabat-Zinn (1990, 1994) beginning in the 1970s. It includes elements of sitting meditation focused on one's breath, awareness of sensations in one's body (body-scan), bringing a relaxed calm to one's mind and body, and simple yoga movements, with an emphasis on being nonjudgmental, trying to adopt a beginner's mind and stay in the present moment, and being kind to oneself and others. There is some evidence that MBSR (Kabat-Zinn, 1990, 1994) can perhaps improve selective attention but there is little evidence of other EF benefits. Six studies have looked at MBSR (see Table 8.23); however, one study looked at only a week's worth of training, another trained novices

Table 8.23. Percentage of Measures on Which Persons Who Practiced Relatively Sedentary Mindfulness Showed More Improvement and/or Better Posttest Results Than Comparison Groups on Measures of EFs, Except Reasoning/Fluid Intelligence, Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				YOUNG CHILDREN (3–6 YEARS OLD)											
1	Flook et al., 2015	Mindfulness-based kindness curriculum	Business as usual: standard curriculum (no treatment)	0	4	0%	0	4	0%	0	4	0%	0	4	0%
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
2	Napoli et al., 2005	Attention Academy program	Reading or other activities	2	3	67%	1	3	33%	1	1	100%	1	3	33%
3	Schonert-Reichl et al., 2015	MindUp	Business as usual: standard curriculum (no treatment)	6	9	67%	6	9	67%	6	9	67%	6	9	67%
	Totals and Percents for children of any age who practiced relatively sedentary mindfulness compared with no treatment			6	13	46%	6	13	46%	6	13	46%	6	13	46%
	Grand Totals and Percents for school-age children who practiced relatively sedentary mindfulness compared with any active-control condition or no treatment			8	12	67%	7	12	58%	7	10	70%	7	12	58%

	Grand Totals and Percents for children of all ages who **practiced relatively sedentary mindfulness** compared **with any active-control condition or no treatment**			8	16	50%	7	16	44%	7	14	50%	7	16	44%
				ADOLESCENTS (15–18 YEARS OLD) IN PRISON											
4	Leonard et al., 2013	"Power Source": Group-based cognitive-behavioral/ mindfulness meditation	Evidence-based cognitive-perception	0	3	0%	1	4	25%	1	4	25%	0	4	0%
				ADULTS (18–75 YEARS OLD): MISCELLANEOUS PROGRAMS											
5	Ainsworth et al., 2013	Focused attention meditation training	Relaxation training	0	1	0%	0	1	0%	0	1	0%	0	1	0%
5	Ainsworth et al., 2013	Open-monitoring meditation training	Relaxation training	0	1	0%	0	1	0%	0	1	0%	0	1	0%
6	Allen et al., 2012	1-Hour focused-attention + open-monitoring meditation & 1-hour developing fullness of feeling	Reading aloud followed by discussion	1	3	33%	0	3	0%	0	3	0%	0	3	0%
7	Jha et al., 2010	Mindfulness training similar to MBSR + additional content relevant to military deployment & stress resilience: high practice	Mindfulness training similar to MBSR + additional content relevant to military deployment & stress resilience: low practice	1	1	100%	1	1	100%	1	1	100%	1	1	100%

(continued)

Table 8.23. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
8	Jha et al., 2015[A]	Training-focused mindfulness-based mind fitness training	Didactic-focused mindfulness-based mind fitness training	0	2	0%	1	3	33%	1	3	33%	0	3	0%
9	Josefsson et al., 2014	Mindfulness meditation (notice whatever arises with a nonjudgmental accepting attitude)	Relaxation training	0	4	0%	0	4	0%	0	4	0%	0	4	0%
10	Mrazek et al., 2013	Mindfulness training course that emphasized physical posture + focused-attention meditation	Nutrition course	4	4	100%	2	4	50%	2	4	50%	2	4	50%
11	Tang et al., 2007	Integrative body-mind training (IBMT; based on traditional Chinese mind–body practices)	Western-based relaxation training	1	1	100%	1	1	100%	1	1	100%	1	1	100%

5	Ainsworth et al., 2013	Focused-attention meditation training	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
5	Ainsworth et al., 2013	Open-monitoring meditation training	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
12	Greenberg et al., 2012, 2013	Mindfulness-based cognitive therapy	No treatment	0	4	0%	2	5	40%	2	5	40%	0	4	0%
7	Jha et al., 2010	Mindfulness training similar to MBSR + additional content relevant to military deployment & stress resilience: low practice	No treatment	0	1	0%	0	1	0%	0	1	0%	0	1	0%
7	Jha et al., 2010	Mindfulness training similar to MBSR + additional content relevant to military deployment & stress resilience: High practice	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
8	Jha et al., 2015[A]	Training-focused mindfulness-based mind fitness training	No treatment	0	1	0%	2	3	67%	2	3	67%	0	3	0%

(*continued*)

Table 8.23. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
8	Jha et al., 2015[A]	Didactic-focused mindfulness-based mind fitness training	No treatment	0	1	0%	1	3	33%	1	3	33%	0	3	0%
9	Josefsson et al., 2014	Mindfulness meditation (notice whatever arises with a nonjudgmental accepting attitude)	No treatment	0	4	0%	0	4	0%	0	4	0%	0	4	0%
13	Morrison et al., 2014	Short-form mindfulness training	No treatment	1	5	20%	1	5	20%	1	5	20%	1	5	20%
	Totals and percents for adults who practiced relatively sedentary mindfulness (not including MBSR or those who received an intensive exposure to mindfulness) compared to any active-control condition			**7**	**17**	**41%**	**5**	**18**	**28%**	**5**	**18**	**28%**	**4**	**18**	**22%**
	Totals and percents for adults who practiced relatively sedentary mindfulness (not including MBSR or those who received an intensive exposure to mindfulness) compared to no treatment			**4**	**19**	**21%**	**9**	**24**	**38%**	**9**	**24**	**38%**	**4**	**23**	**17%**

	Totals and percents for adults who practiced relatively sedentary mindfulness (not including MBSR or those who received an intensive exposure to mindfulness) compared to any active-control condition or no treatment			11	36	31%	14	42	33%	14	42	33%	8	41	20%
	ADULTS (18–75 YEARS OLD): INTENSIVE EXPOSURE														
14	Chambers et al., 2008	Intensive Vispassana meditation course	No treatment	1	2	50%	0	2	0%	0	2	0%	0	2	0%
15	Heeren et al., 2009	Mindfulness-based cognition training (MBCT)	No treatment	2	5	40%	2	5	40%	2	5	40%	2	5	40%
16	MacLean et al., 2010; Sahdra et al., 2011	Intensive Shamatha meditation retreat	No treatment	1	1	100%	1	1	100%	1	1	100%	1	1	100%
17	Zanesco et al., 2013	Vipassana meditation retreat	No treatment	0	1	0%	1	1	100%	1	1	100%	0	1	0%
	Totals and Percents for those who received an intensive exposure to mindfulness compared to no treatment			4	9	44%	4	9	44%	4	9	44%	3	9	33%
	Totals and Percents for adults who practiced any relatively sedentary mindfulness (other than MBSR) compared with no treatment			8	28	29%	13	33	39%	13	33	39%	7	32	22%
	Totals and Percents for adults who practiced any relatively sedentary mindfulness (other than MBSR) compared with any active-control condition or no treatment			15	45	33%	18	51	35%	18	51	35%	11	50	22%

(*continued*)

Table 8.23. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
			ADULTS (18–75 YEARS OLD): MINDFULNESS-BASED STRESS REDUCTION (MBSR)												
18	Jensen et al., 2012	MBSR	Nonmindfulness stress reduction	1	5	20%	0	5	0%	0	5	0%	0	5	0%
19	MacCoon et al., 2014	MBSR	Health enhancement program	0	1	0%	0	1	0%	0	1	0%	0	1	0%
20	Zeidan et al., 2010	MBSR	Book listening: Tolkein's *The Hobbit*	2	3	67%	0	3	0%	0	3	0%	0	3	0%
21	Anderson et al., 2007	MBSR	No treatment	0	3	0%	0	3	0%	0	3	0%	0	3	0%
18	Jensen et al., 2012	MBSR	No treatment	2	5	40%	0	5	0%	0	5	0%	0	5	0%
22	Meland et al., 2015	MBSR	No treatment	2	4	50%	0	4	0%	0	4	0%	0	4	0%
23	Moynihan et al., 2015	MBSR	No treatment	1	2	50%	1	2	50%	1	2	50%	1	2	50%
	Totals and Percents for adults who practiced MBSR compared with any active-control condition			3	9	33%	0	9	0%	0	9	0%	0	9	0%

Totals and Percents for adults who practiced MBSR compared with no treatment	5	14	36%	1	14	7%	1	14	7%	1	14	7%
Grand Totals and Percents for adults who practiced MBSR compared with any active-control condition or no treatment	8	23	35%	1	23	4%	1	23	4%	1	23	4%
Grand Totals and Percents for adults who practiced any form of relatively sedentary mindfulness compared with any active-control condition	10	26	38%	5	27	19%	5	27	19%	4	27	15%
Grand Totals and Percents for adults who practiced any form of relatively sedentary mindfulness compared with no treatment	13	42	31%	14	47	30%	14	47	30%	8	46	17%
Grand Totals and Percents for adults who practiced relatively sedentary mindfulness compared with any active-control condition or no treatment	23	68	34%	19	74	26%	19	74	26%	12	73	16%
Grand Totals and Percents across all ages for those who practiced any form of relatively sedentary mindfulness compared with any active-control condition	12	32	38%	7	34	21%	7	32	22%	5	34	15%
Grand Totals and Percents across all ages for those who practiced any form of relatively sedentary mindfulness compared with no treatment	19	55	35%	20	60	33%	20	60	33%	14	59	24%
Grand Totals and Percents across all ages for those who practiced any form of relatively sedentary mindfulness compared with any active-control or no-treatment condition	31	87	36%	27	94	29%	27	92	29%	19	93	20%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.23 (although they are mentioned in the text) but results for all other EF measures are included.

[A] Jha et al. (2015) did not conduct the needed multilevel data analyses. It is unclear how many of their results would remain significant had they done that. They also did not report differences in the degree of improvement.

on sessions that were extremely long, and another used only one EF outcome measure and one unlikely to be sensitive to EF benefits from MBSR.

Anderson, Lau, Segal, and Bishop (2007) found no EF benefits on a CPT test (sustained attention) or the Stroop test (inhibitory control), but they had participants do MBSR only once a week (much less often than other studies, and much longer sessions than other studies: 120 min).

MacCoon, MacLean, Davidson, Saron, and Lutz (2014) found no EF benefit (compared to a health enhancement program) but only included one EF measure (CPT), and arguably selective attention is more relevant to EFs than sustained attention, which CPTs assess.

Zeidan, Johnson, Diamond, David, and Goolkasian (2010) looked at only one week of MBSR (four sessions total). They included two EF outcome measures. On neither of them did the MBSR group perform better at posttest than those who listened to a reading of *The Hobbit*, but on Verbal Fluency and on one outcome variable for the N-back test (hit rate), the MBSR group showed more improvement than controls. That the MBSR group showed more improvement in the number of correct responses in a row on N-back suggests an improvement in attention.

The possible benefits of MBSR for improving EFs are probably underestimated by the three studies above. It is not surprising that 1 week of MBSR produced little benefit (Zeidan et al., 2010). It is not that surprising that doing MBSR only once a week did not produce much EF benefit, and 2-hour sessions would be extremely long for novices (Anderson et al., 2007).

An MBSR training study by Jensen et al. (2012) suggested that daily MBSR for 8 weeks may be able to improve selective attention. Jensen's team compared 8 weeks of daily MBSR to non-mindful stress reduction, and for half the no-treatment group offered monetary incentives to improve their posttest performance. Jensen and colleagues looked at five EF outcome measures. On one measure of selective attention (the d2 Test of Attention), the MBSR group improved more than active controls and more than no-treatment controls (whether they were given monetary incentives or not). On the other selective attention measure (CombiTVA), the MBSR group improved more than both no-treatment groups, although not more than active controls. Benefits were limited to selective attention; the MBSR group did not outperform any control group in cognitive flexibility, inhibitory control, or sustained attention. Importantly, Jensen et al. assessed stress levels and found that at posttest, the MBSR group had the most reduced levels and lowest levels on physiological indices of stress and on reported perceived stress.

Moynihan et al. (2013) also looked at 8 weeks of MBSR but included no measure of attention. Their one EF outcome measure was Trail-Making. On the ratio of Trails B to Trails A, those who did MBSR improved more and performed better at posttest than no-treatment controls, although this benefit was gone 11 weeks later.

The primary benefit of MBSR is to reduce stress, and reducing stress has been shown repeatedly to improve EFs (Arnsten et al., 2012; Liston et al., 2009). Participants in the six MBSR studies cited above were mentally and physically healthy adults. We predict that when MBSR is studied with persons feeling significant stress or pain, stronger evidence that MBSR improves EFs will be found (as well as marked reductions in stress or pain, which has been demonstrated many times). Indeed, the study that found selective attention benefits (Jensen et al., 2012) is also the study that found reductions in stress. Also, EF benefits have been studied after only short courses of MBSR—only 8 weeks, except in Zeidan et al. (2014) where it was even shorter (only 1 week). There is also little evidence yet on whether (or how long) benefits last after one stops doing MBSR, or whether benefits are maintained with continued MBSR practice.

Meland et al. (2015) studied people who they assumed would be under stress because of an excessive workload (personnel at military helicopter units during a prolonged period of high workload). Their MBSR intervention lasted a long time (4 months with 3-hour sessions twice a week). As it turns out, all the personnel had been in the military for an average of 15 to 20 years and neither the MBSR group nor the no-treatment control group was very stressed at pre- or posttest. On anxiety, worry, and depression scales of 0 (not at all) to 3 (very much), the mean for both groups at both time points was under 1 (and for depression under 0.5). MBSR did, however, reduce the self-reported score on both the worry and depression scales more than shown in the control group, although the posttest scores were not significantly different between groups.

On the SART (a go/no-go sustained attention response task), Meland et al. (2015) found neither better outcomes nor better improvement in the group that went through the mindfulness training versus the no-treatment group. However, on a shortened version of the Attention Capture Task (ACT), they found more improvement in selective attention at both Distances 1 and 2, although not at Distance 3, compared to no-treatment controls. That is, across four EF attention measures (one from SART and three from ACT), they found more improvement on two (50%) and no better posttest performance on any. Our prediction of better outcomes with a longer course of mindfulness practice is not supported by this study. The jury is still out on our prediction of better outcomes with more stressed individuals, because the participants in this study did not feel particularly stressed.

Two other studies, both from Jha's lab, have also looked at possible EF benefits of mindfulness training for persons under stress, although unfortunately neither study assessed stress levels, so we do not know if the training helped to relieve stress. Jha, Stanley, Kiyonaga, Wong, and Gelfand (2010) looked at 8 weeks of mindfulness training for individuals under stress (U.S. Army Marine Corps reservists preparing for deployment to Iraq). The training was similar to MBSR but included additional content relevant to military deployment and stress resilience. Training sessions were 2 hours once a week for 8 weeks, supplemented by an 8-hour silent retreat near the end and instructions to practice 30 min daily.

Overall, participants who practiced mindfulness did not perform better on the Operation Span EF task than no-treatment controls. However, those who practiced more (≥ 400 min over 8 weeks, an average of 634 min) improved more and performed better on Operation Span at posttest than did those who got the same training but practiced less (an average of only 151 min over 8 weeks) or no-treatment controls. In particular, civilians' scores on Operation Span remained stable over the 8 weeks, but for Marines in the control group or who practiced mindfulness only a little, Operation Span scores deteriorated over the 8 weeks, presumably due to the stress of their pending war-zone deployment. On the other hand, Operation Span scores for Marines who spent more hours practicing mindfulness improved modestly. Their positive affect (enthusiasm and energy), as assessed by the PANAS scale, improved.

In a later study, Jha et al. (2015) looked at 8 weeks of mindfulness training versus no treatment for active-duty U.S. Army soldiers 8 to 10 months prior to their deployment to Afghanistan. This mindfulness intervention departed even more from MBSR and was designed specifically for individuals who had had prolonged exposure to severe stress. There were two training conditions; one involved only didactic instruction during the 2-hour classes, whereas the other involved not only instruction, but practice in the mindfulness exercises during each class. Both groups attended four training sessions (once per week in the first 4 weeks). Throughout the 8 weeks, both groups were to practice mindfulness 30 min a day on their own.

The EF outcome measure was SART (a go/no-go sustained attention response task). Those who practiced mindfulness in class and on their own were more accurate on the task at posttest than those who received verbal instruction on mindfulness in class and practiced on their own or no-treatment controls. Those who practiced in class and on their own also made fewer commission errors at posttest than no-treatment controls. Those who received only verbal instruction in class and practiced on their own showed commission error outcomes intermediate between the other two groups—neither significantly different from those who practiced in class and on their own or from no-treatment controls. Although group differences were found in accuracy and commission errors, there were no group differences in RT. The unit of randomization was units of soldiers, but the data were analyzed as if randomization had been at the level of individual participants. It does not appear that any EF group differences would have been significant had the data analysis reflected the method of randomization.

Other Mindfulness Interventions with Adults

Short Mindfulness Interventions

Mrazek, Franklin, Phillips, Baird, and Schooler (2013) looked at the benefits of a 2-week introductory mindfulness course. College students (mean age = 21 years) were randomly assigned to a class on mindfulness or on nutrition. Each met four times a week for 45 min. The mindfulness training emphasized physical posture and 10 to 20 min of focused-attention meditation. Participants were supposed to integrate mindfulness into their daily activities and to meditate for 10 min each day outside of class. The mindfulness training reduced mind-wandering on three different measures compared to active controls. It also improved EFs as indexed by the Operation Span task, although posttest scores were not significantly better than for those who studied nutrition. Performance on reading comprehension items from the GRE improved more and was better at posttest than for the control group. That improvement (in reading comprehension) and the improvement on the Operation Span task were mediated by reduced mind-wandering (i.e., better attention).

In contrast to Chinese mind–body interventions that produced more EF benefits than did relaxation training (see above), a sitting mindfulness meditation intervention that emphasized noticing whatever thoughts, sensations, or perceptions arose in a nonjudgmental and accepting way did not produce any greater EF benefits (as assessed by the Stroop task) than did relaxation training or business as usual (large test–retest effects for all groups; Josefsson, Lindwall, & Broberg, 2014). No differential outcomes on any of the diverse psychological well-being measures were found between the meditation and relaxation groups. Participants in the study were employees at local companies (mean ages per group = 49 and 50 years, respectively) randomly assigned to 4 weeks of training in sitting meditation or relaxation (two 45-min sessions per week).

Ainsworth, Eddershaw, Meron, Baldwin, and Garner (2013) compared a variant of open-monitoring meditation different from that used by Josefsson et al. (2014) to focused-attention meditation. For the former, participants were instructed to allow a sense of awareness of the breath and physical sensations to gradually expand, including sights, sounds, smells, and emotions, allowing any sense of comfort or discomfort to become part of their awareness, noticing whatever changes occurred. The latter group was instructed to find a salient sensation and keep their focus on that in as detailed and exactly pinpointed a way as possible; gently but firmly bringing their attention back whenever it wandered. The Ainsworth group found that both mindfulness conditions produced more improved and better posttest selective attention performance than was found for the no-treatment group (as indexed by RTs on incongruent Flanker trials), mirroring positive results for Chinese mind–body versus relaxation training. This benefit was specific to focused attention; alerting and orienting did not show any group difference. The authors noted that each open-monitoring meditation session began with focused attention before widening the field of attention, and they speculated that, especially since all participants were novice meditators, the open-monitoring condition may have had more aspects of focused attention than had been intended. Participants' mean age was 20 years. The interventions were extremely brief—just three 1-hour sessions, spread over 8 days, plus instructions to practice 10 min each day on their own.

Greenberg, Reiner, and Meiren (2012) looked at cognitive flexibility and Greenberg, Reiner, Meiren (2013) looked at backward inhibition (BI) with healthy young adults (mean age = 26 years) before and after 6 weeks of mindfulness-based cognitive therapy (MBCT) modified to include handling stress. There were seven 120-min sessions over the first 5 weeks, a half-day retreat in Week 6, plus instructions to do 20 min of daily practice on their own. Cognitive flexibility (reduced rigidity) was better in those who did MBCT than in wait-list controls at posttest. For BI, the MBCT group was able to increase speed without sacrificing accuracy, whereas controls markedly improved their accuracy but at the cost of a modest decrease in speed.

Allen et al. (2012) compared mindfulness training in focused-attention and open-monitoring meditation and in developing fullness of feeling to reading aloud in a group followed by discussion in young adults (mean age = 27 years) naïve to meditation. Each group met for 6 weeks, once a week for 120 min. There were no group differences in go/no-go performance or error awareness, but on the Counting Stroop task, those who went through the mindfulness training showed a greater reduction in the RT difference between congruent and incongruent trials (more improvement in inhibitory control) than those who were in the reading group.

Interventions Involving Intensive Immersion in Mindfulness

Four studies have looked at possible EF benefits from intensive immersion in mindfulness. Two were done with persons who had not meditated before:

One and a half weeks of 11 hours of mindfulness meditation each day yielded no benefit on task switching, but it improved WM more than no treatment. The meditation group started off slightly worse in WM than controls on the Backward Digit Span (mean scores of 8.0 vs. 8.35) and ended up slightly better (9.8 vs. 8.4), although the between-group differences were not significant at pre- or posttest (Chambers, Lo, & Allen, 2008).

Heeren, van Broeck, and Philippot (2009) enrolled participants with a wide age span (27–75 years, with a mean of 54 years) and looked at the benefits of 8 weeks of daily mindfulness-based cognitive therapy (MBCT). Heeren et al. found more improvement and better posttest performance on three independent measures of Verbal Fluency (semantic, phonemic, and verb fluency) and on the Haylings test (which assesses inhibitory control) compared with no-treatment controls, though they found neither better performance nor more improvement on a measure of cognitive flexibility (Trail-Making) or another measure of inhibitory control (go/no-go). If the EF benefit from mindfulness training is primarily to attention, then a benefit to cognitive inhibition (Haylings test) would be expected more than a benefit to response inhibition (go/no-go test), since selective attention and cognitive inhibition are both aspects of interference control.

All participants in the study by MacLean et al. (2010) were highly experienced meditators, who had each attended at least three 5- to 10-day mindfulness retreats over the preceding 10 years. Only 2 hours a day of Shamatha meditation over a relatively long period (a 12-week retreat; i.e., spaced rather than massed practice) resulted in better and more improved sustained attention (the only EF outcome measure used) compared with those who did not attend the retreat (MacLean et al., 2010). Thus, even for such experienced meditators, the 12-week retreat resulted in sustained attention benefits. Sahdra et al. (2011) further reported that the retreat attendees improved on response inhibition more than those who did not attend the retreat, and the response inhibition benefit was still evident 5 months later. When the control group later attended the same retreat, they too improved in response inhibition, but there was no additional control group with whom to compare their gains.

A 4-week retreat of 9.8 hours per day of Vipassana meditation did not improve inhibition (go/no-go) more than no treatment, though the meditators performed better at posttest than non-meditators (seemingly they started better and stayed better; Zanesco, King, MacLean, & Saron, 2013). No other EF measure was administered. Retreat participants had been meditating for 14 years on average, and those in the control group had been meditating on average for 10 years.

Mindfulness Interventions with Youths 16 to 18 Years Old

Leonard et al. (2013) looked at the benefits of mindfulness combined with cognitive-behavioral therapy (a program called Power Source) for individuals under stress (incarcerated adolescents). The young men (mean age = 17, range = 16–18 years old) were assigned to Power Source or an evidence-based cognitive-perception intervention for 10 sessions, 75 min each, over 3 to 5 weeks. Their only EF outcome measure was the Flanker test, and there were no group differences on that, or on the alerting or orienting components of the ANT test. However, across all three components of the ANT (Flanker and the two non-EF components), the Power Source group showed less reduction in accuracy over time than the control group (worsening EFs over time is expected in those in highly stressful situations), though no savings in RT. The authors used a group randomized design but did not analyze their data taking that into account. It is doubtful that their one significant result would have remained significant taking that into account.

Morrison, Goolsarran, Rogers, and Jha (2014) looked at the benefits of short-form mindfulness training compared to no treatment for 18-year-old university students, done once a week, for 1 hour, over 7 weeks. They used SART (a go/no-go sustained attention response task) and an Operation Span task (which assesses WM plus other EFs) to assess EF outcomes. There were no group differences on Operation Span in speed or accuracy. There was also no group difference in speed on SART. However, on the ability to correctly withhold responses to non-target items, those who practiced mindfulness improved more and were more accurate at posttest.

In-School Mindfulness Interventions With Young Children

Very promising results have been found for the MindUP program, developed by the Hawn Foundation for elementary school classrooms (Hawn Foundation, 2008). It consists of three daily sessions of 3 min of mindfulness (focusing on breathing and attentive listening to a resonant sound), twelve 45-min lessons designed to improve EFs, social-emotional understanding, and a positive mood, plus practicing those skills throughout the school day, scaffolded if necessary by the teacher. In addition, it includes instruction on the brain bases of EFs and a social-responsibility component that includes acts of kindness in school every week and working together on a community-service activity.

Fourth-graders (9 to 11 years old) who had a year of MindUP rather than the regular curriculum, which also includes a social-responsibility component, showed both better posttest performance and more improvement on all RT measures of inhibitory control used (both on the Flanker task and the Hearts and Flowers task) plus better emotion regulation (Schonert-Reichl et al., 2015). MindUP benefitted cognitive flexibility (task switching, which was assessed on the Flanker/Reverse Flanker task). These results are strong. Replication studies and examination of how long benefits last seem warranted.

Napoli, Krech, & Holley (2005) looked at a 24-week mindfulness program for children in Grades 1 to 3, which, like MBSR, included elements of sitting meditation focused on one's breath, body-scan, awareness of sensations, and simple yoga movements, encouraging students to be nonjudgmental, focus on the present moment, and adopt a beginner's mind. Sessions were 45 min, only once every 2 weeks (i.e., spaced vs. massed practice). Children randomly assigned to this improved more in selective (but not sustained) attention and decreased more in test anxiety than other children randomly assigned to reading or other quiet activities.

Flook et al. (2010, 2015) conducted the only other studies where EF outcomes of in-school mindfulness programs were investigated. The EF results were weak.

Flook and colleagues (2015) investigated a composite mindfulness and kindness preschool program for children 4 to 5 years old consisting of two 20–30 min lessons a week for 12 weeks (~10 hours total). The mindfulness practice aimed at cultivating attentional and emotional regulation; the kindness practices were aimed at empathy, gratitude, and sharing. On none of the four EF outcome measures (Flanker [accuracy and speed], delay of gratification, or card sort) were there any group differences between children who had received the program and wait-list controls.

An earlier study led by Flook (Flook et al., 2010) that evaluated a program called mindful awareness practices (MAPs) does not appear in our tables or tabulations because the only outcome measures were parent and teacher ratings. No behavioral measure of EFs was used. MAPs consisted of exercises designed to promote heightened and receptive attention to moment-by-moment experience that were done twice a week for 8 weeks by children 7 to 9 years old. Parents and teachers filled out a rating scale (the BRIEF). On none of the three indices of the BRIEF (metacognition, behavior regulation, or global executive composite) did parents or teachers rate the children assigned to MAPs as more improved than children assigned to do reading. Both parents and teachers rated the children who had done MAPs as better on inhibitory control (behavioral regulation) than children who had been assigned to silent reading instead, but they had also rated the MAPs children as better on this at pretest; there was no difference in change scores. However, the children who most needed help on EFs (the children worse on EFs at the outset) improved more from MAPs than from reading on all three indices of the BRIEF, according to both parent and teacher ratings.

Concluding Remarks Concerning EF Benefits from Mindfulness

The results presented in Table 8.22 are striking! Every study of a mindfulness activity involving movement (i.e., dynamic mindfulness, other than yoga) found either clear or suggestive evidence of EF benefits. No other program or intervention can make that claim for 100% of its studies. Looking at the percentage of EF measures on which benefits were found, again mindfulness activities involving movement (other than yoga) show the best results. However, when one looks only at near-transfer measures, Cogmed is close behind (although many of the EF measures used to evaluate Cogmed closely resembled the training tasks, whereas that is not true for mindful movement activities). On an impressive 70% of the 23 EF outcome measures, people who did a mindful movement activity (other than yoga) improved more than controls. Omitting the three studies that did not conduct the needed data analyses, this percentage becomes even more impressive. On 82% of the 11 EF outcome measures, those who participated in a mindful movement activity improved more than controls. All the activities included in Table 8.22 (Taekwondo, Tai Chi, traditional Chinese mind–body practices, and Quadrato Motor Training) deserve further study.

People who did mindful movement performed better at posttest than controls on only 50% of the 16 EF outcome measures for which data are available. These numbers change when studies with possibly spurious positive results are removed: Then the results show that those who did mindful movement performed better at posttest than controls on 80% of the five EF outcome measures for which data are available. That is better than the EF results for any other program.

Largely because of studies' failure to report whether there was a group difference in posttest performance, we rated only two studies of mindful movement as providing clear evidence. Suggestive evidence (better change or better outcome on only 50% or more of the measures) is a low bar to pass. Also, bear in mind that it is not uncommon for initial findings to look strong but then for those promising findings to not hold up in subsequent studies. No mindfulness practice involving movement in Table 8.22 has more than three studies evaluating its EF benefits.

Some studies of relatively sedentary mindfulness report promising results as well. Across all 23 studies, 57% report at least suggestive evidence of EF benefits (see Figure 8.6). That is better than most of the methods for improving EFs reviewed here (see Table 8.1). Similarly, omitting the one study with possibly spurious positive results, 55% report at least suggestive evidence of EF benefits. Across the 84 EF measures in these 22 studies, relatively sedentary mindfulness improved performance more than did the control condition on 38% of the measures. That percent is not very high, but it still puts relatively sedentary mindfulness interventions in the top half (see Tables 8.1 and 8.2).

Mak et al. (2018), who reviewed the results for mindfulness interventions with children and adolescents, found the results disappointing. Of the 13 RCTs in their review, only five (38%) found a significant effect on an EF measure. Of the 28 EF outcome variables across those studies, only 11 (39%) found a positive effect on EFs from mindfulness. We looked at almost twice as many studies and looked at results for both children and adults. Looking only at the studies of more sedentary mindfulness with children that we reviewed, 67% of three studies found at least suggestive evidence of EF benefits, and on 50% of the 16 EF measures across the three studies, improvement was better from the mindfulness training than from the control condition. More sedentary mindfulness might not be a great idea for improving EFs in young children.

Results from evaluations of relatively sedentary mindfulness practices generally do not look good for cognitive flexibility, although Greenberg et al. (2012) found better cognitive flexibility after MBCT than was shown by wait-list controls at posttest. The MBSR studies by Anderson et al. (2007) and Jensen et al. (2012) found no benefit to cognitive flexibility, and the mindfulness intervention studies of Chambers et al. (2008) and Flook et al. (2015) found none either. Schonert-Reichl et al. (2015) found better switching on the Flanker/Reverse Flanker test after MindUP. Moynihan et al. (2013) found a benefit to cognitive flexibility (i.e., switching, as indicated by results for the Trails B:Trails A ratio) from MBSR, although not to cognitive flexibility, as indicated by Trails B performance by itself, consistent with Heeren et al.'s (2009) finding of no benefit from MBCT on Trail-Making.

In contrast, the mindful-movement program Quadrato has twice been found to reduce cognitive rigidity and improve flexibility (Ben-Soussan et al., 2015; Venditti et al., 2015), though the measure of cognitive flexibility used (the alternative uses task) has never been used in studies of more sedentary mindfulness practices to our knowledge.

Mindfulness practices generally target attention or interference control, and the EF results from studies of mindfulness look best for selective attention and cognitive inhibition. The clearest example of this is the study of daily MBSR for 2 months by Jensen et al. (2012), who found strong results for selective attention on multiple measures but no benefits for cognitive flexibility, inhibitory control, or sustained attention relative to no-treatment or active controls. Heeren et al. (2009) found both more improvement and better posttest performance on a measure of cognitive interference control (Haylings test) after 2 months of MBCT every day, but no benefits to cognitive flexibility or inhibitory control on Trail-Making or go/no-go. Across two of three measures of selective attention, Meland et al. (2015) found benefits from MBSR. Ainsworth et al. (2013) found that, whether young adults were trained on open-monitoring or focused-attention meditation, they showed more improvement and better posttest performance on the Flanker test than did those who received relaxation training. Practicing mindfulness was found to help preserve performance on the Flanker test in the face of stress (Leonard et al., 2013). Mrazek et al. (2013) found reduced mind-wandering on three different measures after a 2-week introductory mindfulness course for college students, and they found that improvements in WM and reading comprehension after the 2-week course were mediated by reduced mind-wandering (i.e., better attention). Among fourth graders, Schonert-Reichl et al. (2015) found more improvement and better posttest performance on all three RT measures of selective attention in the Flanker/Reverse Flanker task. Among first to third graders, Napoli et al. (2005) found more improvement in selective attention but not sustained attention in those who practiced mindfulness versus those who did reading instead.

Benefits to selective attention have also been reported for yoga, Chinese mind–body training, and taekwondo. Gothe et al. (2017) for yoga and Tang et al. (2007) for IBMT report benefits evident on the Flanker task for yoga and for IBMT, respectively. Lakes & Hoyt (2004) report that those who were trained in taekwondo showed less distractibility at posttest than those in standard PE.

Benefits from mindfulness are clearer for selective attention than for sustained attention. Results from relatively sedentary mindfulness practices have generally been negative on CPTs (Anderson et al., 2007; MacCoon et al. 2014) and on SART (Meland et al., 2015). There are exceptions, though: MacLean et al. (2010) found benefits on a CPT. Jha et al. (2015) and Morrison et al. (2014) found benefits on SART.

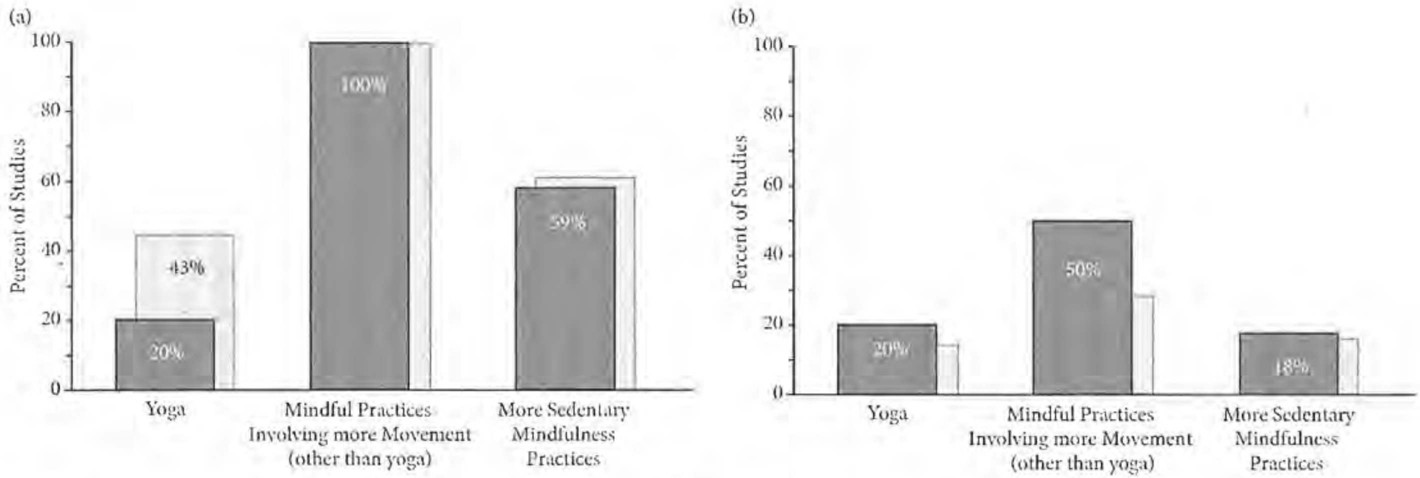

Figure 8.6. Success rates of mindfulness practices for improving EFs. Figure 8.6a: Percentage of studies finding even suggestive evidence that a mindfulness activity benefits EFs (including reasoning). Figure 8.6b: Percentage of studies finding clear evidence that a mindfulness activity benefits EFs (including reasoning). The darker bars in the foreground present the results omitting studies with possibly spurious positive results. The lighter bars in the background present the results for all studies. Studies omitted for having positive results that might not have held up were those that had not corrected for multiple comparisons or had not conducted data analyses reflecting the level at which they randomized. Whenever a study reported > 67% of measures showing positive results for improvement or posttest and did not provide any data on the other, that study is not included in calculations of strong evidence because it is possible the results of that study might have met our criteria had the results not reported been included. Given that, it is possible for the percentage of studies showing strong evidence to occasionally be higher than the percentage showing suggestive evidence, as is the case here for yoga. Figure 8.6a: Percentage of studies finding even suggestive evidence that a mindfulness activity benefits EFs, including reasoning (i.e., studies where the experimental group showed either more improvement or better posttest performance than a comparison group on ≥ 50% of the EF measures). Yoga ($N = 5$): Gothe et al.

(2014), Hariprasad et al. (2013), and Manjunath and Telles (2001). Mindful practices involving movement ($N = 5$): Ben-Soussan et al. (2015), Chan et al. (2013), Nguyen and Kruse (2012), Taylor-Piliae et al. (2010), and Venditti et al. (2015). More sedentary mindfulness practices ($N = 22$): Ainsworth et al. (2013), Chambers et al. (2008), Heeren et al. (2009), Jha et al. (2010), MacLean et al. (2010), Meland et al. (2015), Moynihan et al. (2013), Mrazek et al. (2013), Napoli et al. (2005), Sahdra et al. (2011), Schonert-Reichl et al. (2015), Tang et al. (2007), Zanesco et al. (2013), and Zeidan et al. (2010). For all studies: Yoga ($N = 7$): Gothe et al. (2014), Hariprasad et al. (2013), and Manjunath and Telles (2001). Mindful practices involving movement ($N = 8$): Ben-Soussan et al. (2015), Chan et al. (2013), Lakes et al. (2013), Lakes and Hoyt (2004), Mortimer et al. (2012), Nguyen and Kruse (2012), Taylor-Piliae et al. (2010), and Venditti et al. (2015). More sedentary mindfulness practices ($N = 23$): Ainsworth et al. (2013), Chambers et al. (2008), Heeren et al. (2009), Jha et al. (2010, 2015), MacLean et al. (2010), Meland et al. (2015), Moynihan et al. (2013), Mrazek et al. (2013), Napoli et al. (2005), Sahdra et al. (2011), Schonert-Reichl et al. (2015), Tang et al. (2007), Zanesco et al. (2013), and Zeidan et al. (2010). Figure 8.6b: Percentage of studies finding clear evidence that a mindfulness activity benefits EFs, including reasoning (i.e., studies where there was both more improvement and better posttest performance by the experimental group than by a comparison group on $\geq 67\%$ of the EF measures used). Whenever a study reported $\geq 67\%$ of measures showing positive results for improvement or post-test and did not provide any data on the other, that study is not included in calculations of strong evidence because it is possible the results of that study might have met our criteria had the results not reported been included. Given that, it is possible for the percentage of studies showing strong evidence to occasionally be higher than the percentage showing suggestive evidence, as is the case here for yoga. For studies with the needed statistical analyses: Yoga ($N = 5$): Manjunath and Telles (2001). Mindful practices involving movement ($N = 4$): Nguyen and Kruse (2012) and Venditti et al. (2015). More sedentary mindfulness practices ($N = 22$): Ainsworth et al. (2013), Jha et al. (2010), MacLean et al. (2010), Sahdra et al. (2011), and Schonert-Reichl et al. (2015). For all studies: Yoga ($N = 7$): Manjunath and Telles (2001). Mindful practices involving movement ($N = 7$): Lakes and Hoyt (2004), Nguyen and Kruse (2012), and Venditti et al. (2015). More sedentary mindfulness practices ($N = 23$): Ainsworth et al. (2013), Jha et al. (2010), MacLean et al. (2010), Sahdra et al. (2011), and Schonert-Reichl et al. (2015).

Benefits from mindfulness are clearer for interference control (inhibition at the level of attention [selective attention] and at the level of cognition) than for response inhibition (inhibition at the level of action). As mentioned above, Heeren et al. (2009) found benefits from mindfulness on the Haylings test (cognitive inhibition) but not on a go/no-go test (response inhibition). Allen et al. (2012) and Zanesco et al. (2013) also found no benefit on go/no-go.

There is evidence that one of the main problems in cognitive aging is the reduced ability to ignore or inhibit distractions (i.e., poorer interference control, including selective attention; Gazzaley, Clapp, McEvoy, Knight, & D'Esposito, 2008; Gazzaley, Cooney, Rissman, & D'Esposito, 2005; Gazzaley & D'Esposito, 2007; Hasher & Zacks, 1988). To the extent that that is the case, and to the extent that a mindfulness practice can help with that, mindfulness training might be an effective approach to help curb cognitive decline in older adults. Practicing mindfulness might aid WM because of its benefits to interference control (Jha et al., 2010; Mrazek et al., 2013).

Mindfulness practices reduce stress. They may improve EFs because they reduce stress, and perhaps to the extent that they succeed in reducing stress. Findings consistent with that hypothesis have been found for studies of yoga (Bilderbeck et al., 2013; Gothe et al., 2016; Purohit & Pradhan, 2017) and more sedentary meditation (Jha et al., 2010, 2015; Leonard et al., 2013; Napoli et al., 2005).

School Programs Intended to Benefit EFs

Some of the most encouraging findings for improving EFs come from studies of school programs (see Table 8.24).[15]

The PATHS (Promoting Alternative THinking Strategies; Kusché & Greenberg, 1994) program is an add-on to the school curriculum delivered 3 days a week for 20 to 30 min, but what is covered there is intended to be practiced throughout the school day, scaffolded if necessary by the teacher. The PATHS program promotes prosocial behavior, emotional understanding and other emotional and social competencies, self-control, and social problem-solving. Riggs, Greenberg, Kusché, and Pentz (2006) found that second and third graders who had experienced PATHS were better at posttest and more improved on both EF measures they used (Stroop and Verbal Fluency) than their peers who received the regular curriculum only. This deserves follow-up. An important caveat is that the data were analyzed as if individuals (rather than classes) had been assigned to condition; it's unclear which results, if any, would be significant with proper

[15] The MindUP school program, Attention Academy Program, and Flook's two in-school programs were discussed in the section on mindfulness.

Table 8.24. Percentage of Measures on Which Persons Trained With Various School Programs Showed More Improvement and/or Better Posttest Results on Measures of Executive Functions, Except Reasoning/Fluid Intelligence Across All Studies and Ages, Broken Down by Study

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
				YOUNG CHILDREN (3–6 YEARS OLD)											
1	Diamond et al., 2007[A]	Tools of the Mind	Another curriculum newly developed by the school district				1	4	25%	1	4	25%			
2	Domitrovich et al., 2007[B]	Preschool PATHS	Business as usual: standard curriculum	0	4	0%	0	4	0%	0	4	0%	0	4	0%
3	Flook et al., 2015	Composite mindfulness and kindness curriculum	Business as usual: standard curriculum	0	4	0%	0	4	0%	0	4	0%	0	4	0%
4	Lillard & Else-Quest, 2006[A]	Montessori	Business as usual: standard curriculum				2	3	67%	2	3	67%			
5	Raver et al., 2008, 2011	Chicago School Readiness Project	Business as usual: standard curriculum	2	3	67%	2	3	67%	2	3	67%	2	3	67%
	Solomon et al., 2017[C]	Tools of the Mind	Playing to Learn: play-based school program	1	1	100%									

(*continued*)

Table 8.24. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Totals and Percents for young children trained using various school programs (excluding ones involving mindfulness included in Table 8.24) compared with no treatment			2	7	29%	4	10	40%	4	10	40%	2	7	29%
	Totals and Percents for young children trained using any school program compared with no treatment			2	11	18%	4	14	29%	4	14	29%	2	11	18%
	Grand Totals and Percents for young children trained using a school program (excluding ones involving mindfulness included in Table 8.24) compared with any active-control condition or no treatment			2	7	29%	5	14	36%	5	14	36%	2	7	29%
	Grand Totals and Percents for young children trained using any school program compared with any active-control condition or no treatment			2	11	18%	5	18	28%	5	18	28%	2	11	18%
				CHILDREN (5–7 YEARS OLD)											
6	Napoli et al., 2005	Attention Academy program	Reading or other activities	2	3	67%	1	3	33%	1	1	100%	1	3	33%
7	Blair & Raver, 2014[D]	Tools of the Mind: all children	Business as usual: standard curriculum	1	3	33%	1	6	17%	1	6	17%	1	6	17%
7	Blair & Raver, 2014[D]	Tools of the Mind: lower SES	Business as usual: standard curriculum	3	3	100%	3	3	100%	3	3	100%	3	3	100%

	Dias & Seabra, 2016[C,D]	Intervention Programme for Self-Regulation and Executive Functions (PIAFEx)	Business as usual: standard curriculum	4	7	57%									
8	Traverso et al., 2015[A,D]	Play-based approach to training EFs: Helping story characters overcome challenges	Business as usual: standard curriculum	7	14	50%	1	14	7%	1	1	100%	1	14	7%
	Totals and Percents for children trained using a school program compared with no treatment			11	20	55%	5	23	22%	5	10	50%	5	23	22%
	Totals and Percents for children trained using a school program compared with an active-control condition or no treatment			13	23	57%	6	26	23%	6	11	55%	6	26	23%
				SCHOOL-AGE CHILDREN (7–15 YEARS OLD)											
9	Caviola et al., 2009	Metacognitive WM training using a strategy game	General cognitive activities	0	3	25%	0	3	0%	0	3	0%	0	3	0%
10	Garcia-Madruga et al., 2013	Embedded EF training in reading comprehension lessons	Business as usual: standard curriculum	0	1	0%	0	1	0%	0	1	0%	0	1	0%
4	Lillard & Else-Quest, 2006[A]	Montessori curriculum: older children	Business as usual: standard curriculum				1	2	50%	1	1	100%			
11	Riggs et al., 2006[B]	PATHS	Business as usual: standard curriculum	2	2	100%	2	2	100%	2	2	100%	2	2	100%

(continued)

Table 8.24. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
12	Schonert-Reichl et al., 2015	MindUp	Business as usual: standard curriculum	6	9	67%	6	9	67%	6	9	67%	6	9	67%
	Totals and Percents for school-age children trained using a school program (excluding ones involving mindfulness) compared with no treatment			2	3	67%	3	5	60%	3	4	75%	2	3	67%
	Totals and Percents for school-age children trained on any school program compared with no treatment			8	12	67%	9	14	64%	9	13	69%	8	12	67%
	Totals and Percents for school-age children trained using a school program (excluding ones involving mindfulness) compared with any active-control condition or no treatment			2	6	33%	3	8	38%	3	7	43%	2	6	33%
	Totals and Percents for school-age children trained on any school program compared with any active-control condition or no treatment			8	15	53%	9	17	53%	9	16	56%	8	15	53%
	Totals and Percents for children of all ages trained using a school program (excluding ones involving mindfulness) compared with any active-control condition			0	3	0%	1	7	14%	1	7	14%	0	3	0%
	Totals and Percents for children of all ages trained on any school program compared with any active-control condition			2	6	33%	2	10	20%	2	8	25%	1	6	17%

	Totals and Percents for children of all ages trained using a school program (excluding ones involving mindfulness) compared with no treatment			15	30	50%	12	38	32%	12	24	50%	9	33	27%
	Totals and Percents for children of all ages trained on any school program compared with no treatment			21	47	45%	18	55	33%	18	41	44%	15	50	30%
	Totals and Percents for children of all ages trained using a school program (excluding ones involving mindfulness) compared with any active control condition or no treatment			15	33	45%	13	45	29%	13	31	42%	9	36	25%
	Totals and Percents for children of all ages trained on any school program compared with any active control condition or no treatment			23	49	47%	20	61	33%	20	45	44%	16	52	31%
	SCHOOL-AGE CHILDREN (7–15 YEARS OLD) WITH ADHD														
13	Menezes et al., 2015[D]	Intervention Programme for Self-Regulation and Executive Functions (PIAFEx)	Social skills intervention	1	3	33%	3	18	17%	3	18	17%	1	18	6%
	Grand Totals and Percents for all children, with or without ADHD, trained using a school program (excluding ones involving mindfulness) compared with any active-control condition			1	6	17%	4	25	16%	4	25	16%	1	21	5%
	Grand Totals and Percents for all children, with or without ADHD, trained using any school program compared with any active-control condition			3	9	33%	5	28	18%	5	26	19%	2	24	8%

(*continued*)

Table 8.24. Continued

Study				Significantly Better Improvement			Significantly Better Posttest			Significantly Better Posttest Only Including Measures Where This Was Looked at			Both Significantly Better Change and Posttest		
Study #	Study Name	Condition of Interest	Comparison Condition	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.	# Sign.	# of Measures	% Sign.
	Grand Totals and Percents across all ages trained using a school program (excluding ones involving mindfulness) compared with any active-control condition or no treatment			16	36	44%	16	63	25%	16	49	33%	10	54	19%
	Grand Totals and Percents across all ages trained using any school program compared with any active control condition or no treatment			24	52	46%	23	79	29%	23	63	37%	17	70	24%

Note. Results for reasoning/fluid intelligence (R/FL) are not included in Table 8.24 (although they are mentioned in the text) but results for all other EF measures are included.

[A] This study did not collect pretest data.

[B] The author of this study did not conduct the needed multilevel data analysis. It is unclear how many of their results would remain significant had they done that.

[C] This study was published after the 2015 cutoff date. It is included here because it is important, but it is not included in any calculations of percentages or totals.

[D] The authors of this study did not include a correction for multiple comparisons. It is unclear which of their results would remain significant if they had.

multilevel data analyses. PATHS delivered to Head Start preschoolers 1 day a week produces marked social and emotional benefits but has yet to demonstrate EF benefits (Domitrovich, Cortes, & Greenberg, 2007).

The Chicago School Readiness Project (CSRP) was an add-on for Head Start preschool classrooms. It emphasized developing verbally skilled strategies for emotion regulation. Stress-reduction workshops were conducted for teachers all year. Children with the worst externalizing behavior received one-on-one counseling. CSRP is more focused on behavior management than PATHS but otherwise had similar goals (see Table 8.1 in Diamond & Lee, 2011). Four-year-olds who experienced CSRP in Head Start improved more and performed better at posttest on two of the three measures of inhibitory control used (Tapping and Balance Beam composite, and experimenters' ratings of impulse control) than their peers in regular Head Start classes (Raver et al., 2008, 2011). The CSRP children didn't show a benefit in delaying gratification, but only one study we've reviewed has found improvement on the delay of gratification paradigm from any program. CSRP children also improved more in vocabulary, letter naming, and math than did controls. CSRP's improvement of academic skills was mediated largely via its improvement of EFs. EFs in the Spring of preschool predicted achievement 3 years later in both math and reading (Li-Grining et al., 2011). These results, too, are most encouraging.

Results from three independent evaluations have been published on the Tools of the Mind (Tools) curriculum for preschool and kindergarten. Tools (Bodrova & Leong, 2007) is based on the work of Vygotsky (1978) and is a full curriculum. It emphasizes improving EFs (especially self-control), social and emotional skills, and building a sense of community as much as academics. Diamond et al. (2007) found better inhibitory control on the Reverse Flanker task in 5-year-old children who had been through Tools than in their peers who had been in another new curriculum of which the district was quite proud. There were no significant group differences, however, on the standard Flanker task or on Blocks 1 or 2 of the Hearts and Flowers task (all of which had ceiling effects) or on Block 3 of Hearts and Flowers (which suffered from a floor effect). Whether children were in Tools or not accounted for more variance in EFs than did age or gender. The children were not evaluated before the intervention, so it is possible that children in Tools of the Mind had better inhibitory control to begin with, though the groups were closely matched on a great many demographic variables.

Blair and Raver (2014) found better and more improved emotion regulation on the dot-probe task, but they did not find better or more improved inhibitory control and cognitive flexibility on the Hearts and Flowers task, card sorting, or the Flanker task (which included reverse and mixed Flanker trials), in kindergarten children who received Tools versus the regular school curriculum.[16] Benefits to reading, math, and vocabulary were even larger the following year (in Grade 1) compared with controls. Effects were about eight times larger in low-income schools (see Figures 8.3a and 8.3b). Low-income children who had experienced Tools were better and more improved on fluid intelligence/reasoning (Raven's Matrices) than their peers who had received the standard curriculum, but this was not significant for more economically advantaged children.

In another study of Tools, published after our cutoff date and so not included in our tables or analyses (Solomon et al., 2018), a daycare-based Tools program for 3- to 4-year-olds was compared to a high-quality, existing play-based program. Children whose parents had rated them as highly hyperactive and/or inattentive at the outset of the year showed greater gains on an inhibitory control task ("Touch your toes when I say touch your head" and "Touch your head when I say touch your toes") than children in the existing program. The authors concluded that "Tools may be advantageous in classrooms with children experiencing greater challenges with self-regulation, at no apparent cost to those less challenged in this regard" (p. 2). In sum, the results for Tools are encouraging and deserve more longitudinal follow-up.

Other work not included in our tables or analyses because the first report was published in Portuguese (Dias & Seabra, 2013) and a later one was published after our 2015 cutoff date (Dias & Seabra, 2016) also deserves mention. Dias and Seabra developed an Intervention Programme for Self-Regulation and Executive Functions (PIAFEx) for schoolchildren in Brazil that intentionally borrows some principles from Tools. In their study, which is included in our tables and analyses, Menezes et al. (2015) looked at whether this program could help 7- to 13-year-old children (mean age = 10 years) with ADHD when delivered twice a week for 1 hour over 35 weeks. They found a benefit on Stroop and a verbal WM test compared to no-treatment controls, but no benefits on a visuospatial WM Test, Trails B, the Wisconsin Card Sort, Verbal Fluency, or other measures, and it is not clear if the benefits they found would remain significant if corrections for multiple comparisons had been done.

Those disappointing results are consistent with other studies of Tools itself as an add-on to existing curricula. Leong and Bodrova originally tried Tools as an add-on, with Tools activities done for roughly an hour a day. Benefits were narrow and specific to the context in which the skills were practiced. Clements et al. (2012) replicated the limited benefits from Tools as an add-on. Diamond et al. (2007), Blair and Raver (2014), and Diamond, Lee, Senften, Lam, and Abbott (accepted) replicated the marked benefits from Tools as

[16] Blair and Raver (2014) conducted multiple comparisons without correcting for that in their significance testing. Not all their positive findings might still be significant had they done that.

an all-day curriculum originally reported more informally by Bodrova and Leong (2001).

In a study with first graders (mean age = 6 years), Dias and Seabra (2016) looked at PIAFEx implemented as the school curriculum (all day, 5 days a week, for 15 weeks). Compared to those in regular first-grade classes, children in PIAFEx improved more on some measures of inhibitory control (Trails B and the Simon task) and cognitive flexibility (Trails B and errors on a cancellation attention task [CAT] that required switching attention) but not on other measures of inhibitory control (Stroop and go/no-go) or cognitive flexibility (score on the CAT). Information is not provided on whether there was any group difference in posttest scores and no correction for multiple comparisons was made.

Two other play-based school programs have been investigated. Traverso, Viterbori, and Usai (2015) looked at EF benefits from a program where children were asked to help two story characters overcome various challenges. The program was short (only 1 month and only three 30-min sessions per week). Those who participated in the program showed more improvement in, and better posttest performance on, RT on the Flanker test than those who did not participate. There were no group differences in accuracy. Children in the program also improved more on other measures of inhibitory control (delaying gratification and circle drawing) as well as measures of WM (backward word span and Keep Track span), attention (matching familiar figures), and cognitive flexibility (accuracy in the mixed block of the Hearts and Flowers test, although there were no differences in RT for that block). That is quite impressive. No differences were found on another delay of gratification measure or go/no-go. Often when people improve on a measure, they improve on either speed or accuracy, so we would say that this program helped children perform better on most measures, including the Flanker and Hearts and Flowers tests, but not on go/no-go. Enthusiasm needs to be tempered a bit, however, because Traverso et al. conducted multiple comparisons without correcting for that in their significance testing.

The other play-based program involved a story protagonist who had certain things to do (Caviola, Mammarella, Cornoldi, & Lucangeli, 2009). The children (fourth graders) and teacher discussed what strategy to use to achieve the protagonist's objective. This program too was very brief (only 1 month and only two 50-min sessions per week). The only EF outcome measures were three WM tests, and no benefits compared to business-as-usual controls were found on any of them.

Garcia-Madruga et al. (2013) investigated EF and reading benefits from embedding training in EFs (WM, attention, inhibition, and switching) within work on reading comprehension. This was done with 8- to 9-year-olds and also for only a very short time (only 1 month; 50-min sessions three times per week). On neither WM (reading span) nor reasoning was any benefit found relative to no-treatment controls, but a benefit to reading comprehension was found.

Lillard and Else-Quest (2006) looked at benefits from Montessori education but included very few EF outcome measures and did not collect any preintervention data. Parents of all children in the study had wanted their children to attend public Montessori instruction; choice of who got in was made by lottery. Children who attended Montessori showed more cognitive flexibility than children in the standard curriculum both at 6 years of age on card sorting and at 12 years on a creativity measure. There was no difference in delay of gratification at age 6.

Of the 13 school programs (including those studied by Flook et al., 2015, Napoli, 2005, and Schonert-Reichl et al., 2015), 54% found at least suggestive evidence of EF benefits. The following school programs show promising evidence of EF benefits: Attention Academy, CSRP, MindUP, Montessori, PATHS, and Tools. Of the seven studies investigating those promising school programs, 75% found at least suggestive evidence of EF benefits (Table 8.1). When the two studies whose positive findings might not have held up if the requisite statistical analyses were conducted are omitted, the results show 67% of studies of the promising school programs found at least suggestive evidence of EF benefits (Table 8.2). Only mindful movement practices have found better results than promising school programs. In Table 8.1, this difference is small; promising school programs show the best results on two of the metrics and show second best on the other two. When studies with potentially spurious results are omitted, however, promising school programs drop to third place on two metrics and second on the other two—they are still second only to mindfulness practices involving movement but now by a wider margin. These school programs share the goals of promoting social and emotional outcomes as well as academic excellence and minimizing stress. These promising school programs deserve further study.

EF Outcomes From Other Programs

Results for EF benefits are summarized here for Experience Corps, theater, piano instruction, and learning digital photography or quilting, and El Sistema orchestra.

Experience Corps (Glass et al., 2004) is a program that brings older adults into schools with the goals of improving the mental and physical health of the senior volunteers who participate and helping students feel more at home and less alienated in school, as well as improving their academic outcomes. Older African American women who participated improved more on selective attention (Flanker) than their peers who did not participate. Posttest scores are not given. No other EF outcome measure was used (Carlson et al., 2009). We'd like to see this promising program receive more study.

Park et al. (2014) conducted an extremely careful, very well-designed study with disappointing results. Older adults were randomly assigned to spend 14 weeks learning digital photography, quilting, or both, or to either of two controls conditions: participating in structured activities with others in a social club or alone. No differential benefit to any EF skill was found (not on Flanker, Stockings of Cambridge, or WM) or to fluid intelligence (Raven's). Episodic, recognition, and recall memory improved in those who did photography but not in the quilters.

Bugos, Perlstein, McCrae, Brophy, & Bedenbaugh (2007) used random assignment to examine the potential of 6 months of individualized piano instruction for mitigating age-related cognitive decline in healthy adults 60 to 85 years old. They found more improvement on one EF measure (Trials B) than in no-treatment controls and that benefit was still maintained 3 months later. On their other EF measure (a composite of Forward Digit Span [an STM measure] and Backward Digit Span), though, they found no group difference.

Noice, Noice, and Staines (2004) reported that older adults who were randomly assigned to theater (training in acting; seven 90-min sessions over the course of a month) improved more and were better at posttest in problem-solving (the Means-End Problem-Solving Procedure by Platt & Spivack, 1975) than their peers assigned to either visual arts or no treatment. On the other EF measure, the Listening Span task, those who received acting training also improved more than the other groups but this just missed being statistically significant (p = .056). Participants assigned to visual arts appreciation did not differ from those assigned to no treatment in improvement or posttest on either EF measure. Those who did theater improved more in feelings of psychological well-being and reported more well-being at posttest than either control group (visual arts or no treatment), although there were no differences in self-esteem. The gains by those who had trained in theater persisted undiminished for 4 months after the training ended.

There are many reasons why theater might be an excellent avenue for improving EFs. One factor might be its positive impact on emotional and social factors, such as Noice et al. (2004) found for psychological well-being in their study. Noice and Noice (2006) offer other possible factors, such as practice in processing material at a deep level (so that it is remembered better) and practice in staying in the present moment (inhibiting attentional or cognitive interference):

> Actors . . . determine the goal of every utterance of the character, breaking down scripts into what they call "beats" (the smallest goal-directed chunks of dialogue). . . . A link is forged between almost every word or phrase and the goal that caused the character to utter it (Noice & Noice, 1997, 2004). A consistent

finding in the text-comprehension literature is that goal statements are better recalled than nongoal statements (e.g., Trabasso & van den Broek, 1985). Processing the script at such depth produces a great deal of verbatim retention without rote memorization. . . .

During rehearsal, they try to devote all their conscious awareness to remaining in the present moment by attending to the other actors, only glancing down at the script when necessary. . . .

[In a subsequent study (Noice & Noice, 2004)] we specifically told [participants] not to try to remember the words but to put all their concentration on meaning them (i.e., actively using them to gain a specific end such as warning a friend). . . . Meaning the words produced greater retention than memorizing them did. . . . This finding has been replicated repeatedly using different populations and procedures and various types of materials. (Noice & Noice, 2006, p. 15)

Further study of the potential EF benefits of theater with children, young adults, and elders would be most welcome, especially when training in generalizing the cognitive skills learned in theater to other contexts is provided.

An excellent study of El Sistema (Holochwost et al., 2017) came out too late to be included in Table 8.4 or our calculations, but it deserves mention. El Sistema is an orchestral music program developed by Jose Antonio Abreu in Venezuela to rescue poor children through music (Booth & Tunstall, 2016). It emphasizes playing together in ensemble from the start, the joy of making music, not embarrassing anyone over a mistake, building community, learning to work together and learning from one another (child teaching children), and demanding daily practice and training. A predominantly African American parochial school in Philadelphia offers El Sistema and decides who gets in by lottery. The parents of all children in this study wanted their children to get El Sistema; half got randomly selected for it. None of the 265 children in the study were classified as special education and most were lower income. El Sistema meets for 120 min every school day (39 weeks/year). Forty minutes of that is instruction in a small-group setting and 40 min is rehearsal in an ensemble. The drop-out rate from El Sistema over the 3 years of the study was very low (only 10%). Holochwost and colleagues used an intent-to-treat data analysis, which is the most conservative and most rigorous. Testers were blind to condition.

Holochwost and colleagues found standardized test scores, academic grades in English and math, and performance on seven out of nine (78%) EF measures improved more (and were better at posttest) for children in El Sistema than for children in the control group (see Table 8.25.) Effect sizes on the Flanker and Stroop tests were quite large (0.5 or greater). Some effects were not evident, however, until children had been in El Sistema for 3 years.

Table 8.25. Results in the Holochwost et al. (2017) Study of El Sistema

Variable	Difference*	Significance	Effect Size
Standardized test scores (Terra Nova)	13.5 points higher	$p < .007$	$d = 0.24$
Grades in English	2.5 points higher	$p < .001$	$d = 0.30$
Grades in Math	3.9 points higher	$p < .01$	$d = 0.42$
EF Measures:			
1) Go/No-go: better accuracy	3.5% higher	$p < .004$	$d = 0.40$
2) Flanker Test: better accuracy	6.6% higher	$p < .01$	$d = 0.35$
more efficient (based on RT and percent correct)	12.3 better	$p < .001$	$d = 0.50$
smaller Flanker effect (RT difference on incongruent & congruent trials)		$p < .001$	$d = 0.50$
3) Stroop Test: more efficient (based on RT and percent correct)		$p < .001$	$d = 0.50$
faster RT	313 msec faster	$p < .0001$	$d = 0.57$
(for the bottom 25%)	620 msec faster		$d = 1.13$
4) Wisconsin Card Sorting Test: fewer perserverative errors		$p < .02$	$d = 0.18$
5) Tower of London	NS (too difficult)		
6) Trail-Making Test	NS (too insensitive)		

*Those in the program for 3 years vs. controls.

Across All Approaches to Improving EFs, Which Are the *Most* Promising Thus Far?

The approach that has been most successful thus far at improving EFs is **mindfulness practices involving movement** (such as t'ai chi, taekwondo, Chinese mind–body, and Quadrato). Every single study that has investigated whether training in a mindful movement practice can improve EFs has found at least suggestive evidence that it can. That is not true of any other approach we have examined. The EF results for mindfulness practices involving movement are far better than those for other movement activities without a mindfulness component and better than those for mindfulness practices primarily done seated, although that is not true if the results for yoga are included with the other mindful movement practices. The superiority of the results for mindful movement practices is especially evident when studies are omitted that had positive results that might not have held up had they corrected for multiple comparisons or had not taken into account when analyzing their data that they had randomized at the group level (Table 8.2). Here, mindful movement practices show the best EF outcomes of all approaches on all four of our indices. The difference between the percentage of studies showing at least suggestive evidence of EF benefits for mindful movement practices and the approach with the second-best results is a whopping 33%. When looking at the percentage of studies showing clear evidence of EF benefits, a 10% difference between the results for mindfulness practices involving movement and the next-most-successful approach is found. These results for mindful movement should be followed up.

More studies of the mindfulness practices involving movement that have already been studied are needed, with more EF outcome measures and more longitudinal follow-up. We would also encourage research of possible EF benefits from other mindful movement practices, such as aikido, judo, qigong, or the Niroga program (Frank et al., 2012; Frank, Kohler, Peal, & Bose, 2017). We are perplexed that the results have been so mixed for yoga, with two studies finding outstanding EF results (Gothe et al., 2014, 2016, 2017; Manjunath & Telles, 2001) but the other six studies finding less. Research exploring the possible reasons for this or better identifying the conditions under which yoga is most beneficial for EFs would be most welcome.

The second most successful approach for improving EFs is **promising school programs**. They show consistently better results than any cognitive training approach (computerized or noncomputerized) for improving EFs across all indices we used (see Tables 8.1 and 8.2).

The best results for improving inhibitory control from any approach have been found for school programs with children in preschool through Grade 4 (MindUP, PATHS, Tools of the Mind, and CSRP). To our knowledge, no work has been published of a school program that tried to improve inhibitory control in children beyond Grade 4. That school programs have been the most successful of all approaches at improving inhibitory control matters because inhibitory control seems to be the EF most predictive of long-term outcomes (Miller et al., 2011; Moffitt et al., 2011).

Expectations of individuals who deliver the programs and interventions are likewise important. Although teachers in the control group for the Tools school program study by Diamond et al. (2007) were as enthusiastic and optimistic about the prospects of the new program they were delivering as were the Tools teachers, for many other studies of noncomputerized approaches, the possibility exists that it was the expectations of teachers or trainers that drove the results rather than the program itself. It is critical to have a control condition for which there is great excitement and for which expectations are as high as for the experimental condition.

Programs that are part of a public school's curriculum have several critical advantages. They can reach the most children, in the most economical way, and in the fairest way, in the sense that all schoolchildren can be reached (not just the privileged few who can afford to pay for outside programs). School programs are also able to provide greater doses, frequency, and duration than most other interventions or approaches to improving EFs. That is especially true when EF training is embedded in activities throughout the school day (as is done in Tools and Montessori and to some extent in PATHS and CSRP). Also, school programs can train diverse EFs under very diverse circumstances. Training diverse EFs makes it more likely that more EFs will improve, since transfer is narrow and people generally only improve on what they train on. Training under diverse circumstances makes transfer to other contexts, especially novel ones, more likely. The results suggest that the combination of a lot of training and practice under diverse circumstances appears to be particularly effective. We hope school systems and others will take note of this.

The third most successful approach at improving EFs is **noncomputerized cognitive training** (see Tables 8.1 and 8.2). It falls in the top half of all approaches for improving EFs on all four indices in Tables 8.1 and 8.2. Across all studies of noncomputerized cognitive training, 67% report at least suggestive evidence of EF benefits. EF outcomes from noncomputerized cognitive training are better than those for any type of computerized cognitive training. The higher levels of in-person interaction may account for the encouraging results in comparison to most computerized approaches, especially if the teacher or trainer is supportive and has great confidence that the trainee will succeed.

While for school programs those administering the outcome measures have generally been blind as to who received the intervention, for noncomputerized cognitive training, the norm has unfortunately been the reverse: Those administering the assessment measures have generally not been blind to who was in which condition—notable exceptions being the ACTIVE study (Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006), Cheng et al. (2012), and Mackey et al. (2011). Without blinding, it is possible for tester expectations to affect the results.

Outstanding results for WM have been found in two studies from a group that used noncomputerized complex-span training (Borella et al., 2010; Carretti et al., 2013). It will be interesting to see if these results hold up in other studies by other groups, especially since it is surprising to see such good results from such a minimal amount of training (only three 1-hour sessions over 2 weeks).

The fourth most-successful approach for improving EFs is **Cogmed**. It (like the approaches ranked first, second, and third) was ranked in the top half of all approaches for improving EFs on all four indices in both Table 8.1 and 8.2. No other approaches can claim this. No other computerized cognitive training approach shows results as good as those for Cogmed. No other computerized cognitive training approach ranks in the top half of all approaches. Cogmed certainly succeeds in improving the aspects of WM it trains. It is the only method of computerized training to consistently show sustained near-transfer benefits. Benefits to WM from Cogmed have been shown to last for 3 to 6 months and even for 1 year. It may also improve other aspects of WM and attention. More studies are needed to see if Cogmed improves WM and perhaps attention in school situations and in other arenas of life. Selective attention (indeed, interference control in general of both internal and external distractions) is closely tied to WM. We would not be surprised if WM training improved interference control, including selective attention, but convincing evidence in support of that does not presently exist. Cogmed is marketed as beneficial to children with ADHD, yet its generalization to ADHD symptomatology has not been confirmed by blinded observers. Ideally, WM and attention in the real world should be assessed in objective ways; no one who administers or scores the measures should be aware of which children are in the experimental group and which are not.

WM training, whether using Cogmed or N-back tasks, may be a promising approach for older adults beginning to suffer from selective WM deficits. We recommend more study of that. Age-related cognitive decline is often specifically in WM (Hedden & Park, 2001; Park & Payer, 2000; Wang et al., 2011). The one study that tried Cogmed training with older adults (mean age of 64; Brehmer et al., 2012) found those who trained on Cogmed showed more improvement on all four (100%) of their EF near-transfer measures and on the Cognitive Failures Questionnaire than controls who did nonincrementing Cogmed; all improvements were still evident 3 months later. Older adults (mean age of 68) who trained on N-back tasks in the study by Stepankova et al. (2014) improved more and performed better at posttest than no-treatment controls on both of their WM measures (Letter-Number Sequencing and Forward + Backward Digit Span task), visuospatial processing (block design), and visuospatial reasoning (matrix design). Li et al. (2008) found only very narrow transfer in their N-back training study that included older adults (mean age of 74), but their only other outcome measures were complex-span tasks, which one might expect would be insensitive to N-back training. Older adults might well comprise a population in whom computerized WM training could be especially beneficial.

Karbach and Verhaeghen (2014) similarly concluded from their meta-analysis that WM training might be highly effective for older adults with WM decline. A word of caution is warranted here, however, in that the Cogmed study with older adults (Brehmer et al., 2012) and one of the N-back studies with older adults (Stepankova et al., 2014) conducted multiple comparisons without correcting for that in their significance testing. Their results might not look so rosy had they done that.

Do not give up on older folks. EFs can be improved even in those more than 70 years old. Sink et al. (2015) found more EF improvement from their physical-activity training program in those 80 or older than in those younger. Williams and Lord (1997) found EF benefits from enriched aerobic exercise among participants whose mean age was 72. Noncomputerized reasoning and problem-solving training that included real-world tasks improved the reasoning and problem-solving of seniors whose mean age was 74 years, and those benefits were still evident 1, 2, and even 5 years later (the ACTIVE study: Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006). In Karbach and Verhaeghen's (2014) meta-analysis, prolonged practice with computerized WM training showed gains as large for older adults as those for younger adults.

Across all the approaches reviewed here (except aerobic exercise interventions), generally more weeks has produced better results than fewer weeks, within the range of durations studied. Cogmed training has generally been more successful at improving EFs than N-back training. One reason for that might be that the duration of Cogmed training is usually longer (5–8 weeks, vs. 2–5 weeks for N-back). Similarly, Basak et al. (2008) found that 5 weeks of training using Rise of Nations produced better EF benefits than 2½ weeks. Of three mindfulness retreats, the one that lasted longest (13 weeks vs. 1.5 or 4 weeks) and had the most spaced practice (2 hours a day vs. 10 or 11 hours per day) produced the best EF results (MacLean et al., 2010, vs. Chambers et al., 2008, & Zanesco et al., 2013).

There are exceptions, however. For example, two of the studies with the best EF outcomes (Tang et al., 2007, which used IBMT, and Green et al., 2012, which used Cogmed) lasted only 1 week and 4weeks, respectively. Perhaps studies of aerobic exercise interventions have not found better EF outcomes from longer interventions because most of the interventions (whether more or less successful) have generally lasted far longer than cognitive interventions (for both plain and enriched aerobic exercise programs, the more successful ones lasted on average 16 and 17 weeks, respectively, whereas less successful ones lasted on average 27 and 20 weeks, respectively).

In general, better results have been found with training sessions that lasted 30 min or more than with shorter sessions, though the results for Quadrato Motor Training with only 7-min sessions is a marked exception (Ben-Soussan et al., 2015). Cogmed sessions have generally lasted 30 to 45 min, whereas N-back sessions have generally been shorter (lasting only 15–30 min). Perhaps that is one reason why EF outcomes have generally been better for Cogmed than N-back training. Cogmed has yielded better results with children 7 to 14 years old than with children 4 to 5 years old (the former practiced 30–45 min at a time, the latter only 15 min). Mawjee et al. (2014, 2015) found, however, that, at least for adults, the benefits from 45 min of Cogmed a day were no greater than the benefits from 15 min per day. Davis et al. (2007, 2011) found better EF outcomes from 40-min sessions of enriched aerobic exercise than from 20-min sessions. The benefits from Tools as an add-on to existing curricula are markedly less than when the Tools training in EFs is embedded in all activities throughout the school day (Blair & Raver, 2014; Bodrova & Leong, 2007; Clements et al., 2012; Diamond et al., 2007; Miller et al., 2014).

Perhaps studies of aerobic exercise interventions have generally not found better results from longer sessions because *all* sessions (whether for the more or less successful programs) have lasted more than 30 min, and even the aerobic portion has, in general, (both for the more and less successful programs) been over 30 min (see Table 8.7). On average, sessions in more successful plain and enriched aerobic exercise programs lasted 46 and 56 min, respectively, whereas sessions for plain and enriched aerobic exercise programs less successful at improving EFs were a little longer (57 and 64 min, respectively).[17] Perhaps a session length of about 45 to 55 min is better than one of more than 55 min; at some point, sessions might get too long and produce diminishing returns. The aerobic portion of sessions across more and less successful plain and enriched aerobic exercise interventions has varied from a mean of 35 min (more successful plain aerobic exercise programs) to a mean of 48 min (less successful enriched aerobic exercise programs). Perhaps once a threshold of 30 to 40 min for the aerobic portion is reached, there are no further EF benefits, or even diminishing returns, from going longer than that. Similarly, a study with very long sessions of MBSR (2 hours) found no EF benefits from that (Anderson et al., 2007); the sessions were likely too long (especially for novices).

Most studies have focused on training WM. There is some evidence that training attention or reasoning might produce better results. Two of four studies of attention training (50%; one noncomputerized: Semrud-Clikeman et al., 1999; one computerized: Wass, Porayska-Pomsta, & Johnson, 2011) report at least suggestive evidence of EF benefits. Five of the seven studies of reasoning training (71%; three noncomputerized ones: the ACTIVE study [Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006]; Blieszner et al., 1981; Plemons et al., 1978; one computerized: Corbett et al., 2015; one of both computerized and noncomputerized training: Mackey et al., 2011) found at least suggestive evidence of improved reasoning.

[17] FITKids was an outlier here, lasting 120 minutes. Without FITKids, the mean duration of sessions of less successful enriched aerobic programs reviewed here was 57 minutes.

In the ACTIVE study, the better reasoning of those trained on reasoning versus no-treatment controls remained true 1, 2, and even 5 years later (and participants here were older adults). The probably reason why the fourth study of reasoning training (Cheng et al., 2012) did not find suggestive evidence of benefits is because so many participants did not complete the training. Those who completed at least 80% of the training showed better and more improved reasoning 6 months later than those who had trained less, and by 1 year after training, that difference was even greater.

Both studies of exergames (Maillot et al., 2012; Staiano et al., 2012) found suggestive evidence of EF benefits. That merits further investigation.

We would also like to point out the success in improving EF outcomes of targeted training that involves real-world activities—the ACTIVE study (Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006; where training included not only laboratory tasks but also real-world activities such as food preparation and managing a budget), Experience Corps (Carlson et al., 2009), El Sistema music (Holochwost et al., 2017), theater (Noice et al., 2004), and the tantalizing results from three recent studies of sports (Alesi et al., 2014, 2016; Ishihara et al., 2017; Koutsandréou et al., 2016). These, too, merit further investigation.

Across All Approaches, Which Have Been Least Successful Thus Far in Improving EFs?

EF results have been worse for **resistance training** than any other method for improving EFs reviewed here. Resistance training comes in last on three of the four indices for assessing program efficacy in improving EFs in both Tables 8.1 and 8.2. It falls in the bottom half on all four indices in both tables. No study of resistance training found strong evidence of EF benefits; only 22% found even suggestive evidence. Across 30 EF measures investigated across nine studies, resistance training failed to produce better posttest EF performance than the comparison condition on 93% of the measures investigated. A caveat is that resistance training was the active control condition in four of the nine studies reviewed here; in those four studies, investigators had hoped not to find EF benefits for resistance training.

The next-worst results for EF benefits come from studies of aerobic exercise interventions without explicit EF challenges or motor skill demands (**plain aerobic exercise**), like brisk walking or running. That, too, falls in the bottom half on all four metrics in both Tables 8.1 and 8.2.

The third-worst results for broad EF benefits (near and far transfer, including reasoning) come from studies of **computerized complex-span** training.

Although several computerized cognitive training approaches claim to aid ADHD, there is a lack of objective evidence that such training improves ADHD symptoms or academic performance, although unblinded, subjective parent ratings often indicate benefits. Our conclusion here is consistent with those of others (Cortese et al., 2015; Rapport et al., 2013; Sonuga-Barke et al., 2013). For example, Cortese et al. concluded that, "For trials implementing working memory training . . . effects on ADHD were negligible even considering most proximal measures. This suggests that this form of training, which has been widely promoted for use with patients with ADHD, has little or no efficacy for core ADHD symptoms. . . . Crucially, there was . . . no evidence that these effects generalized to important areas of everyday functioning, which themselves are influenced by working memory ability, such as reading and arithmetic" (pp.171–172).

When potentially spurious positive results are excluded, EF benefits from **yoga** are quite disappointing (Table 8.2). A few studies have found outstanding EF results from yoga, but most studies have not. It is unclear why there is such a stark discrepancy across studies. Perhaps the critical difference is how the yoga was presented (were the mindful, spiritual aspects front and center, or was it the physical exercise component?) and/or characteristics of the instructor.

Our prediction that aerobic exercise that trains and challenges EF skills (**enriched aerobic exercise**) would improve EFs more than plain aerobic exercise was supported, but the EF results for enriched aerobic exercise still fall among the bottom half of all approaches investigated. The EF results for enriched aerobic exercise are better than for plain aerobic exercise on all four of our indices, although two of the differences are slight (see Tables 8.1 and 8.2). Thus, EF outcomes for enriched aerobic exercise are better than for plain aerobic exercise, but the results are still relatively poor compared with other methods for improving EFs.

The newer studies of sports (Alesi et al., 2016; Ishihara et al., 2017; Koutsandréou et al., 2016) provide more encouraging results than studies of enriched aerobic exercise that only included sports elements. We think participating in a sport is a more promising approach; this is discussed further below in the section "Our Predictions About How to Most Effectively Improve EFs."

Results for far transfer are generally poor, regardless of the method of training—and all results for resistance training and plain aerobic exercise are far transfer—consistent with the generally disappointing EF benefits from those activities. Of the four Cogmed studies that looked at far transfer to reasoning/fluid intelligence, only one study (25%) found it; Klingberg et al. (2005) found both more improvement and better final test scores. Across all six studies of complex-span training (computerized and noncomputerized), only one study (Borella et al., 2010) found far-transfer benefits to reasoning/fluid intelligence, although all six studies looked. Across all 11 studies of N-back training that looked for benefits to reasoning/fluid intelligence, only four reported even suggestive evidence of that (36%). Three (27%) reported clear evidence (Jaeggi et al., 2010; Rudebeck et al., 2012; Stepankova et al., 2014). Of the five N-back studies with only a no-treatment control group, four (80%) reported at least suggestive evidence of far transfer to reasoning/fluid intelligence, but only one study out of seven (14%) with an active control group found that.

One might say that it is unfair to compare cognitive benefits from cognitive training to those from physical-activity training. Yet, if aerobic exercise and resistance training are promoted specifically as ways to improve EFs, then there should be evidence that they do that. The results for EF benefits are pretty poor for resistance training and plain aerobic exercise even if no comparison to other training methods is made (e.g., not even suggestive evidence of EF benefits in 78% of resistance-training studies and in 67%–69% of aerobic exercise studies; see Tables 8.1 and 8.2). Also, resistance training and aerobic exercise interventions last far longer than do cognitive training programs. For example, the length of Cogmed training is generally 5 to 8 weeks, while the length of complex-span or N-back training is only 2 to 5 weeks. On the other hand, the average length of resistance-training interventions has been 26 weeks, of plain aerobic exercise, 23 weeks, and of enriched aerobic exercise, 19 weeks—on average over 400% longer than cognitive training interventions.

It is highly likely that a great many studies reviewed here were underpowered to find significant EF effects and many did not choose their outcome measures well. On the other hand, surely some studies that failed to find significant EF benefits were never published. That is particularly likely for studies of WM or attention training, since an EF benefit would have been the primary focus of such studies. For only roughly 50% of the approaches reviewed here have at least half the studies reported at least suggestive evidence of EF benefits (see Tables 8.1 and 8.2).

Limitations of the Present Systematic Review and a Call to Others to Analyze the Extant Literature in Ways Other Than We Have

There is no one right way to analyze results across studies. We encourage others to use the rich information provided here on each study in the text and in Tables 8.3 and 8.4 to try different ways to make sense of the results across studies.

For example, others might choose to exclude studies with only one EF outcome measure. Should a study with only one EF outcome measure be said to provide clear or strong evidence, even if the study found more improvement and better posttest scores on that one measure? Should a study with only one EF outcome measure that found no benefits on that be considered as providing negative evidence when, if more EF measures had been included, some EF benefits might have been found?

What about studies that choose several insensitive outcome measures or several outcome measures insensitive to the kind of approach they were evaluating? Should those studies be considered as providing weak evidence or should the results for their weak measures be discounted? Should outcome measures carry more or less weight depending on their difficulty? We encourage others to come up with alternative perspectives on what constitutes strong or clear evidence and/or what constitutes at least suggestive evidence.

Certainly, studies should be required to report results for all their outcome variables. To the extent that some studies have not done that, it diminishes the validity of conclusions that can be drawn from the literature. The same goes for publishing reports of negative results; valid conclusions cannot be drawn if studies with positive results continue to be published more than those with negative findings.

The same cognitive task is often administered differently or analyzed differently by different researchers, complicating conclusions that can be drawn from the literature. For example, the critical Stroop condition can be a single-task condition (say the color of the ink of color words) or a mixed-task condition (say the color of the ink of color words except when the word is in a box, then read the word) and the dependent measure can be the percentage of correct responses on incongruent trials in the critical block, or on all trials in that block, or the difference in accuracy on that block and an easier block. The proportion of incongruent trials in the critical Stroop block (trials where a color word appears in the ink of another color) can vary from 100% to 33%. Similarly, the proportion of no-go trials in a go/no-go task can vary from 50% to 20% across studies. While Trail-Making is usually administered the same way by all, some researchers use performance on Trails B as their EF measure, whereas others use performance on Trails B minus performance on Trails A. The number of trials administered can also vary widely across studies for tasks that are called by the same name.

The "same" program or intervention can be administered differently by different individuals. Too rarely have studies checked or reported fidelity in implementing an intervention, and almost never has consistency across different individuals nominally implementing the same program been checked.

For example, Zeidan et al. (2010) found no EF benefits from MBSR but had participants do it for only 1 week (four sessions total), far shorter than other studies. Anderson et al. (2007) also found no EF benefits from MBSR, but had participants do it only once a week, much less often than other studies, and in far longer sessions than other studies (2 hours). Is it fair to count those studies when evaluating the efficacy of MBSR for improving EFs? The same question applies to a third MBSR study. MacCoon et al. (2014) found no EF benefit from MBSR training but included only one EF measure (CPT) and that assesses sustained attention. Arguably it is selective attention rather than sustained attention that one would predict MBSR might improve. This is an example of using only one EF outcome measure and likely not a sensitive one.

We feel strongly that it is important to show both more improvement and better posttest outcomes than controls. Others may feel it suffices to show better improvement alone.

Others may want to exclude outcome measures that bear a close similarity to the cognitive tasks on which participants were trained.

Some may disagree with what we consider to be EF tests or tests of other cognitive abilities. For example, we have categorized mental rotation tasks as assessing spatial ability rather than EFs, and we have characterized cancellation measures (except those with particularly demanding selective attention demands, such as the d2 Test of Attention) as assessing speed of processing rather than EFs; others might want to categorize these as EF measures.

Similarly, some may disagree with what EF ability we consider a test to be assessing. For example, there is considerable debate about what the Stroop test assesses; we consider it a measure of inhibition. Most people consider N-back tasks to be WM measures, but because the presence of lures puts demands on inhibitory control, we consider N-back tasks to be measures of WM + inhibitory control.

Although we noted where studies did not correct for multiple comparisons or did not analyze their data in accord with group randomization although they had randomized at the level of group (not individuals), we still recorded their findings as they reported them. Others might want to take a more stringent approach toward those studies by omitting them, by asking the authors to re-analyze their data, or by adjusting significance levels or re-analyzing the data themselves. For improvements that reflect the experimental group simply catching up to the control group, we counted those improvements but indicated our skepticism about whether they really reflect genuine benefits from the experimental condition. Others might discount those improvements altogether.

It was very difficult for us to extract whether the experimental group had truly improved more than the control group or whether the experimental group truly performed better at posttest than the control group for a great many studies. It is certainly possible, indeed likely, that despite our best efforts, we have made mistakes in interpreting reported results. Far too few studies reported effect sizes. Some reported just means and standard deviations without giving p values for between-group comparisons. A more enterprising individual might contact the study authors and ask if they might be willing to provide effect sizes and any other missing data analyses, or at least to provide enough information for effect sizes to be calculated.

Certainly, a shortcoming of our review is that we counted each significant result equally without taking effect size into account. We counted the number of significant effects without taking into account the size of any because for a number of studies, we could not determine effect sizes. We strongly encourage others to make a determination of whether strong or clear evidence exists for a program by taking into account the size of the effects.

We included all studies meeting our criteria that we could find regardless of how old they were. Others might want to exclude studies published before a certain date. We excluded studies with fewer than eight participants per group; someone else might want a higher, perhaps much higher, cutoff. One might want to exclude studies where the intervention occurred for fewer than x number of sessions or x number of weeks (our only exclusionary criterion relevant to this was to exclude studies that looked at only a single instance or session). Others might choose to exclude studies where the attrition rate was too high, compliance was too low, evidence was lacking for even the most proximal benefits from the intervention (i.e., no improvement on exactly what was trained, which was exceedingly rare), or where raters or testers were not blind to which condition subjects had participated in.

We did not include in our calculations studies published after 2015, although we noted some such studies when the delay in publication of this volume allowed more time. Conclusions might change with the addition of newer studies. We included three studies that had not done pretesting; others might choose to exclude them. We included some studies that had not used random assignment; others might choose to exclude those.

Others might want to exclude all studies that included only a no-treatment or business-as-usual control group because that provides only a very low bar to pass. Note, however, that for Cogmed, miscellaneous other cognitive training, plain aerobic exercise, and yoga, stronger results for the condition of interest were found when it was compared to an active control condition than when it was compared to no treatment (see Tables 8.9, 8.16, 8.18 and 8.21).

We encourage others to look at the data differently from the way we have and to see how their conclusions confirm or differ from ours. We do not mean our systematic review to be the final say, but simply one credible way to look at the evidence available through 2015.

We offer a caution, though, about the seeming mathematical precision of meta-analyses, except for studies of Cogmed or N-back training. The interventions are simply too diverse; their methods, content, dose, frequency, and duration are too different; and their outcomes measures are too different from one study to another for a meta-analysis to be meaningful. Only for Cogmed and N-back is there sufficient comparability across a number of studies. While several studies of Cogmed and of N-back training have looked at EF improvements using quite similar trainings and similar outcome measures, the same cannot be said for any other approach to improving EFs. There have been several studies of aerobic exercise, resistance training, yoga, and more sedentary mindfulness; however, few have looked at benefits of the same activities and few have used the same outcome measures. Most approaches to improving EFs (except Cogmed and N-back) have had only one or two studies investigating them.

A Call to Researchers to Consider Additional Analyses of Their Data

It might be worthwhile for studies to analyze their results separately for participants initially most behind on EFs, since everything suggests that those individuals are likely to show the largest benefits from the experimental condition. It might also be worthwhile to analyze results separately for those who attended a large percentage (perhaps ≥ 90%) of the sessions for the experimental condition or who showed the most direct benefit from the experimental condition (whether that is most improvement on the cognitive tasks on which they trained or most improvement on fitness or skill measures directly linked to the physical activity on which they trained). Studies might want to analyze results with all participants and a second time excluding participants who attended only a few sessions of the experimental condition. It might be worthwhile to assess mood and/or feelings of efficacy, pride, and/or self-confidence before and after, and do an analysis of the EF results once including only those whose mood or self-confidence improved, since we predict those are the individuals most likely to show the largest benefits from the experimental condition.

There has been much debate about what makes an adequate control condition and what makes an optimal control condition. As Simons et al. (2016, p. 116) wrote, "Just because a control group is active does not mean it is adequate." Time actually spent in the control condition should be comparable to that for the experimental condition. Expectation of benefits should ideally be as high for the control condition as for the experimental one, since we know that expectations can play a large role in any effect (Boot et al., 2013; Rosenthal & Jacobsen, 1968). Since expectations for, and excitement about, something new are usually high, in part simply because it is new, ideally the control condition should be something new.

Klingberg had reasoned that an excellent control for Cogmed would be the same Cogmed games, just without difficulty increasing. Many have criticized this control condition as being too unlikely that participants would expect similar benefits and too boring (potentially affecting participants' motivation to play the training games, the amount of time they spent playing them, and the length of time they remained in the study). Klingberg and his colleagues, to their credit, have tried to collect empirical data on this. They have evidence that participants have not reported feeling significantly more bored by the nonincrementing version of Cogmed than by regular Cogmed (e.g., Bergman Nutley et al., 2011) nor have they dropped out at higher rates. Nevertheless, Klingberg and colleagues have little evidence that participants found either version to be of passionate interest or that participants felt the nonincrementing version to be as exciting or deeply engaging as the regular version.

Mackey et al. (2011) had one group train on reasoning and another group train on speed of processing. These were equally interesting to participants, but the two abilities are not independent. A smaller difference might have been found between groups than if one of the conditions had been more unrelated to EFs, such as recall or recognition memory or perceptual discrimination. Any new skill will require EFs initially to acquire the skill, and that can potentially reduce between-group differences in outcomes.

Matching experimental and control conditions extremely well, where efforts are made to vary only one variable—as Schmidt et al. (2015) attempted to do when they had a high physical demand and high cognitive demand condition and a high physical demand and low cognitive demand condition—may match conditions so well that it is difficult to find significantly stronger benefits from the condition of interest.

We are impressed by the use of visual search (cognitively demanding but not requiring EFs) as the control condition for N-back training by Redick et al. (2013), single-task training as the control condition in many studies of task-switching training (Dörrenbächer et al., 2014; Karbach & Kray, 2009; Kray et al., 2012; Pereg et al., 2013), visual-search training as two control conditons for complex-span training (Harrison et al., 2013), visual-perception training as a control condition for attention training (Tucha et al., 2011), watching children's videos or TV clips and images as the control condition for training young children on computer games requiring inhibition and for training infants on visual attention (Rueda et al., 2005, 2012; Wass et al., 2011), stretching and toning as the control condition for aerobic exercise or resistance training (see Tables 8.18–8.20), and sedentary activities, such as painting or other visual arts or board games, as control conditions for aerobic exercise (Fabre et al., 2002; Krafft, Pierce, et al., 2014; Krafft, Schaeffer, et al., 2014; Krafft, Schwarz, et al., 2014) or theater (Noice et al., 2004).

As the control conditions for learning digital photography or quilting, Park et al. (2014) included one control condition that had similar social group interactions (a social club with common structured activities) and one that specifically omitted that (structured activities done alone that relied on existing knowledge or not empirically shown to improve cognition). That was an admirable study design indeed! The control condition in one of our studies was also good because it pitted two newly introduced curricula against one another (Tools of the Mind and a new curriculum the district itself had developed, for which the district had very high hopes; Diamond et al., 2007).

Finally, we need to be assessing the outcomes we really care about, i.e., the ability to use EFs in real life. Objective, real-world measures of EFs are desperately needed. Right now, by and large, the choice is between objective but arbitrary laboratory tests or subjective questionnaires asking about use of EFs in real-life situations. Perhaps virtual reality technology will provide ways to objectively assess the ability to use EFs in real-life situations.

A Call to Researchers to Study Factors Affecting How Long Benefits Last

Does it matter which method is used to try to sustain benefits (e.g., booster sessions or embedding challenges to EFs in daily activities)? If refresher or booster sessions are used, at what intervals should they be given, and for what duration? Do the answers to these questions differ by type of program, EF component (e.g., WM or response inhibition), the age, gender, or cultural group of the participants, or other variables?

There is a desperate need for more studies that look at benefits months and years after an intervention has ended. How long do benefits last? What affects how long, or if, benefits last? Little is known about whether the length of time that benefits last differs by any characteristic of the participants or mentors or by type of program or activity. What do participants do after an intervention ends—do they continue doing that activity on their own? Do they find other ways to challenge the EFs that were challenged during their training? For school programs, do teachers and programs in subsequent years reinforce the EF-enhancing aspects of a program that produced EF benefits?

No one has looked at whether EF benefits last from any form of physical activity (plain aerobic exercise, resistance training, enhanced aerobic exercise, yoga, martial arts, or anything else) except Taylor-Piliae et al. (2010) and Oswald, Gunzelmann, Rupprecht, and Hagen (2006). Longitudinal follow-up should be done for the most promising physical-activity approaches.

Two studies report benefits still evident 5 years later (Oswald et al., 2006, for balance, coordination, and flexibility training + cognitive training sessions, and less so for cognitive training alone; [18] and the ACTIVE study: Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006, for noncomputerized reasoning training that included some real-world situations).

There were age differences in the longevity of benefits from the Schmiedek et al. (2014) study of intensive computerized training in WM and speed of processing. Younger adults still showed benefits in episodic memory and reasoning/ fluid intelligence (though not WM) 2 years later. For older adults, no benefits were evident 2 years later.

Academic benefits from CSRP were still evident 3 years later and were mediated by improved EFs (Li-Grining et al., 2011). The benefit to reading from Tools that had not been significant at the end of kindergarten was significant in the Fall of Grade 1 (Blair & Raver, 2014). The benefit to vocabulary was sustained and expanded to include all children who had received Tools, not just those in high-poverty schools. The effect on mathematics was somewhat reduced by Grade 1. Notably, the benefits to reading, vocabulary, and math relative to controls in Grade 1 were present despite controlling for both pre- and posttest kindergarten results, indicating that benefits continued to accrue over and above those seen at the end of kindergarten. EF benefits from school programs in the early years need replication studies and should try to follow children for several years.

WM benefits from Cogmed have been found 6 months later (Bigorra et al., 2015; Holmes et al., 2009; van der Donk et al., 2015) and 1 year, but not 2 years, later (Roberts et al., 2016). Far-transfer benefits from Cogmed were no longer present 3 months later in Klingberg et al. (2005) but were even larger 6 months later in Bigorra et al. and were present for the first time 6 months later in Holmes

[18] Among older adults, 75 to 93 years old (mean age = 80) at the study's start, Oswald et al. (2006) found that 5 years later those in the no-treatment, psychoeducational, physical activity, or combined psychoeducation + physical activity conditions showed declines on the study's cognitive measures. Those who had received combined cognitive and physical training or cognitive training alone showed significant cognitive preservation that was still evident 5 years later. The scores for the combined cognitive and physical training group were the highest of any group 5 years later on reasoning (WAIS similarities), several memory tasks, and speed of processing. (The term *highest scores* here means showing the least decline.) The physical training in the Oswald et al. (2006) study involved no resistance training and little aerobic exercise. It concentrated instead on balance, eye–hand coordination, motor coordination, and flexibility, including movements from gymnastics, dance, and yoga, although it also included playing tennis and table tennis. The cognitive training included practice on visual-search tasks, a maze task, and a Stroop word-color task (with an emphasis on speed), and lots of memory tasks (e.g., remembering phone numbers, shopping lists, and names) where memory strategies were taught. The psychoeducational intervention involved lectures, group discussions, exercises, and role play on everyday problems (e.g., avoiding falls, dealing with the death of a loved one, nutrition, and understanding prescription labels). Trainings were administered to small groups of 15 to 20 persons every week or two for a total of 30 sessions. The cognitive and psychoeducational sessions were 90 min; physical exercise was 45 min; the combined cognitive training plus physical training was 90 + 45 min (135 min) as was the combined psychoeducation and physical training. (This study was not included in our tables of calculations because pre- to post-test change scores are only reported averaged across multiple EF and non-EF domains. We mention it here because for the 5-year follow-up results, they report outcomes for individual measures, including measures of EFs.)

et al. Parental ratings on the BRIEF in the Bigorra et al. study did not significantly differ between the Cogmed and control group right after training, but 6 months later the parents of those who had done Cogmed saw more improvements than the parents of controls. Teachers saw some benefits right away, but they saw more and larger benefits 6 months later compared with their ratings of children in the control group. Holmes et al. found a benefit to mathematical reasoning 6 months after Cogmed that had not been evident immediately after Cogmed training. Roberts et al. reported only on math performance 2 years after training, but they found that those who had trained on Cogmed performed worse in math 2 years later than others who had received regular classroom instruction rather than CogMed.

Benefits from N-back training have been shown to last 2 to 5 months for N-back performance itself but not for other EF measures (Pugin et al., 2014). Benefits for reasoning from reasoning training have been found even 5 years later (Ball et al., 2002; Willis et al., 2006). Benefits from 100 sessions of processing speed and WM training were not present for WM 2 years later, but benefits to episodic memory and fluid intelligence (present immediately after training for younger, but not older, adults) were still evident 2 years later (Schmiedek et al., 2014). Benefits from complex-span training were not still evident 18 months later (Dahlin, Nyberg, et al., 2008). Benefits to inhibitory control from BrainGame Brian were still evident 3 months later, though the benefit to visuospatial WM relative to controls was slightly reduced 3 months later (Dovis et al., 2015). Benefits from a 3-month Shamatha mindfulness retreat were still evident 5 months later (MacLean et al., 2010).

The ACTIVE study and Taylor-Piliae et al. (2010) are the only studies reviewed here that looked at whether continued practice or booster sessions could help after an intervention had ended. Taylor-Piliae et al. asked participants to continue doing what they had been assigned to (t'ai chi or aerobic exercise + resistance training & flexibility training)—one class a week and three sessions at home per week—during the 6 months between when the programs ended and follow-up assessment. They found that at 6-month follow-up assessment, the t'ai chi group had not only maintained its superiority in WM but had improved even more in WM. The ACTIVE study (Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006) found that booster sessions seemed to help preserve the reasoning gains longer from their training. We know very little about when to give booster sessions or what the best durations or frequencies might be.

As already mentioned, it is unrealistic to expect benefits, much less lasting ones, if participants do not attend training sessions. Attendance (compliance) should be monitored and reported. As also mentioned, it is unrealistic to expect benefits to last indefinitely if one does not continue using and challenging the trained skills and continue doing the kind of activity that led to the improvement in the first place. No study of EF benefits has looked at whether or how long participants continue to do the activity they were trained on or continue to challenge the EF skills on which they were trained. Presumably using and challenging the skills needs to be embedded in one's regular routine, or at least periodic booster or refresher sessions should be offered. Research is sorely needed on whether these assumptions are correct.

What About Training People in Strategies to Minimize the Need for EFs, so That People Do Not Have to Expend So Much Effort Trying to Exercise EFs?

Most EF tasks assess the ability to exercise EFs, but on Mischel's delay-of-gratification task (Mischel, Ayduk, & Mendoza-Denton, 2003), the children who succeed usually do so by finding ways to minimize the EF demands (e.g., by looking away or finding something else to do so they are not so tempted to eat the treat). Much of Baumeister's work, too, finds better self-control outcomes in adults who find ways to reduce the need for self-control is not taxed as much (Baumeister & Alquist, 2009; Muraven & Baumeister, 2000).

It makes good sense to avoid expending effort needlessly. Thus, in addition to helping people improve their EFs, also teaching them how to minimize the demands on their EFs (e.g., by writing themselves notes, thereby reducing WM demands, or by placing unhealthy foods out of sight, so that less willpower is required to avoid eating them) might end up being one of the best ways for people to achieve optimal outcomes.

It would be of great interest to see training studies move beyond *only* trying to improve EFs to start trying to help people be more efficient in their deployment of EFs, learning ways to minimize or circumvent the need for EFs wherever possible. Why expend all the effort to exercise EFs when you could achieve the same excellent result without using EFs, thus saving your finite energy resources for when you really need them?

What About Looking at the EF Benefits of Being Outside in Nature?

There has been very little study of the benefits for EFs of being in nature. This might be well worth looking into. Perhaps the EF benefits from brisk walking have generally been better than for any other form of plain aerobic exercise or resistance training because walking was more often done outdoors than other types of exercise in those studies. Some mindful movement activities, such as t'ai chi, are usually done outdoors, but others like taekwondo or Quadrato are not.

One study found that children with ADHD concentrated better after walking in a park (Faber Taylor & Kuo, 2009). Indeed, the effect sizes were so impressive that the authors suggested that "doses of nature" might serve as a safe and inexpensive way to manage ADHD symptoms. Other researchers have fatigued the attention of participants and then had them spend 40 min walking in the natural environment, walking in an urban environment, or listening to soft music and relaxing (Hartig, Mang, & Evans, 1991). They found that those who walked in nature performed better on proofreading (requiring focused concentration) than those in the other two conditions. In a follow-up study, Hartig, Evans, Jamner, Davis, and Gärling (2003) found that a walk in a nature reserve improved performance on an attention test, reduced stress, and increased positive feelings.

Pesce, Masci, et al. (2016) conducted an RCT with 460 children 5 to 10 years old that contrasted 6 months of weekly physical education games with more cognitive and motor skill demands with traditional PE. Children in the enriched PE exhibited more improvement in inhibitory control than children in traditional PE. Importantly, time playing outdoors seemed to be critical to this effect. Only when the training in ball skills was paralleled by a medium-to-high level of outdoor play was this effect evident. The authors concluded, "Outdoor play appears to offer the natural ground for the stimulation by designed PA games to take root in children's mind" (Pesce, Masci, et al., 2016, p. 1).

Another recent study found greater psychological and health benefits from physical activity done outside in nature than from the same activities done inside (Calogiuri et al., 2015). Atchley, Strayer, and Atchley (2012) report an impressive 50% improvement in EFs after participants had taken part in a 4- to 6-day wilderness hiking trip. Kaplan (1995) and Atchley et al. (2012) have theorized that natural environments help to restore attention because people do not have to work so hard to concentrate in nature, there are fewer distractions; that "rest," they theorize, helps to restore attention and the ability to concentrate and focus. Ulrich (1983) has theorized that, because of our evolutionary past, the visual and aesthetic properties of nature produce an automatic response that can reduce stress and evoke positive emotions.

Our Predictions About How to Most Effectively Improve EFs

We predict that the activities that will most successfully improve EFs will include each of the following elements:

- They will tax EFs, continually challenging them in new and different ways.
- They will be personally meaningful and relevant, inspiring a deep commitment and emotional investment on the part of participants to the activity and perhaps also to one another.

- They will have a mentor or guide who firmly believes in the efficacy of the activity and is supportive (sincerely cares about and believes steadfastly in the individual participants).
- They will provide joy, reduce feelings of stress and loneliness and inspire self-confidence and pride.

What activities are most likely to have those characteristics? We propose the answer is *real-world activities*, as studies of El Sistema music (Holochwost et al., 2017), Experience Corps (Carlson et al., 2009), theater (Noice et al., 2004), and sports (Ishihara et al., 2017; Koutsandréou et al., 2016) suggest. We predict that a great many activities not yet studied for their possible EF benefits might well improve EFs, including *group musical activities* (such as band, choral singing, or a drumming circle; Ho, Tsao, Bloch, & Zeltzer, 2011; Metzler-Baddeley et al., 2014; Smith, Viljoen, & McGeachie, 2014), *mindful movement activities* (such as aikido, judo, jiujitsu, qigong, and taekkyeon), *sports* (such as basketball, synchronized swimming, rock climbing, or rowing crew), *other physical activities* (such as orienteering, wilderness survival, or youth circus; Bolton, 2004; Davis & Agans, 2013), *communal dance forms* (such as contradance, hip hop, and rueda (Gill, 2009), *other creative activities* (such as filmmaking), *social-service activities* (such as "Free the Children"—now called "WE"; Kielburger & Kielburger, 2008; Kielburger & Major, 1999), participating in the *Boy Scouts* or *4-H* (Gestsdóttir & Lerner, 2007; Lerner, Lerner, Bowers, Lewin-Bizan, & von Eye, 2011), *caring for an animal* (Ling, Kelly, & Diamond, 2016; Raina, Waltner-Toews, Bonnet, Woodward, & Abernathy, 1999), musical or physical activities that are less communal, or any number of other activities that tax EFs, engender a strong emotional commitment on the part of participants, have inspiring leaders, bring joy, and build self-confidence.

Continually Challenge EFs in New and Different Ways

Real-world activities train diverse EF skills in diverse situations. Rarely does exactly the same situation occur twice in real life. When EFs are always trained in the same few contexts, the training is less likely to generalize outside those contexts (for a similar arguments, see Moreau & Conway, 2014, and Pesce, Croce, et al., 2016).

It has been known for decades that variable training (or varied practice), where participants are continually presented with novel situations in which to practice a skill, leads to better long-term performance than constant or blocked training with the same materials (Ahissar & Hochstein, 2004; Bransford et al., 1977; Rosenbaum et al., 2001; Schmidt & Bjork, 1992; Shapiro & Schmidt, 1982). A problem with many computerized cognitive training and physical-activity training regimens is there is a fair bit of repetition within a very limited set of contexts. People then become very good in those specific contexts, but such training does not provide a good basis for generalizing to other contexts. People only come to generalize by being presented with lots of different contexts where that skill is needed. School programs that embed training in, and challenges to, EF skills in diverse activities, such as reading, math, and play, capitalize on this principle.

Training regimens typically focus on training one skill at a time, eliminating demands on all other skills for the moment. However, the real world is inherently complex, often requiring multiple EF skills at once or in close succession.[19] Perhaps that is one reason why Ishihara et al. (2017) found better results when youngsters practiced a simplified form of tennis from the start rather than starting the usual way with practicing individual tennis skills in isolation.

The training strategies that produce better long-term results, that encourage learning at a deeper rather than at a more superficial level, generally take longer to show benefits. For example, benefits from constant practice are evident earlier than benefits from variable practice, but the transfer from constant practice is exceedingly narrow and task-specific. Similarly, massed practice (practicing a lot over a short period) produces better immediate gains than does spaced practice, but spaced practice (where practice is distributed over time) leads to better long-term gains (Landauer & Bjork, 1978; Rea & Modigliani, 1985; Rosenbaum et al., 2001; Shea & Morgan, 1979).

If we want lasting benefits, we need to be patient. We will likely need to continue the training for longer and will likely need to wait until longer after the training ends to see the full benefit. These assumptions should be tested.

Training diverse skills in parallel takes longer to show gains than training just one skill. For example, Bergman Nutley et al. (2011) found greater immediate

[19] Diamond and Ling (2016, p. 40) stated:

> Most sports place demands on each of the EFs. Participants need to remember complex movement sequences, mentally work with lots of information, processing in real-time cues such as people's positions and where they will likely go next (for ball sports, cues about the ball's location and trajectory), mentally compare the present situation with past ones, and use that to predict what is likely to happen next or down the line (i.e., they must use WM). Participants need to inhibit attending to distractions and keep their attention focused; they must inhibit a planned action when that is suddenly no longer a good idea and inhibit what might be their first inclination, such as the temptation to try to score oneself rather than passing (i.e., they must use inhibitory control). And, they must use cognitive flexibility: The situation is constantly changing. Participants must quickly and accurately evaluate and respond to those changes, flexibly switching plans in real time, adjusting to the unexpected, adapting to complex and rapidly changing conditions. The situation they are faced with at any moment is often different from anything they have faced before. They can never know for sure what someone else will do; at best they can only predict. Some of this can become automatized and no longer require top-down control, but (a) that is less true for people relatively new to a sport and (b) typically the difficulty of what one is facing keeps increasing. As other players or opponents get better at the sport, the inherent difficulty of what one is faced with increases, providing constant challenge.

benefits to WM and reasoning, respectively, when children were trained only on WM or only on reasoning than when children were trained on both. If we want robust, lasting benefits to diverse EF skills, longer training may be needed and assessment at longer intervals after training might be needed if we are to see the full gains.

Relevant here is a hypothesis championed by both Pesce and Vazou that the difference between physical-activity programs that are successful in improving EFs and those that are not lies in the presence of skilled instructors who use effective teaching methods to create challenging learning contexts that promote mental engagement and the motivation to push oneself and master new skills (Pesce, Masci, et al., 2016; Vazou & Smiley-Oyen, 2014).

Deep Commitment, Passionate Interest, Emotional Investment

We predict that whether participants are emotionally invested in an activity that requires EFs may be key to whether that activity improves EFs. EFs should improve most when people are engaged in activities they care deeply about and for which improving EFs improves performance. Aspects of activities that may lead people to deeply care about the activity (become emotionally invested in it) include feeling the activity matters, having a say in how the activity is done, forming strong personal bonds with others doing or teaching the activity, thoroughly enjoying the activity, gaining feelings of pride, self-confidence, or improved self-esteem from doing the activity, and feeling challenged in a good way by it.

Emotional investment matters because, if someone is deeply committed to an activity, that person will devote great time and effort to it. When doing something you thoroughly enjoy, 'work' feels like 'play.' If that activity happens to train and challenge EFs, then sizeable EF improvements should be seen, because it is the time spent practicing, pushing oneself to improve that drives the benefit (Diamond & Lee, 2011; Ericsson, 2006; Ericsson et al., 2009; Ericsson & Towne, 2010). Few of the scores of attempts to improve EFs have looked at participants engaged in anything they deeply care about.

An exception is perhaps Prins et al.'s (2011) study of Cogmed with gaming elements. They found that the gaming elements really sparked the children's interest and the extra time children spent doing Cogmed, beyond that required, was six times greater for the version with gaming elements versus normal Cogmed.

If participants really enjoyed the activity that was studied, they would be more likely to continue doing it after the study. Doing the activity itself and continuing to derive joy from it should help to extend the duration of EF benefits. Research shows that we have better working memory and selective attention when we're happy (Csikszentmihalyi, Abuhamdeh, & Nakamura 2005; Von Hecker & Meiser, 2005; Wendt, Tuckey, & Prosser, 2011; Yang, Yang, & Isen, 2013). The strongest effect is on cognitive flexibility (Hirt et al., 2008). People are able to work more flexibly (Murray, Sujan, Hirt, & Sujan, 1990) and more readily see connections among unusual and atypical members of categories (Isen, Daubman, & Nowicki, 1987; Isen, Johnson, Mertz, & Robinson, 1985) when they are happy.

There is some evidence that EF benefits from any activity may be proportional to how much joy that activity instills (El Haj, Postal, & Allain, 2012; Heyman et al., 2012; Lee, Chan, & Mok, 2010; Raichlen et al., 2012). A possible biological mechanism underlying that could be: When people are enjoying themselves, endocannabinoids (endogenous cannabinoids in the brain) activate the dopamine neurons that project to prefrontal cortex and the nucleus accumbens (Okon-Singer, Hendler, Pessoa, & Shackman, 2015; Wang & Lupica, 2014). The projection to prefrontal cortex aids EFs and the projection to the nucleus accumbens embellishes the experience of pleasure and the willingness to stay on task, endure countless hours of hard work and boring practice, and push oneself to keep improving, all in service of achieving one's goal (Floresco, 2015; see Figure 8.7).

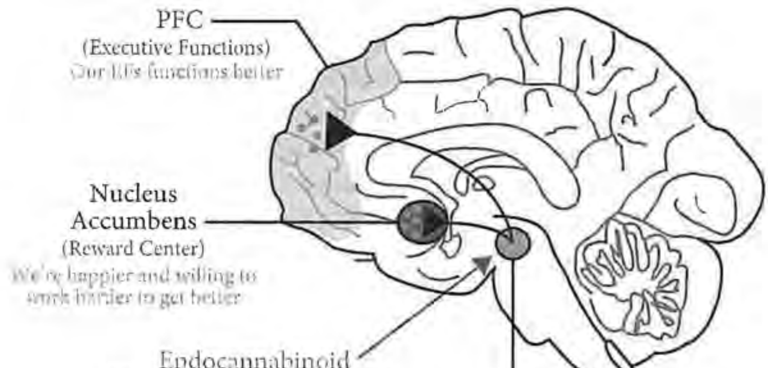

Figure 8.7. How the brain's release of endocannabinoids might help EFs and the willingness to persevere to achieve a goal. When an individual is happy, endocannabinoids activate the ventral tegmental area (the VTA), the source of the dopamine projection to prefrontal cortex (PFC; central for EFs) and the nucleus accumbens (central to the experience of pleasure and willingness to persevere in the service of a goal).

Real-World Activities Versus Practicing Isolated, Decontextualized Skills

People learn something when it is relevant to (when they need it for) what they want to do (Cordova & Lepper, 1996; Olson, 1964). Training decontextualized skills, isolated from their use in a real-world activity, is unlikely to engender deep personal commitment. We predict that training real-world activities will help EFs more than training isolated skills.

Training people on arbitrary computerized tasks or on skills abstracted from a sport, without ever playing the sport, have thus far produced minimal and/or extremely narrow EF benefits. We often train people on decontexualized, component skills first, such as learning technique on a musical instrument or learning individual sports skills, instead of training them by having them play with others from the start in an orchestra or by having them play a simplified version of the sport from the start. The intriguing results from the study by Ishihara et al. (2017) that if tennis is taught by playing a simplified version of tennis from the outset, children improve more and faster at tennis and EFs, than if tennis is taught the traditional way by first learning and practicing the forehand stroke, then backhand, etc., deserve to be followed up. The same principle applies to El Sistema (playing in an orchestra from the outset) versus traditional music instruction (Booth & Turnstall, 2016).

Empowering Participants by Giving Them a Say

Letting participants have a say in how an activity is organized or conducted increases their commitment to it. When people have a say, they experience more ownership of the activity. It is theirs, rather than something imposed on them. Having input (even about something as trivial as the order in which things are done) has been consistently shown to produce more engagement in the activity and more improvement (Cordova & Lepper, 1996; Hooyman, Wulf, & Lewthwaite, 2014; Iyengar & Lepper, 1999; Khan et al., 2014; Williams et al., 1999), even when participants were instructed to do exactly what they would have chosen to do anyway (Ackerlund Brandt et al., 2015). It is empowering to feel that your opinion and ideas count (Eisman et al., 2016; Eitam, Kennedy, & Higgins, 2013; Larson, 2000; Ryan & Deci, 2000). We predict that people will be more invested in EF training (and experience greater EF gains) if they have even a small say in shaping the training activity. Giving people a voice in shaping an EF-training activity has yet to be tested. A fundamental problem with RCTs is that people randomized to do something usually do not have the same commitment to it as people who chose to do it.

Interpersonal Components

The character and quality of interpersonal aspects of an activity are likely far more important than most EF researchers have appreciated.

Positive Relationship Between the Trainer or Mentor and the Participants

Multiple lines of evidence strongly suggest that personal characteristics of those leading a program have a major impact on how beneficial a program is. This has received too little attention in the EF-training literature and deserves more study. The beneficial effects of someone who believes in and cares about you can be huge. A supportive mentor, who believes in the program and the ability of participants to succeed, who genuinely cares about each individual participating, and who helps build the self-confidence and self-esteem of participants, can be critical to a program's success (Frank, 1961; Freedman, 1993).

Lakes and Hoyt (2004) found tremendous EF benefits when the developer of a Taekwondo program was the person administering it. Not only did he believe in his program, but also by all accounts he is a remarkable human being. Whether the person leading a program is committed to its success, believes firmly in its efficacy, and believes in the participants in the program, and whether the local community is supportive of an intervention and has had a say in crafting it, are just some of the many factors that might be key to why or whether a program is successful.

Cogmed includes a one-on-one in-person mentoring component with a supportive, encouraging adult. One study suggested that that component might be even more decisive for the benefits from Cogmed training than the computerized component that is emphasized (de Jong, 2014). The two times Cogmed has been compared to other programs with significant trainer–participant interaction, the benefits from Cogmed and the other programs have not differed much (Gray et al., 2012; van der Donk et al., 2015).

A deeply caring relationship between the trainer and the children produces the best outcomes. After reviewing copious amounts of data from all over the world, Melhuish concluded that what matters most for early-childhood-education outcomes is not the adult to child ratio, class size, instructional style, or quality of materials. What matters most is the adult–child relationship (Melhuish, 2004; Melhuish, Ereky-Stevens, et al., 2015).

Smith and Smoll have repeatedly found that win–loss records bear little relation to youths' self-esteem, enjoyment of a sport, performance anxiety, or feelings about their coach. Indeed, "Virtually all the systematic variance in outcome was accounted for by differences in coaching behaviors" (Smith & Smoll, 1997, p. 17). The most positive outcomes occurred with coaches who conveyed that they genuinely cared about the youths, were generous in giving praise and in giving encouragement in the face of mistakes, minimized stress (in particular, were never hurtful or mean and never embarrassed a team member), fostered camaraderie, and emphasized the importance of having fun while doing the activity (Smith, Smoll, & Barnett, 1995; Smoll, Smith, Barnett, & Everett, 1993).

The founder of Communities in Schools, Bill Milliken, has famously said, "It's relationships, not programs, that change children. A great program simply creates the environment for healthy relationships to form between adults and children. Young people thrive when adults care about them . . . and when they also have a sense of belonging to a caring community" (https://www.communitiesinschools.org/about-us/).

None of this proves that the relationship between the trainer or mentor and the trainees will prove decisive for EF outcomes, but we predict it will.

Building Social Connections and a Sense of Camaraderie

Humans are fundamentally social (Baumeister & Leary, 1995; Cacioppo & Patrick, 2008). We need to feel liked and accepted. We need to feel we're not alone. Feeling socially excluded not only is painful subjectively, it also activates the same brain network as that for physical pain (Eisenberger, 2012). We thrive when we know beyond a shadow of a doubt that there are people who care about us, believe in us, and are there for us. There is evidence that people tend to be far more invested in an activity if they are working together with others toward an important shared goal (Michael, Sebanz, & Knoblich, 2016). We are often happiest when we feel part of a group working toward a common goal (Putnam, 2000).

Many real-world activities, such as sports, involve working together with others toward a common goal. Some of the best results for improving EFs have come from programs that build feelings of community and connections with others (e.g., Experience Corps; Carlson et al., 2009). It is interesting that in the study by Verghese et al. (2003) that followed almost 500 adults who showed no sign of dementia at age 75 for 5 years, the researchers found that those who did social ballroom dance showed the least signs of dementia, while other physical activities, such as walking, biking, swimming, or participating in group exercise, were not associated with any reduced risk of dementia.

Results of three different meta-analyses indicate that people show greater adherence to an exercise program (fewer missed sessions, longer participation) when they participate in groups (especially cohesive ones without major differences in ability) rather than on their own (Burke, Carron, Ets, Mtoumanis, & Estabrooks, 2006; Carron, Hausenblas, & Mack, 1996; Dishman & Buckworth, 1996). Thus, positive social elements might aid EF benefits in part just by increasing exposure to the activity.

When we're lonely, our EFs suffer (Cacioppo & Patrick, 2008; Campbell et al., 2006). When we feel socially supported, we show better EFs (Cacioppo & Patrick, 2008; Tangney, Baumeister, & Boone, 2004). Feeling alone, without social support, has been shown to impair selective attention, self-control, and reasoning (Baumeister, DeWall, Ciarocco, & Twenge, 2005; Campbell et al., 2006; Twenge, Catanese, & Baumeister, 2002). Even anticipating being alone in the future has been shown to impair logical reasoning (a higher-order EF), although not simple memorization (which does not require EFs; Baumeister, Twenge, & Nuss, 2002).

That was true even in comparison with anticipating other negative experiences. Conversely, simple getting-to-know-you interactions with strangers (without any cooperative goal) was found in one study to boost EFs as much as doing cognitive activities (Ybarra, Winkielman, Yeh, Burnstein, & Kavanagh, 2011).

Minimize Stress and Avoid Negative Experiences

Studies of various mindfulness practices provide evidence in support of our hypothesis that programs that reduce stress will be more effective in improving EFs (for yoga: Bilderbeck et al., 2013; Gothe et al., 2016; Purohit & Pradhan, 2017; for more sedentary meditation: Jha et al., 2010, 2015; Leonard et al., 2013; Napoli et al., 2005). For example, Gothe et al. (2014, 2016, 2017) found that yoga resulted in more reduced stress and anxiety according to both self-report and cortisol measures than was found for the control group, and that yoga produced impressive EF outcomes across all five measures compared to the control group. Similarly, Napoli et al. (2005) found that children in a mindfulness program decreased more in test anxiety and improved more in EFs than other children randomly assigned to reading or other quiet activities.

We would like to underline a finding from Curtis, Smith, & Smoll (1979) and Smith et al. (1983). In both studies, the investigators found that although baseball and basketball coaches rarely engaged in punitive or hostile actions toward the youths they were coaching, those rare behaviors had devastating and disproportionate impacts. It is not enough to usually be supportive. It is an important principle that one negative act, such as humiliating someone, can override the benefit of scores of positive ones.

Montessori (1989) was adamant that one should never embarrass a child. Mentors and program leaders need to create an environment where participants feel safe to take risks and try. That means that participants feel it is okay if they make mistakes. Treating errors and failed attempts as learning opportunities, or as simply what happens when you venture beyond what you are already confident of, has been demonstrated to be important for improving at diverse skills (Blackwell, Trzesniewski, & Dweck, 2007; Dweck, 2002, 2006). We predict that will also be key for improving EFs.

The biological mechanisms by which even mild stress disproportionately affects prefrontal cortex, the brain region that plays a key role in subserving EFs, have been well described. There are more receptors for the stress hormone cortisol in prefrontal cortex than in any other region of the primate brain (Sánchez, Young, Plotsky, & Insel, 2000). Thus, prefrontal cortex is especially sensitive to increases in cortisol. Mild stress markedly increases the amount of the neurotransmitters dopamine and norepinephrine in prefrontal cortex but not elsewhere in the brain (Deutch & Roth, 1990; Finlay, Zigmond, & Abercrombie., 1995). These levels of dopamine and norepinephrine are too high for prefrontal cortex to function properly. Higher levels of dopamine in prefrontal cortex during stress correlate with the degree of EF impairment (Murphy, Arnsten, Goldman-Rakic, & Roth, 1996). High levels of dopamine and norepinephrine interfere with signal transfer from the dendrites to the cell body, impairing signal-to-noise in prefrontal cortex (Marek & Aghajanian, 1999; Yang & Seamans, 1996). High levels of norepinephrine during stress also engage low-affinity norepinephrine receptors (alpha-1 receptors; Arnsten, 2000; Ramos et al., 2005) that impair prefrontal cortex function by reducing neuronal firing (Birnbaum et al., 2004; Mao, Arnsten, & Li, 1999). Indeed, scientists have worked out the intracellular signaling events that open ion channels and weaken prefrontal cortex network connections (Arnsten, 2009). Even mild stress impairs the communication between prefrontal cortex and other brain regions, which impairs EFs (Liston, McEwen, & Casey, 2009).

It is no accident that stress increases both cortisol and catecholamine neurotransmitter levels in prefrontal cortex. In part, prefrontal cortisol receptors regulate prefrontal dopamine and norepinephrine levels. During stress, the higher levels of cortisol block the transporters that would normally clear dopamine and norepinephrine, allowing levels of those neurotransmitters to increase (Grundemann, Schechinger, Rappold, & Schömig, 1998)

Improve Self-Confidence and Increase Feelings of Self-Efficacy

When people feel confident that they are capable of succeeding and believe that through effort theycan improve has been shown, in multiple arenas, to be pivotal in affecting whether people do succeed (Bandura, 1994, 2006; Blackwell et al., 2007; Dweck, 2002, 2006; Murphy & Dweck, 2010). Our expectations about whether or not we can do something have a huge effect on whether we succeed (Aronson et al., 1999; Good, Aronson, & Harder, 2008; Steele & Aronson, 1998). We do not know of any data specifically on the importance of believing in yourself or expecting that you can succeed for whether or how much your EFs improve. However, we predict that these attitudes will be as important for improving EFs as they are for improving on anything else.

It helps people to believe in themselves and to feel proud and self-confident if they are given challenges that are do-able but push their limits (so they can see for themselves that they are capable). It also helps if the trainer, mentor, or teacher shows that he or she firmly believes that the trainee or student will succeed (Rosenthal & Jacobsen, 1968). One way to show faith in someone is to give them an important responsibility. For example, when students who were major discipline problems and poor readers were asked to take on the responsibility of tutoring students several years younger who were struggling with reading, both groups improved significantly in reading, the tutors' school attendance, grades, attitudes toward school, and self-concept improved, and their discipline problems disappeared (Cardenas, Montecel, Supik, & Harris, 1992).

Many of the real-world activities reviewed here take participants repeatedly through a cycle of what had looked impossible becoming easy after hours and hours of practice. Participants see themselves accomplishing things they had never thought possible. That builds confidence.

Final Thoughts

We predict that if a program challenges EFs and brings joy, builds self-confidence, and enhances social well-being, EFs should improve more than if the program focuses only on challenging EFs. That is, supporting the other aspects of an individual (emotional, social, and physical) that support optimal EF performance may be key to seeing benefits and seeing them last.

That prediction is consistent with a theory advanced by Diamond (2013, 2014; Diamond & Ling, 2016; Ling et al., 2016), illustrated in Figure 8.8, which holds that activities that will most successfully improve EFs will not only directly train and challenge EFs, but also indirectly support EFs by lessening things that impair them (like sadness or stress) and by enhancing things that support them (like joy or feeling socially supported or self-confident). People show better EFs when they are happy, feel socially supported, and are healthy and physically fit (Etnier et al., 2006; Hirt et al., 2008; Isen et al., 1987). These are not independent factors. For example, when feelings of being socially supported improve, people also feel happier. The different parts of a person are fundamentally interrelated (Diamond, 2007). Similarly, when people are sad, stressed, lonely, or not physically fit, those conditions impair prefrontal cortex functioning and hence EFs. Indeed, prefrontal cortex and EFs show earlier and greater impairments from sadness, stress, or loneliness than any other brain region or skill or ability (Arnsten, 2015; Baumeister, DeWall, Ciarocco, & Twenge, 2005). Thus, if someone is stressed, sad, or lonely, the very EF skills a program is trying to improve will suffer.

This is a markedly different perspective from that of most EF researchers. Most EF-training studies have focused only on directly training EFs (or improving aerobic fitness to improve EFs), ignoring powerful emotional and social factors that affect EFs. Most EF-training studies have not trained participants on anything they care deeply about.

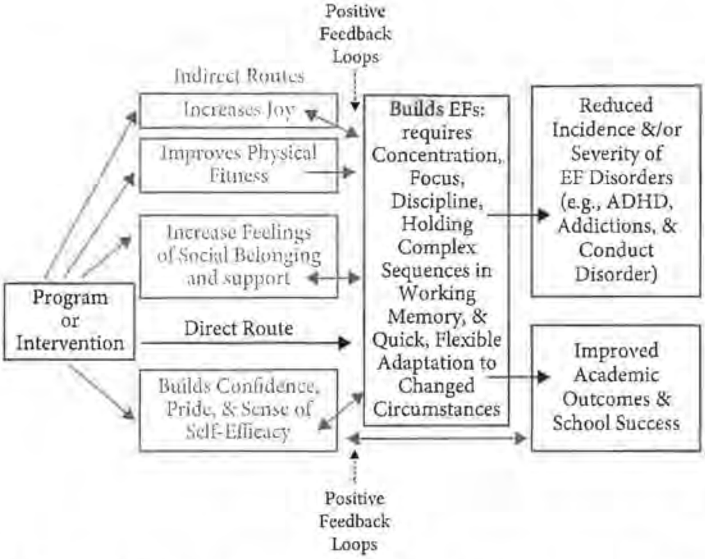

Figure 8.8. Model of direct and indirect routes by which a program or intervention might improve EFs, thereby leading to better school outcomes and the reduced incidence or severity of mental health disorders reflecting poor EFs. While training and challenging EFs are needed for them to improve, that alone may not be enough to achieve the best results. Reprinted with permission from Diamond, A. (2012). Activities and programs that improve children's executive functions. *Current Directions in Psychological Science, 21*, 335–341.

More studies should look at affect, mood, stress levels, and feelings of being socially supported before and after an intervention. Interventions that focus on reducing stress levels should enroll stressed individuals if they want to see sizeable EF benefits.

It could be that the critical difference between the studies where more or fewer EF benefits were found has to do with variables that few studies have reported, such as participants' emotional investment in the training activity, the physical environment in which the activity was done (e.g., outdoors in nature, outside in a city, or indoors), whether the group of participants developed significant camaraderie or not, whether the atmosphere created was one that fostered risk-taking (including risking making a mistake) versus one where participants worried about being embarrassed, and whether the activity leader had a strong conviction that EF benefits would be seen and was supportive rather than punitive.

If a Real-Life Activity Improves EFs (Be It Theatre, Martial Arts, Soccer, a School Curriculum, or Something Else), What Aspect(s) of the Program Are Responsible for That? Why Did the Program Improve EFs?

Our hunch is that the most beneficial programs are gestalts and trying to study just one aspect in isolation will not prove a fruitful endeavor. Beneficial programs work, we hypothesize, because they not only train and challenge EF skills, but also bring joy, pride, and self-confidence, engender a deep commitment, and provide a sense of social belonging (e.g., team membership). For a similar perspective, see Pesce (2012).

For example, soccer is not just aerobic; it requires and builds many fine and gross motor skills, such as eye–hand coordination and balance, requires and builds EF skills, including focused attention, WM, and cognitive flexibility, can build self-confidence and provide great joy, and is social, with all members of a team striving toward a common goal.

The Taekwondo program studied by Lakes and Hoyt (2004) not only worked on physical fitness and motor skills but also trained and challenged EFs and addressed children's social and emotional needs.

Smith and Smoll (1997) found the best outcomes from sports programs where participants helped and supported one another, where they felt the coach genuinely cared about them, where self-confidence was built up and youths' voices were honored, and where participants were not worried about being shamed or embarrassed.

Similarly, the school programs found to improve EFs not only train and challenge diverse EF skills but also address social and emotional needs that support performing at one's best.[20] They build feelings of community and pride, reduce

[20] For example, both the Tools program and Montessori curricula for young children (a) embed training in, and challenges to, EFs in all aspects of the school day, (b) provide supports (scaffolds) for weak EFs so all children experience success and can practice trying to exercise EFs, (c) gradually remove supports as children improve (thus progressively increasing difficulty), (d) go to great lengths to avoid having any child feel embarrassed, (e) imbue the attitude that mistakes are learning opportunities, (f) make it quite clear that they have faith in each child and that each and every one will succeed, (g) give children important responsibilities (conveying the message that each is needed and each is capable), (h) give children a say in planning their day and what skills they work on (encouraging feelings of autonomy and empowerment), (i) provide no extrinsic rewards (such as stickers; in Montessori programs there are not even grades; the intrinsic reward from learning is considered sufficient motivation), (j) nurture a feeling of community, where the children help one another, (k) place a strong emphasis on oral language, (l) have the children engage in active, hands-on learning much of the day singly or with one, two, or a few other children, which enables the teacher to (m) provide individual attention (observing carefully and listening with total attention to what a child has to say), (n) provide individual instruction, and (o) permit each child to progress at his or her own rate (individualized pacing). We expect that the whole package is critical to producing the benefits.

interpersonal conflict, try not to embarrass any child, and build in training on, and progressively greater challenges to, self-control, selective attention, and WM into most school activities. Isolating individual aspects to try to determine which is the critical component will likely risk losing the benefits (for a similar perspective, see Park et al., 2007).

Which Will Matter More, the Type of Program or the Way It Is Done?

We predict that the way a program it is done will prove to be more decisive. An example of "Tain't What You Do (It's the Way That You Do It)" is provided by Trulson (1986). He studied two martial arts programs. One was traditional Taekwondo emphasizing self-control and character development. The other was martial arts presented only as a physical activity and competitive sport. The first produced benefits (e.g., less aggression and anxiety and improved self-esteem). The latter produced deficits (e.g., more aggressiveness and diminished self-esteem).

Similarly, a sports program can be destructive if it tears down participants' self-esteem, is relentlessly competitive emphasizing being better than someone else rather than better than one's own past best, abdicates character-building aspects of the activity, or forgets that first and foremost the activity should be fun. Indeed, Smith, Smoll, and their colleagues have repeatedly found that sports programs high in supportiveness produce major benefits to youths' self-esteem and willingness to persist in the face of adversity, whereas sports programs nominally the same (the same sport, with youths of the same age) where supportiveness was low produce the opposite effects on self-esteem and perseverance (Smith & Smoll, 1997). Programs high in supportiveness had four features: Instead of emphasizing competing against others, the coaches emphasized "giving maximum effort and making improvement. The explicit and primary focus [was] on having fun, deriving satisfaction from being on the team, learning sport skills, and increasing self-esteem" (p. 18). Second, the coaches gave a lot of positive reinforcement, encouragement, and sound technical instruction, and avoided responding hostilely or punitively. They specifically tried to reduce youths' fear of failing. Third, through modeling supportive behaviors and praising actions that promoted team unity, the coaches established norms on their teams that emphasized "mutual obligation to help and support one another" (p.18). Fourth, the coaches involved the youths in decisions regarding team rules.

Smith and Smoll (1997, p. 17) concluded, "The most important factor determining outcomes is the manner in which this important social learning situation [i.e., the physical activity] is structured and supervised." We don't know that the effects of program characteristics that Trulson (1986) and Smith and Smoll (1997) reported will be found for EF outcomes, but we predict they will.

Almost any activity could probably be the means for improving EFs as long as it has the elements mentioned above—(1) it keeps taxing EFs in new and different ways, (2) it is personally meaningful, inspiring a deep commitment and emotional investment, (3) it has a mentor who firmly believes in the activity and in the trainees, and (4) it provides joy and camaraderie, reduces feelings of stress, and inspires self-confidence and pride. The way an activity is done will prove, we predict, to be more critical than what the activity is.

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