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Younger Adults Show Long-Term Effects of Cognitive Training on Broad Cognitive Abilities Over 2 Years

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In the COGITO study (Schmiedek, Lövdén, & Lindenberger, 2010), 101 younger adults practiced 12 tests of perceptual speed, working memory, and episodic memory for over 100 daily 1-hr sessions. The intervention resulted in positive transfer to broad cognitive abilities, including reasoning and episodic memory. Here, we examine whether these ability-based transfer effects are maintained over time. Two years after the end of the training, 80 participants returned for follow-up assessments of the comprehensive battery of transfer tasks. We found reliable positive long-term transfer effects for reasoning and episodic memory, controlling for retest effects by including participants from the original control group. This shows, for the first time, that intensive cognitive training interventions can have long-term broad transfer at the level of cognitive abilities.

Keywords: cognitive training, cognitive abilities, transfer effects, latent change score models, long-term effects

Attempts to improve cognitive functioning with training interventions have a long history in psychology. For many years, interventions used strategy instruction and practice on tasks from psychometric test batteries of cognitive abilities, and at most these interventions produced transfer effects (i.e., improvements on untrained tasks) that must be considered narrow (Noack, Lövdén, Schmiedek, & Lindenberger, 2009). More recently, however, cognitive training research has produced a number of findings that paint a more positive picture of the effectiveness of practice-induced changes of cognitive functioning. The most promising findings come from trainings that (a) build on self-guided practice, rather than instruction of strategies (cf. Hofland, Willis, & Baltes, 1981); (b) focus on the core capacities of working memory (WM; e.g., Dahlin, Stigsdotter-Neely, Larsson, Bäckman, & Nyberg, 2008; Jaeggi, Buschkuehl, Jonides, & Perrig, 2008; Klingberg et al., 2005; see Morrison & Chein, 2011, for review) or executive functions like task switching (Karbach & Kray, 2009); and (c) use computerized setups that adapt task difficulties to a continuously challenging level. Holding individualized task-difficulty up high creates a continuous mismatch of cognitive demands and individual functional supplies. Such mismatches, if present for a prolonged period, could have the potential to improve cognitive processing efficiency rather than merely exploiting the available behavioral flexibility with effective, but typically task-specific, strategies (Lövdén, Bäckman, Lindenberger, Schaefer, & Schmiedek, 2010). As of recently, failed replications of WM training studies have also been reported (Chooi & Thompson, 2012; Redick et al., 2012), and critical reviews on WM training have appeared (Melby-Lervåg & Hulme, 2013; Shipstead, Hicks, & Engle, 2012; Shipstead, Redick, & Engle, 2012). Thus, the jury on the effectiveness and efficiency of cognitive training is still out and awaiting further empirical evidence that allows evaluating its usefulness.

To be of practical relevance for everyday competencies, training-induced changes need to meet two criteria. First, changes need to be located at the level of broad cognitive *abilities*, that is, they have to reach beyond the acquisition of task-specific *skills*. Second, changes need to be enduring, that is, maintained for some

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time after the training intervention has ended (cf. Sternberg, 2008). Ideally, training interventions enhance the long-term trajectory of cognitive development, foster success in educational and professional settings, and extend the period in old age during which individuals are able to live independently (Hertzog, Kramer, Wilson, & Lindenberger, 2008).

Empirically, the first criterion can be evaluated by investigating the range of transfer effects. Effects observed on individual transfer tasks, however, provide only weak evidence for improvements in general cognitive abilities. If an ability (e.g., reasoning) had indeed improved, one would expect that performance on indicator tasks (e.g., Raven's Advanced Progressive Matrices; Raven & Horn, 2009) of this ability should improve. However, because performance on observed tasks can be influenced by factors beyond the underlying ability, like measurement error or task-specific skills, the practice of relying on individual indicators of a given ability can easily lead to false positive findings (e.g., improvements due to the acquisition of task-specific skills) as well as negative findings (e.g., due to lack of power because of improvements in ability being blurred by task-specific variance and measurement error) regarding the question of whether the underlying ability has improved.

Therefore, studies on transfer of training need to investigate whether transfer can be discerned at the level of cognitive abilities (Lövdén et al., 2010; Noack et al., 2009; Schmiedek, Lövdén, & Lindenberger, 2010; Shipstead et al., 2012). This requires assessing transfer with broad selections of heterogeneous tasks that cover the range of the target ability in a comprehensive manner and test changes at the level of common factors of these tasks. Such common factors represent sources of variance that are shared across tasks and are therefore free from measurement error and task-specific influences. Demonstrating transfer at this level provides a more solid basis for concluding that ability has improved than focusing on the task level.

Using data from the COGITO study, in which 101 younger and 103 older adults practiced a battery of 12 cognitive tasks over 100 daily sessions, Schmiedek et al. (2010) could show that a cognitive intervention can result in transfer at the ability level for reasoning (i.e., fluid intelligence) and episodic memory in healthy younger adults. In addition, transfer was observed on a factor of WM tasks in both age groups. The tasks comprising this factor were structurally similar to the trained ones but differed in task content. Transfer of training was not reliable for reasoning and episodic memory in the older adults, and for perceptual speed as well as for a factor of complex span tasks of WM in both age groups.

Regarding the criterion of temporal preservation, there is evidence that improvements can be maintained up to several years, particularly for improvements on the trained tasks (e.g., Ball et al., 2002) and for specific strategies and skills (e.g., Brehmer et al., 2008; Klauer & Phye, 2008; Stigsdotter-Neely & Bäckman, 1993). For long-term transfer effects, empirical evidence is scarcer. There is some indication that transfer effects can be maintained up to 18 months (e.g., Borella, Carretti, Riboldi, & De Beni, 2010; Dahlin, Nyberg, Bäckman, & Stigsdotter-Neely, 2008; Holmes, Gathercole, & Dunning, 2009; Li et al., 2008). Regarding the question of transfer breadth, earlier studies are of limited value because they were either confined to near transfer or to single indicator tasks per target ability.

It is completely unknown whether transfer at the level of latent ability factors induced by cognitive interventions can be maintained over longer periods of time (e.g., years). The COGITO study provides an opportunity to address this question because participants of the training and control groups came back for follow-up assessments of the transfer tasks about 2 years after posttest. Sample sizes at follow-up were sufficiently large to investigate long-term transfer effects at the ability level using *latent change score models* (McArdle, 2009; McArdle & Prindle, 2008). These models have the advantage of allowing to directly test transfer effects at the latent factor level, which no longer contains task-specific sources of variance or measurement error (see Figure 1). We predicted that the pattern of positive transfer at the factor level at follow-up (i.e., changes from pretest to follow-up for the training group minus corresponding changes for the control group) that we observed at posttest would be maintained at follow-up. As no reliable transfer effects for the abilities of episodic memory and reasoning could be demonstrated for the older adults at posttest, we restricted our analyses to the younger adults.

Method

Participants and Procedure

During the training phase, 101 younger adults (51.5% women, M_{age} = 25.6 years, SD_{age} = 2.7, range: 20–31 years) completed an average of 101 practice sessions (*SD* = 2.6, range: 87–109). Participants in the no-contact control group were 44 younger adults (47.7% women, M_{age} = 25.2 years, SD_{age} = 2.5, range: 21–29 years). Before and after the training, participants completed pre- and posttests during 10 sessions that consisted of 2–2.5 hr of comprehensive cognitive test batteries and self-report questionnaires. On average, time elapsing between pre- and posttest was 197 versus 193 days for the training and control groups, respectively. Additional information on sample characteristics and study dropout can be found in Schmiedek, Lövdén, and Lindenberger (2010) and Schmiedek, Bauer, Lövden, Brose, and Lindenberger (2010).

The cognitive assessment of the posttest sessions was repeated at the 2-year follow-up (time from posttest to follow-up: M_{time} = 755 days, *Mdn* = 749 days, range: 679–927 days, for the training group; M_{time} = 745 days, *Mdn* = 742 days, range: 693–798 days, for the control group). Participation rates at follow-up were satisfactory (80 younger adults in the training and 32 in the control group, corresponding to 79% and 73% of the original sample sizes, respectively). Comparisons of pretest performance on the transfer tasks and on the Digit-Symbol Substitution Test (Wechsler, 1981) showed that the follow-up sample did not differ significantly from the dropouts between posttest and follow-up (*p*s $>$ .05), with the exception of numerical reasoning, for which the follow-up sample had significantly higher performance at pretest than did the dropouts, $t(99) = 2.22$, $p = .028$. The present analyses were confined to the follow-up sample. Within this sample, pretest differences on the transfer tasks and the Digit-Symbol Substitution Test between the trained and control groups were not significant (*p*s $>$ .05).

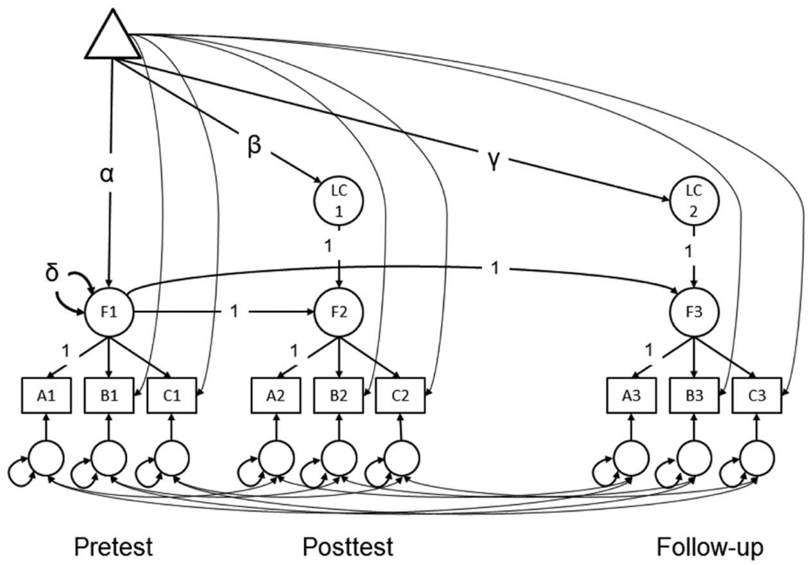

Figure 1. Latent change score model for modeling training-induced changes at the latent factor level. Squares represent observed variables, circles represent latent factors, and the triangle serves to represent information regarding means and intercepts. A1–A3, B1–B3, C1–C3 = observed indicator variables A, B, and C (i.e., tasks of one ability) measured at the three time points; F1–F3: latent factor of ability at the three time points; LC1: latent change factor from pretest to posttest; LC2: latent change factor from pretest to follow-up; α: latent mean of ability factor at pretest; β: mean change of latent ability factors from pre- to posttest; γ: mean change of latent ability factors from pretest to follow-up; δ: variance (individual differences) in latent ability at pretest; variances of the latent change factors was fixed to zero, because they were not significant. Loadings of observed variables on latent factors, intercepts of observed variables, and residual variances were fixed to be the same across the three time points and across training and control groups (i.e., strict measurement invariance). Residuals for the same observed variable were allowed to correlate across time points.

Tasks

In each session, participants practiced 12 different computerized tasks with two to eight blocks each. For perceptual speed, those were three two-choice reaction tasks (odd vs. even numbers; consonants vs. vowels; symmetric vs. asymmetric figures) and three comparison tasks (two strings of digits/consonants, or two three-dimensional figures). For episodic memory, tasks required participants to memorize word lists, number–word pairs, or object positions in a grid. WM tasks were adapted versions of the alpha span, numerical memory updating, and spatial n-back tasks (for details of all tasks, see Schmiedek, Lövdén, & Lindenberger, 2010). Difficulty levels for the choice-reaction, episodic memory, and WM tasks were individualized using different presentation times based on pretest performance.

Transfer tasks included computerized tasks as well as 27 tasks from the paper-and-pencil Berlin Intelligence Structure (BIS) test (Jäger, Süß, & Beauducel, 1997). The three near transfer WM tasks were based on the same three paradigms as the practiced WM tasks, but used different content material. The far transfer WM tasks were established complex span tasks (reading span, counting span, and rotation span). For episodic memory, one computerized word paired-associates task and nine tasks from the BIS (three for each content domain) were used. Transfer in reasoning was measured with 15 items from the Raven's Advanced Progressive Matrices (Raven & Horn, 2009) as well as with nine tasks from the BIS, three for each content domain.

Data Analysis

Effect sizes (d) for single tasks were calculated as mean pre-post (pre-follow-up) differences in accuracy divided by the *SD* of the experimental group at pretest. Net effects provided in Table 1 were obtained by subtracting the effect sizes for the control from those of the training group. Whether these net effects were statistically significant was investigated by testing the interaction of occasion and group with linear mixed effect models (using PROC MIXED in SAS 9.3; Kenward-Roger degrees of freedom; see Littell, Milliken, Stroup, Wolfinger, & Schabenberger, 2006) that allowed for different variances at pre- and posttest (F tests for the interaction are provided in Table 1). Effects at the latent level were analyzed with *latent change score models* (McArdle, 2009; McArdle & Prindle, 2008). In these models, latent factors were defined by a set of transfer tasks. Improvements at the latent factor level were captured by the means of latent change score factors (see Figure 1). In order for these means to be readily interpretable, it is necessary that factor loadings and intercepts are constrained to be equal across occasions and experimental groups (strong measurement invariance). Here, we even aimed for strict measurement invariance (i.e., residual variances also fixed across occasions and experimental groups). Tests of whether mean changes at the latent factor level were significant were conducted by comparing the –2LL of models in which means of the latent change factor were estimated separately for the training and control groups with models in which both means were constrained to be equal, result-

Table 1
Transfer Effects for Follow-Up Sample and Individual Tasks at Posttest and Follow-Up

Task	Pre-post net effect size	Pre-Post × Experimental Group	Pre-follow-up net effect size	Pre-Follow-up × Experimental Group
Working memory—Near				
Animal span	.02	$F(1, 110) = 0.01$, *ns*	−.06	$F(1, 110) = 0.11$, *ns*
N-back numerical	.41	$F(1, 110) = 6.21$, $p = .014$	.46	$F(1, 110) = 9.07$, $p = .003$
Memory updating spatial	.07	$F(1, 124) = 0.18$, *ns*	−.05	$F(1, 124) = 0.06$, *ns*
Working memory—Far				
Reading span	.00	$F(1, 124) = 0.00$, *ns*	.31	$F(1, 124) = 1.72$, *ns*
Counting span	.03	$F(1, 124) = 0.03$, *ns*	.24	$F(1, 124) = 1.24$, *ns*
Rotation span	.08	$F(1, 124) = 0.28$, *ns*	.04	$F(1, 124) = 0.08$, *ns*
Reasoning				
Verbal	.12	$F(1, 110) = 1.38$, *ns*	.22	$F(1, 110) = 4.14$, $p = .044$
Numerical	.25	$F(1, 110) = 5.40$, $p = .022$	.32	$F(1, 110) = 7.11$, $p = .009$
Figural/spatial	.23	$F(1, 110) = 3.68$, *ns*	.27	$F(1, 110) = 7.30$, $p = .008$
Raven	.21	$F(1, 109) = 1.58$, *ns*	.40	$F(1, 107) = 3.90$, *ns*
Memory				
Verbal	.49	$F(1, 110) = 17.09$, $p <.0001$	.15	$F(1, 110) = 1.68$, *ns*
Numerical	.53	$F(1, 110) = 11.15$, $p = .001$	.16	$F(1, 110) = 1.20$, *ns*
Figural/spatial	.20	$F(1, 110) = 3.42$, *ns*	.21	$F(1, 110) = 3.43$, *ns*
Word pairs	.22	$F(1, 110) = 2.20$, *ns*	.16	$F(1, 110) = 0.92$, *ns*

Note. Pre = pretreatment; post = posttreatment.

ing in a χ^2 test with one *df*. Testing whether effects at follow-up differed from those at posttest were conducted by comparing the unconstrained model to one in which the differences training minus control were constrained to be equal for both latent change factors, resulting in a χ^2 test with one *df*.

Model fits were acceptable for reasoning, $\chi^2(75) = 83.91$, root-mean-square error of approximation (RMSEA) = .05, and episodic memory, $\chi^2(75) = 93.61$, RMSEA = .07, but not for the model of WM near transfer tasks, even if only strong measurement invariance was modeled, $\chi^2(60) = 106.73$, RMSEA = .12. We therefore refrain from interpreting results for WM at the latent factor level.

Latent effect sizes were calculated by dividing the latent mean differences by the latent *SDs* at pretest. For analyses of the BIS test, tasks were parceled for each ability construct by calculating composites of standardized scores for the three tasks of each content domain. As these scores were thus already standardized based on pretest *SD*s, mean differences are in effect-size metric and do not need to be divided by *SD*s.

Results

In the following, we focus on long-term transfer effects at the latent factor level and restrict our analyses to those transfer effects for which we found significant results at posttest for the younger adults (Schmiedek, Lövdén, & Lindenberger, 2010); that is, for latent factors of reasoning and episodic memory. Results on transfer effects at the observed task level are reported in Table 1.

For the latent factor of reasoning, there was a significant interaction of experimental group and occasion, $\chi^2(2) = 15.54$, $p <$ .001. The latent net effect sizes were .17, $\chi^2(1) = 7.41$, $p = .006$, at posttest and .23, $\chi^2(1) = 14.57$, $p < .001$, at follow-up. The difference of these effects was not reliable, $\chi^2(1) = 1.12$, *ns*. As shown in Figure 2, this was due to relative stability of latent means for both the trained and the control group. For the latent factor of episodic memory, there was a significant interaction of experimental group and occasion, $\chi^2(2) = 31.45$, $p < .001$. The latent net effect sizes were .47, $\chi^2(1) = 30.48$, $p < .001$, at posttest and .18, $\chi^2(1) = 3.88$, $p = .041$, at follow-up. The difference of these effects was reliable, $\chi^2(1) = 11.54$, $p < .001$. The reduction of the effect was mainly due to a reduction of the effect in the trained group (see Figure 2).

In sum, the results at the latent factor level show that the improvements at the ability level for reasoning and episodic memory were (a) significant at posttest for the reduced follow-up sample, (b) significant at the 2-year follow-up, and (c) significantly reduced at follow-up, in comparison to transfer at posttest, for episodic memory, but not for reasoning. Group differences in motivation are unlikely to be the cause of these effects, as self-reported motivation to work on the tasks did not differ significantly between the training and control groups (see Figure 3).

Discussion

The present results show that far transfer to broad cognitive abilities can be maintained over several years. The sizes of the observed reliable effects were not large. However, their breadth renders them beneficial for a number of real-life outcomes. As reasoning and episodic memory are abilities of high predictive validity for everyday competency (Tucker-Drob, 2011), even small effects can have a substantial impact on performance in educational, professional, and leisure activity settings. Training interventions that lead to small effects of wide scope and high temporal stability may pay off more than interventions that lead to strong but specific effects that do not last for long.

Regarding reasoning, transfer effects at follow-up were significant at the observed task as well as at the latent ability level and of comparable size as at posttest. While for episodic memory, transfer effects were not significant anymore at the observed task level for verbal, numerical, and figural-spatial memory at follow-up (see Table 1), the effect at the level of their common factor was reduced in comparison to the posttest effects, but still

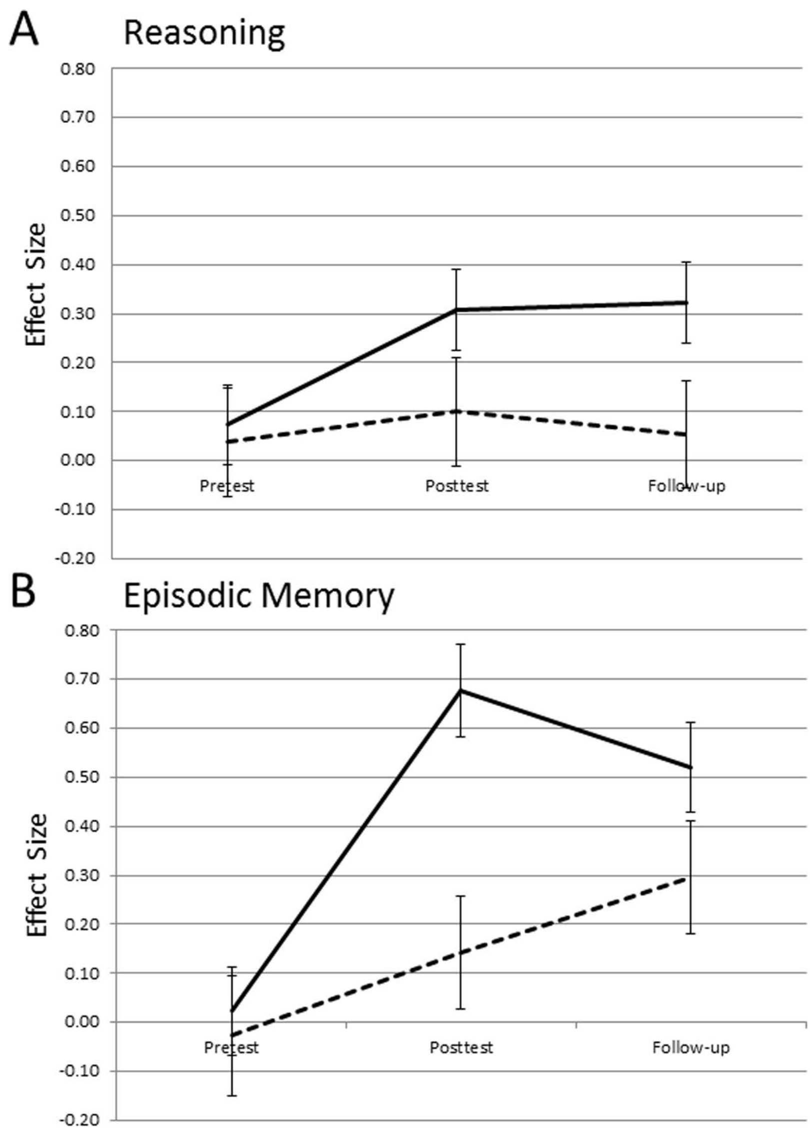

Figure 2. Latent means and associated standard errors for the training and control groups at pretest, posttest, and follow-up. Training group shown with solid lines, control with dashed lines. A: latent factor of reasoning; B: latent factor of episodic memory. As the indicator tasks of the latent factors were standardized by *SD*s at pretest, latent means are in effect size metric.

maintained reliable. This further demonstrates the usefulness of investigating transfer at the latent factor level. At the observed task level, performance is measured with imperfect reliability due to measurement error and might be influenced by task-specific strategies that have been acquired during the training, but could not be reactivated in an effective manner after 2 years. As the latent level only captures sources of variance that have a general influence on all indicator tasks of the factor, general effects, if present, are more easily detectable there.

How did transfer to broad cognitive abilities come about, and how was it maintained over the considerable period of 2 years? We hold that plasticity at the neural level requires a sustained challenge of the cognitive system produced by a mismatch between cognitive demands and functional supplies (Lövdén et al., 2010). The breadth (12 heterogeneous tasks that differed in content and paradigms), intensity (high difficulty due to adjustment to individual performance levels), and dosage (100 sessions of about 1 hr duration) of the training fulfills this requirement and could thereby lead to plastic brain changes, for example, in gray matter (Draganski et al., 2006), white matter (Scholz, Klein, Behrens, & Johansen-Berg, 2009), and neurotransmitter systems (Bäckman et al., 2011; McNab et al., 2009). For a subsample of COGITO participants, Lövdén, Bodammer, et al. (2010) have found indications of improved white-matter microstructure as well as increased volumes of the anterior corpus callosum at posttest. Little is known about the temporal stability of plastic neural changes, and we do not know whether and how they help to preserve positive transfer in broad cognitive abilities.

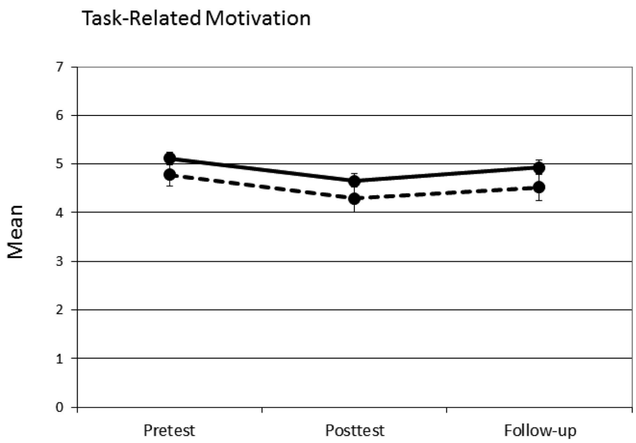

Figure 3. Self-reported motivation to work on the tasks at pretest, posttest, and follow-up for the training and control groups. Participants answered the question "I tried to do well on the tasks" on an 8-point scale (0 = *does not apply at all,* 7 = *does apply very well*) at the end of the session in which they had worked on the Berlin Intelligence Structure test. This information was available on all three occasions for 71 participants from the training and 31 of the control group participants. Solid and broken lines show means for the trained and control group, respectively. Error bars denote standard errors. While the main effect of occasion was significant, $F(2, 202) = 4.69$, $p = .010$, neither the main effect of group, $F(1, 201) = 2.88$, *ns*, nor the interaction of group and occasion, $F(2, 202) = 0.03$, *ns*, was reliable.

In addition to plastic changes at the neural level, we also need to consider rather complex reciprocal effects among the developmental trajectories of cognitive and other psychological variables. Improved cognitive abilities may open opportunities in the educational and professional paths of younger adults that in turn lead to continuously raised levels of cognitive demand, which may help to perpetuate the beneficial effects of the training. Similarly, increased cognitive capacities might lead to an increased need for cognition (Cacioppo, Petty, Feinstein, & Jarvis, 1996) or openness to experience (Jackson, Hill, Payne, Roberts, & Stine-Morrow, 2012) that makes participants seek and face cognitive challenges in their lives. Findings of long-term benefits of early education programs that sometimes last decades after the intervention programs have ended (Barnett, 2011) underscore the importance of taking a developmental perspective on cascading outcomes of training interventions.

The finding that latent transfer effects were reduced at follow-up for episodic memory, but not reasoning, speaks to the possibility that the acquisition of general strategies might also have contributed to the findings for episodic memory at posttest. Besides the influence of task-specific strategies, which should not influence findings at the latent factor level, our participants might also have acquired and practiced more general strategies, like mental imagery, that are supportive for a broad selection of episodic memory tasks. Difficulties with an ad-hoc reactivation of these strategies at follow-up might explain the reduction of transfer effects. As no reasoning tasks were included in the training and as potential strategies used with the practiced WM tasks are much less likely to be of help for performance on the transfer reasoning tasks, a strategy-based explanation of the transfer to reasoning is difficult to entertain.

In sum, the present findings provide room for cautious optimism (cf. Hertzog et al., 2008). Cognitive trainings can produce transfer effects that are sufficiently large in scope and stable over time to justify the considerable effort that is needed to produce them. Future studies should hold up the proposed standard of investigating transfer at the level of latent ability factors and improve on the investigation of the mechanisms that produce transfer and maintenance. Future research will need to take close and continuous looks at postintervention developmental trajectories on behavioral, social, and neural dimensions to better understand the conditions under which cognitive training interventions can trigger a cascade of changes that result in improved or maintained cognitive competence.

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