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# The Malleability of Working Memory and Visuospatial Skills: A Randomized Controlled Study in Older Adults

Hana Stepankova
Prague Psychiatric Center, Prague, Czech Republic, and Charles University in Prague

Jiri Lukavsky
Prague Psychiatric Center, Prague, Czech Republic, and Academy of Sciences of the Czech Republic, Prague

Martin Buschkuehl
University of California, Irvine, and MIND Research Institute, Irvine, California

Miloslav Kopecek and Daniela Ripova
Prague Psychiatric Center, Prague, Czech Republic

Susanne M. Jaeggi
University of California, Irvine

There is accumulating evidence that training on working memory (WM) generalizes to other nontrained domains, and there are reports of transfer effects extending as far as to measures of fluid intelligence. Although there have been several demonstrations of such transfer effects in young adults and children, they have been difficult to demonstrate in older adults. In this study, we investigated the generalizing effects of an adaptive WM intervention on nontrained measures of WM and visuospatial skills. We randomly assigned healthy older adults to train on a verbal *n*-back task over the course of a month for either 10 or 20 sessions. Their performance change was compared with that of a control group. Our results revealed reliable group effects in nontrained standard clinical measures of WM and visuospatial skills in that both training groups outperformed the control group. We also observed a dose–response effect, that is, a positive relationship between training frequency and the gain in visuospatial skills; this finding was further confirmed by a positive correlation between training improvement and transfer. The improvements in visuospatial skills emerged even though the intervention was restricted to the verbal domain. Our work has important implications in that our data provide further evidence for plasticity of cognitive functions in old age.

*Keywords:* aging, plasticity, transfer, dose–response effect, *n*-back



For ages, older adults have been respected for their acquired expertise, knowledge, and wisdom (Rowley & Slack, 2008). Nevertheless, there is a decline in certain cognitive functions during ontogenetic aging which is most apparent in basic sensory functions and processing speed, but it also extends to higher cognitive functions such as executive control and fluid intelligence (Gf; Baltes & Lindenberger, 1997; Bugg, Zook, DeLosh, Davalos, & Davis, 2006; Park et al., 2002; Ryan, Sattler, & Lopez, 2000; Salthouse, 1996), and, ultimately, everyday life functioning (Alzheimer's Association, 2011). Therefore, it would be highly beneficial if there were means to prevent cognitive decline or even improve old adults' cognitive functions. In the current study, we investigated whether a focused cognitive intervention targeting working memory (WM) has the potential to serve as such a means in a healthy young–old adult sample.

There have been numerous attempts to improve higher cognitive functions in older adults, and in particular, researchers have been focusing on improving fluid reasoning or memory by having participants learn and implement cognitive strategies (e.g. Ball et al., 2002; Schaie & Willis, 1986; Verhaeghen, Marcoen, & Goossens, 1992). However, although it has been shown that older adults are able to considerably increase performance with practice, the success of those studies has often been limited. That is, in many cases, the generalizing effects were restricted to tasks that were closely related to the trained domain (e.g. Hayslip, 1989; Zelinski, 2009). Nevertheless, in recent years and especially with the advent of computerized and sophisticated adaptive intervention designs,

Hana Stepankova, Prague Psychiatric Center and the Department of Psychology, Faculty of Arts, Charles University in Prague, Prague, Czech Republic; Jiri Lukavsky, Prague Psychiatric Center and the Institute of Psychology, Academy of Sciences of the Czech Republic, Prague, Czech Republic; Martin Buschkuehl, School of Education, University of California, Irvine, and the MIND Research Institute, Irvine, California; Miloslav Kopecek and Daniela Ripova, Prague Psychiatric Center; Susanne M. Jaeggi, School of Education, University of California, Irvine.

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Correspondence concerning this article should be addressed to Susanne M. Jaeggi, School of Education, 3452 Education, University of California, Irvine, Irvine, CA 92697-5500. E-mail: smjaeggi@uci.edu

the focus of training research has shifted towards "process-based" interventions (Lustig, Shah, Seidler, & Reuter-Lorenz, 2009). For example, there has been accumulating evidence that training on WM can improve skills related to Gf (see, e.g., Buschkuehl & Jaeggi, 2010; Morrison & Chein, 2011; Jaeggi, Buschkuehl, Shah, & Jonides, 2013, for recent reviews).

WM is commonly described as a set of mechanisms capable of retaining a limited amount of information in an active state for the use in ongoing cognitive tasks (e.g. Cowan et al., 2005; Jonides et al., 2008). Gf in turn is described as our general ability to understand and reason inductively about novel and abstract concepts, that is, our ability to solve new or unfamiliar problems independently of previous specific practice or experience (Cattell, 1971; Gottfredson, 1997). It is assumed that WM is one of the mechanisms that underlies individual differences in higher cognitive functions, such as Gf (e.g., Oberauer, Schulze, Wilhelm, & Süss, 2005). Furthermore, it has been argued that WM and Gf share common cognitive processes such as capacity constraints (e.g. Fukuda, Vogel, Mayr, & Awh, 2010; Halford, Cowan, & Andrews, 2007) or the ability to resolve interference (Engle, 2002; Wiley, Jarosz, Cushen, & Colflesh, 2011). Because of this process overlap, it has been hypothesized that improving WM skills would facilitate performance in Gf tasks (Buschkuehl & Jaeggi, 2010; Jaeggi, Buschkuehl, Jonides, & Perrig, 2008; Morrison & Chein, 2011). Another key feature for the success of WM training might be the use of adaptive designs that continuously and individually adjust the task difficulty according to the participants' performance. The adaptive procedure allows participants to train at their individual capacity limits and requires a continuous engagement of executive control. The idea of adaptivity is based on the assumption that training is optimal if the task is neither too hard nor too easy since participants will otherwise disengage due to frustration or boredom (cf. Jaeggi et al., 2008, 2010b; Jaeggi, Buschkuehl, Jonides, & Shah; 2012; Jaeggi, Buschkuehl, Perrig, & Meier, 2010a). The adaptivity concept is in line with Bjork and Bjork's (2011) concept of *desirable difficulties*, which promotes learning. Almost all of the recent WM-based interventions have used adaptive techniques (e.g. Anguera et al., 2012; Buschkuehl et al., 2008; Chein & Morrison, 2010; Jaeggi, Buschkuehl, Jonides, & Shah, 2011; Jaeggi et al., 2008, 2010b; Klingberg et al., 2005; Klingberg, Forssberg, & Westerberg, 2002; Loosli, Buschkuehl, Perrig, & Jaeggi, 2012; Richmond, Morrison, Chein, & Olson, 2011), and in some of those studies, adaptive versus nonadaptive interventions were explicitly compared, and an advantage for adaptive interventions was found (e.g., Klingberg et al., 2002, 2005; Lilienthal, Tamez, Shelton, Myerson, & Hale, 2013). Furthermore, studies in which an adaptive procedure was not used failed to show transfer (Craik et al., 2007; Li et al., 2008), suggesting that adaptivity is one of the key features for training success.

When it comes to older adults, only a few training studies have focused on WM as a training domain. The few published studies in which participants were trained on WM-related skills reveal an inconsistent pattern of results ranging from null-effects to quite substantial generalizations (Hindin & Zelinski, 2012; Melby-Lervåg & Hulme, 2013; Uttal et al., 2013; Zelinski, 2009). These inconsistencies might be the result of different training paradigms, different training frequencies, and differences in the selected criterion tests, as well as individual differences (cf. Jaeggi et al., 2012; Shah, Buschkuehl, Jaeggi, & Jonides, 2012). For example, Dahlin, Neely, Larsson, Backman, and Nyberg (2008) reported functional activation changes after WM updating training; however, in contrast to what they observed in their young adult sample, the investigators found those activation changes were not accompanied by significant changes in behavior in their older adult sample, except for the task on which the participants had been trained. Buschkuehl et al. (2008) as well as Zinke, Zeintl, Eschen, Herzog, and Kliegel (2011) observed improvements in WM in old-old adults after WM training; however, the improvements were restricted to tasks that were closely related to the trained domain. A similar pattern of positive near transfer within the trained domain of WM was reported by Richmond et al. (2011), as well as by Li et al. (2008). On the other hand, Brehmer et al. (2011) observed effects in older adults that went beyond mere improvements in the trained WM domain in that they found positive effects in measures of sustained attention and episodic memory. Schmiedek, Lövdén, and Lindenberger (2010) reported improvements in Gf in old adults after a complex cognitive intervention that consisted of training not only on WM but also on perceptual speed and episodic memory that lasted several months. Improvements in measures of Gf are also reported after much shorter interventions. For example, Borella, Carretti, Riboldi, and De Beni (2010) observed improvements in Gf and other executive control tasks after just a few days of WM training (see Carretti, Borella, Zavagnin, & De Beni, in press, for a recent replication). Finally, there are other reports of improvements in Gf in older adults after interventions that target processes related to WM skills and executive control (Basak, Boot, Voss, & Kramer, 2008; Karbach & Kray, 2009; Tranter & Koutstaal, 2008).

The current study was based on previous research with young adults and children, which demonstrated improvements in measures of Gf after training on an *n*-back task (Jaeggi et al., 2008, 2010b, 2011). An *n*-back task requires participants to process a stream of stimuli (e.g., letters) and indicate for each letter whether it is the same stimulus as the one presented *n* positions back in the sequence. The difficulty of the task can be parametrically varied by increasing the value of *n* (e.g. Jonides et al., 1997). *N*-back tasks require participants to continuously update mental representation of the target items while dropping irrelevant items from consideration, in addition to simple storage processes (Conway et al., 2005; Jonides et al., 1997). In addition to having a "high face validity as a measure of WM due to the attentional requirement to continuously update the stimuli being held on-line" (Hill et al., 2010, p. 4), *n*-back tasks are usually fairly well correlated with measures of Gf (e.g. Jaeggi et al., 2010a, 2010b; Kane, Conway, Miura, & Colflesh, 2007), suggesting at least some process overlap, which might facilitate transfer (cf. Jaeggi et al., 2010b). As discussed previously, it has been argued that this process overlap is most likely related to the ability to resist distraction and resolve interference (Gray, Chabris, & Braver, 2003; Jaeggi et al., 2011; Wiley & Jarosz, 2012).

The foremost goal of the current study was to examine the efficacy of an adaptive computer-based WM intervention in healthy, community-dwelling older adults. To that end, we trained two groups of participants on a single *n*-back task for either 10 or 20 sessions over the course of 5 weeks. We compared the training groups' performance in nontrained measures of WM and visuospatial skills before and after training with the performance of a no-contact control group. Four standardized and widely used clinical measures of WM and visuospatial skills were administered and subsequently combined into a composite score for each domain in order to increase measurement quality (Conway et al., 2005; Kane, et al., 2004). In addition to investigating transfer to WM and visuospatial skills, we were especially interested to see whether training frequency and training gain predicted the extent of transfer in both constructs, which would thereby extend previous findings demonstrating either a dose–response effects in Gf (Basak et al., 2008; Jaeggi et al., 2008) or a relationship between training gain and transfer on Gf (Jaeggi et al., 2011).

## Method

### Design

We used a pretest/posttest randomized controlled trial design with two experimental groups and a control group (CG). All participants were tested twice in an interval of 5 weeks (baseline and posttest). The two experimental groups underwent a computer-based WM intervention between the two assessments, which differed in terms of training frequency, in that participants were asked to train either twice or four times per week distributed over the course of 5 weeks. The low frequency group (Ex10) trained twice a week for a total of 7–12 sessions (mean: 9.42 sessions; $SD = 0.96$; 200–250 min total), whereas the high frequency group (Ex20) trained four times a week for a total of 18–23 sessions (mean: 19.60 sessions; $SD = 0.60$; 450–500 min total). The CG did not train and was appointed to control for test–retest effects.

The study was approved by the Ethics Committee of Prague Psychiatric Center (ref. no. 122/09), and informed written consent was obtained from all participants.

### Participants

Sixty-eight community-dwelling young-old adults were recruited for the study. The participants were recruited through a study-related website, journal advertisements, and flyers and posters. Participants first underwent a standardized clinical interview that determined their eligibility for the study. Exclusion criteria were serious medical illness that prevented unrestricted participation in the study, neurological disorders (head trauma with loss of consciousness, epilepsy, uncorrected sensory impairment, aphasia), a current episode of a psychiatric disorder, or diagnosed or suspected organic mental disorder (especially dementia). We also assessed participants‘ medication use (i.e. antidepressants, hypnotics, anxiolytics, thyroid pills, or other medication), and participants who occasionally took hypnotics (one participant) or anxiolytics (three participants) were asked to refrain from taking the medication 3 days and 3 nights before the pre- and posttest assessments. Finally, we screened participants' general cognitive status with the Mini-Mental State Examination (MMSE; Folstein, Folstein, & McHugh, 1975; cutoff criterion = 27), and in addition, everyone was screened for depression with the Geriatric Depression Scale–15 (Yesavage & Sheikh, 1986; cutoff criterion = 7, scores between 8 and 10 were followed up with a psychiatric clinical interview).[1] Once participants were determined to be eligible for the study, they were randomly assigned to either one of the intervention groups or the control group. Participants who were assigned to the intervention groups trained on their own computers or on a laptop that was provided for the duration of the study.[2] Participants were required to train at home on their own, according to their intervention-specific schedule (discussed previously). That is, there was no contact between the participants and the research team during the intervention period, and the training data files were retrieved after training completion.[3] The computerized training program saved the participants' responses together with a time stamp that allowed us to assess the training time for each session and whether participants adhered to the required training schedule. All participants received a nominal payment of 500 CZK after the second assessment (i.e., about 29 USD), regardless of condition or performance. They were given a brief report of their performance in both assessments, and everyone received the training program for further personal use after study completion.

One person completed the first assessment only and withdrew from the study due to unforeseen family events. One person was excluded from data analyses due to a MMSE score $< 27$ and considerable low scores in the other tests, suggesting a global cognitive impairment. Another person was excluded due to failure to comply with the training schedule. Four participants were moved from the Ex20 to the CG as technical problems prevented them from training. One person was moved from the Ex20 to the Ex10 as she only trained 12 times instead of the required 20 sessions. Note that this group re-allocation was performed solely for logistical reasons and was related neither to transfer nor training performance. Thus, the final sample size was 65 (mean age: 68.1 years; $SD = 2.6$; 47 women); see Table 1 for the demographic data for each group of participants. Note that the three groups did not differ in age, education, gender distribution, initial *n*-back performance, or performance in any of the manifest cognitive baseline assessments (all $F$s $< 2.19$).

### Baseline Assessment and Transfer Measures

In order to assess specific training effects, we administered the same single *n*-back task that was used for training at the pre- and posttest sessions (discussed later). The dependent measure was the average *n*-back level obtained in those sessions.

We assessed transfer in two domains, WM and visuospatial skills. The first target construct, WM, was represented by Digit Span (DS) and Letter Number Sequencing (LNS); subtests of the Wechsler Memory Scale–III (WMS–III; Wechsler, 1997b) and the Wechsler Adult Intelligence Scale–III (WAIS–III; Wechsler, 1997a). The DS comprises two scores: the DS forward subtest requires simple auditory WM capacity, immediate memory, and attention span; individuals are asked to repeat an orally presented digit sequence in forward order. The DS backward subtest assesses more complex WM by requiring individuals to repeat an orally presented digit sequence in backward order. The dependent variable is the sum of correctly repeated forward and backward sequences. In the LNS, a series of orally presented letters and

[1] We only had to follow up with one participant who had a score of 10; however, her data were included as the clinical interview revealed that she did not fulfill the criteria for depression.

[2] All but one participant used his or her own computer for training.

[3] All participants were given detailed written instructions on how to run the program and how to do the task, along with contact information in case they needed assistance; however, none of the participants made use of this opportunity.

Table 1
*Demographic Information*

| Variable | Control group[a] | | Experimental group (10 sessions)[b] | | Experimental group (20 sessions)[c] | |
|---|---|---|---|---|---|---|
| | Mean (*SD*) | Range | Mean (*SD*) | Range | Mean (*SD*) | Range |
| Age (years) | 68.08 *(3.01)* | 65–74 | 67.95 *(2.19)* | 65–72 | 68.15 *(2.62)* | 65–74 |
| Years of education | 14.72 *(2.84)* | 11–20 | 15.30 *(3.18)* | 11–21 | 14.90 *(3.19)* | 10–21 |

*Note*. Control group: $N = 25$; Experimental groups: $N = 20$.
Gender: [a] 18 women and seven men. [b] 15 women and five men. [c] 14 women and six men.



numbers are presented in a random order. Participants are required to reorder and repeat the list by first reproducing the numbers in ascending order and then the letters in alphabetical order. The dependent variable is the number of correctly repeated lists. The LNS requires sequencing, mental manipulation, short-term auditory memory, visual–spatial imaging, and processing-speed (Crowe, 2000) and is regarded as a good clinical test to measure WM functioning (Hill et al., 2010; Shelton, Elliott, Hill, Calamia, & Gouvier, 2009).

The second target construct, visuospatial skills, was operationalized by two tasks—Block Design (BD) and Matrix Reasoning (MR). Both are nonverbal subtests of the WAIS–III (Wechsler, 1997b). Both tests are often used as proxy for Gf (e.g., Bugg et al., 2006; Friedman et al., 2006), and they are reported to have high *g* loadings (Colom, Jung, & Haier, 2006; Roivainen, 2010). In the MR test, participants are presented with a display of geometric shapes or patterns, with one shape or pattern missing. They are required to either name or point to the correct answer alternative to complete the pattern out of five response options. The test is thought to require visual information processing, abstract reasoning skills, learning ability, and mental flexibility (Groth-Marnat, 2009, p. 156). The BD test requires participants to organize colored blocks to match pictures on cards and employs visual perception of abstract designs, spatial processing, visuomotor coordination, and processing speed (e.g. Groth-Marnat, 2009; Kaufman, 2006, p. 401). Whereas there was no time limit on MR, BD was timed to a maximum of 21 min as defined in the handbook of the WAIS–III (Wechsler, 1997b). The dependent measure was determined according to the handbook of the WAIS-III (Wechsler, 1997a). In the MR, it corresponded to the number of correctly solved items, which was also true for the BD, but in addition, the scoring depended on the amount of time needed to solve each item. Note that since there are no parallel-test versions of our transfer measures, the same task versions were used in the pre- and posttest sessions.

## Training Task

We used a verbal version of the n-back task as an intervention task, which was based on other n-back interventions used before (cf. Jaeggi et al., 2008, 2010a, 2011). Participants were presented with a sequence of large yellow capital letters presented in the middle of a screen with a blue background. Participants had to indicate whether the currently presented letter was the same as the one presented *n* positions back in the sequence by pressing the spacebar on the keyboard (no responses were required for nontargets). Presentation time was 500 ms, and the interstimulus interval was 1,500 ms. Each training session consisted of 20 blocks, each lasting for about 1 min and consisting of $20 + n$ stimuli. There were six targets per block that were presented at random positions in the sequence. Each session took about 25 min to complete. The program continuously adapted the level of difficulty according to each individual's performance after each block (0–2 errors: the level of *n* was increased by one for the next block; 3–5 errors: the level remained the same; 6 or more errors: the level of *n* was decreased by one for the next test). Each session started with Level 1 (1-back). At the end of each session, participants received feedback in that they were shown a diagram depicting the levels of *n* they reached in that particular session. The dependent measure consisted of the mean level of *n* reached in each training session.

## Data Analyses

We assessed specific training effects with a univariate analysis of covariance (ANCOVA) using the posttest *n*-back performance as dependent variable, the pretest *n*-back performance as a covariate, and group (CG, Ex10, Ex20) as a between-subject factor. We hypothesized that the two training groups would outperform the CG and that the Ex20 group would outperform the Ex10 group. To test those predictions, we calculated Helmert contrasts in order to compare performance on the group level (i.e., CG vs. Ex10 *and* Ex20; Ex10 vs. Ex20).

The change in cognitive performance in the transfer measures was assessed with two composite scores. Although our pre- and posttest task selection can be clearly assigned to WM (consisting of DS and LNS) or visuospatial skills (consisting of MR and BD) on a theoretical level, we nevertheless aimed to confirm this distinctiveness statistically. To do so, we inspected the correlation coefficients between the composite measures (cf. Table 2), and we

Table 2
*Correlation Matrix (Based on the Pretest Scores)*

| Variable | WM | | VSS | |
|---|---|---|---|---|
| Working memory | DS | LNS | MR | BD |
| Digit Span | — | | | |
| Letter–Number Sequencing | 0.48*** | — | | |
| Visuospatial skills | | | | |
| Matrix Reasoning | 0.13 | 0.08 | — | |
| Block Design | 0.16 | 0.10 | 0.46*** | — |

*Note*. $N = 65$. WM = working memory; VSS = visuospatial skills; DS = Digital Span; LNS = Letter–Number Sequencing; MR = Matrix Reasoning; BD = Block Design.
*** $p < .0001$.

conducted a principal component analysis on the pretest data, which clearly revealed two factors explaining 73% of the total variance. As expected, DS and LNS loaded on one factor, and MR and BD loaded on the other one, confirming their suitability for the subsequent composite analyses.

Next, we divided the pretest score of each task (DS, LNS, MR, and BD) by the standard deviation (*SD*) of the respective pretest scores. We also divided the posttest scores of each task by the *SD* of the respective pretest scores. This resulted in test scores that were standardized in units of *SD* Note that the *SD* was calculated across all three groups. Finally, the standardized scores of the WM tasks (DS and LNS) were averaged for the pretest and for the posttest; the same was done with the two visuospatial skills tasks (MR and BD). These composite measures were used in the forthcoming analyses.

Transfer was assessed with univariate ANCOVAs using the posttest composite as dependent variable, the pretest composite as a covariate, and group (CG, Ex10, Ex20) as a between-subject factor.[4] Again, we hypothesized that the two experimental groups would outperform the CG and that the Ex20 group would outperform the Ex10 group. Consequentially, we calculated Helmert contrasts in order to compare transfer performance on the group level (i.e., CG vs. Ex10 *and* Ex20; Ex10 vs. Ex20).

Finally, we correlated the gain in WM and visuospatial skills with the gain in *n*-back performance as measured with the pre- and posttest *n*-back task in order to investigate the relationship of training improvement and extent of transfer.

## Results

### Specific Training Effects

The ANCOVA analyzing specific training gains as assessed with the pre- and posttest *n*-back task revealed that the covariate (pretest performance) was significantly related to the posttest performance, $F(1, 60) = 9.56$, $p < .001$, $\eta^2_{\text{partial}} = .14$. There was also a significant effect of group after controlling for pretest performance, $F(1, 61) = 13.30$, $p < .001$, $\eta^2_{\text{partial}} = .31$. The participants of the 10-day training group improved their scores by 0.97 ($SD = 0.85$) *n*-back levels and the 20-day training group by 1.39 ($SD = 1.22$) *n*-back levels. The CG retained their original scores (gain = 0.07, $SD = 0.40$); see Figure 1. Planned contrasts revealed that both experimental groups outperformed the CG in terms of *n*-back gain from pretest to posttest regardless of the number of training days, $t(60) = 4.94$, $p < .001$ (one-tailed, $r = .54$). Further, there was a trend showing that the *n*-back improvement of the Ex20 group was larger than the improvement of the Ex10 group, $t(60) = 1.54$, $p = .06$ (one-tailed, $r = .19$).

### Transfer Effects

The descriptive data for all manifest pre- and posttest tasks as well as the composite scores are reported in Table 3 as a function of group. Note that there were no significant group differences in any of the pretest measures, neither in the manifest nor in the composite variables (all *F*s < 2.52).

Concerning transfer on WM, the pretest performance (the covariate) was significantly related to the posttest performance, $F(1, 61) = 108.76$, $p < .001$, $\eta^2_{\text{partial}} = .64$. There was also a significant

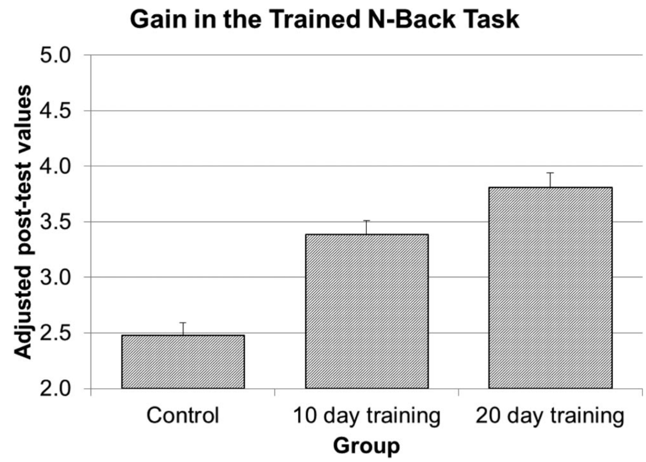


*Figure 1.* Specific training effects. Visualized are the means of the posttest scores adjusted by the effect of the pretest covariate. Error bars represent standard errors of the mean.

effect of training time after controlling for pretest performance, $F(2, 61) = 3.59$; $p < .05$, $\eta^2_{\text{partial}} = .11$; the adjusted group means are visualized in Figure 2. Planned contrasts revealed that regardless of the number of training sessions, training on the *n*-back task resulted in improved WM, $t(61) = 2.68$, $p < .01$ (one-tailed, $r = .32$) as compared with the CG. The contrast between Ex10 and Ex20 was not significant ($p = .49$, one-tailed).

Regarding transfer on visuospatial skills, the covariate was significantly related to the posttest performance, $F(1, 61) = 149.03$, $p < .001$, $\eta^2_{\text{partial}} = .71$). There was also a significant effect of training time after controlling for pretest performance, $F(2, 61) = 7.28$; $p < .01$, $\eta^2_{\text{partial}} = .19$, the adjusted group means are visualized in Figure 2. Planned contrasts revealed that regardless of the number of training sessions, training on the *n*-back task resulted in improved visuospatial skills, $t(61) = 3.29$, $p < .001$ (one-tailed, $r = .39$) as compared with the CG. In addition, the Ex20 group showed a significantly larger gain in visuospatial skills than the Ex10 group, $t(61) = 1.83$, $p < .05$ (one-tailed, $r = .23$). To further confirm this dose–response effect, we considered Cohen's *d* (i.e., the standardized effect size; cf. Table 3). The lowest effect size was observed in the control group ($d = 0.34$), followed by the effect size of the Ex10 group ($d = 0.60$), and the effect size of the Ex20 group ($d = 0.98$). In order to estimate the impact of training frequency, we subtracted the effect size of the control group from each of the experimental groups, resulting in $d = 0.26$ for the Ex10 group and $d = 0.64$ for the Ex20 group, which further supports our notion of a dose–response effect in the visuospatial domain.

Finally, we used Spearman correlations to examine the relationship between *n*-back performance gain as measured with the *n*-back task assessed in the pre- and posttest sessions and the gains

[4] Note that an alternative way to analyze the data would be through a repeated-measures analysis of variance (ANOVA); however, an ANCOVA is more powerful than a repeated-measures ANOVA because the covariate explains additional variance (cf. Weinfurt, 2002). Nevertheless, we also calculated repeated-measures ANOVAs and the resulting significance levels were identical to the ones resulting from the ANCOVAs.

Table 3
*Descriptive Data For All Pre- and Posttest Measures on a Manifest and Construct Level as Function of Group*

| Variable | *N* | Pretest | | | | Posttest | | | | *p* | Effect size (Cohen's *d*) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Mean | *SD* | Min | Max | Mean | *SD* | Min | Max | | |
| Experimental group (10 sessions) | | | | | | | | | | | |
| *n*-back | 20 | 2.4 | 0.36 | 1.75 | 2.9 | 3.38 | 0.92 | 2.4 | 5.8 | *** | 1.39 |
| Digit Span | 20 | 18.65 | 3.7 | 12 | 24 | 20.25 | 4.01 | 12 | 28 | ** | 0.41 |
| Letter–Number Sequencing | 20 | 9.5 | 1.5 | 7 | 12 | 10.4 | 2.26 | 7 | 15 | * | 0.47 |
| Block Design | 20 | 42.25 | 8.89 | 25 | 58 | 46.2 | 8.84 | 25 | 64 | ** | 0.45 |
| Matrix Reasoning | 20 | 18.15 | 4.23 | 10 | 23 | 20.25 | 3.77 | 10 | 25 | ** | 0.52 |
| Working memory (composite) | 20 | 4.86 | 0.79 | 3.35 | 6.20 | 5.30 | 1.01 | 3.35 | 7.48 | *** | 0.49 |
| Visuospatial skills (composite) | 20 | 4.36 | 0.77 | 2.61 | 5.32 | 4.81 | 0.73 | 3.04 | 5.71 | *** | 0.60 |
| Experimental group (20 sessions) | | | | | | | | | | | |
| *n*-back | 20 | 2.49 | 0.4 | 1.9 | 3.35 | 3.88 | 1.29 | 2.65 | 7.4 | *** | 1.46 |
| Digit Span | 20 | 18.95 | 3.49 | 14 | 27 | 20.5 | 3.53 | 14 | 29 | * | 0.44 |
| Letter–Number Sequencing | 20 | 9.75 | 2.49 | 4 | 14 | 10.6 | 2.62 | 5 | 14 | 0.09 | 0.33 |
| Block Design | 20 | 38.35 | 8.68 | 22 | 51 | 44.9 | 8.77 | 31 | 57 | *** | 0.75 |
| Matrix Reasoning | 20 | 17.5 | 4.45 | 8 | 24 | 21.1 | 2.95 | 15 | 25 | ** | 0.95 |
| Working memory (composite) | 20 | 4.97 | 0.91 | 3.15 | 7.10 | 5.38 | 1.01 | 3.53 | 7.37 | * | 0.43 |
| Visuospatial skills (composite) | 20 | 4.06 | 0.84 | 2.34 | 5.15 | 4.83 | 0.72 | 3.38 | 5.93 | *** | 0.98 |
| No-contact control group | | | | | | | | | | | |
| *n*-back | 25 | 2.35 | 0.41 | 1.5 | 3.15 | 2.43 | 0.47 | 1.7 | 3.6 | 0.38 | 0.18 |
| Digit Span | 25 | 17.44 | 3.86 | 10 | 25 | 18.4 | 3.8 | 13 | 24 | 0.09 | 0.25 |
| Letter–Number Sequencing | 25 | 9.96 | 2.03 | 7 | 14 | 9.64 | 1.78 | 7 | 12 | 0.32 | −0.17 |
| Block Design | 25 | 36.92 | 8.46 | 24 | 55 | 39.84 | 9.52 | 23 | 57 | * | 0.32 |
| Matrix Reasoning | 25 | 15.76 | 4.93 | 4 | 23 | 17.04 | 5.02 | 6 | 24 | * | 0.26 |
| Working memory (composite) | 25 | 4.81 | 0.90 | 3.33 | 6.32 | 4.86 | 0.84 | 3.48 | 6.20 | 0.67 | 0.06 |
| Visuospatial skills (composite) | 25 | 3.80 | 0.87 | 1.79 | 4.95 | 4.10 | 0.92 | 2.12 | 5.72 | ** | 0.34 |

*Note*. In order to calculate the reported statistics for the composite measures, we divided the pretest values of the manifest variables by the standard deviation from the pretest session (based on all three groups) and then averaged in each domain (i.e., Digit Span and Letter–Number Sequencing for working memory and Block Design and Matrix Reasoning for visuospatial skills). The same procedure was used for the posttest statistics; *p* values are based on paired *t* tests (posttest vs. pretest). Min = minimum; max = maximum.
* $p < .05$. ** $p < .01$. *** $p < .001$.

in WM and visuospatial skills. We found that the improvement in *n*-back performance was positively related to the improvement in both, WM (Spearman $\rho = 0.32$; $p < .05$) and visuospatial skills (Spearman $\rho = 0.26$; $p < .05$).

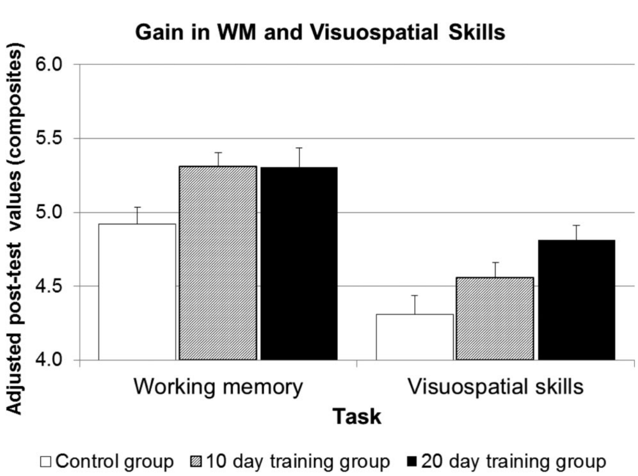


*Figure 2.* Transfer effects. Visualized are the means of the posttest scores adjusted by the effect of the pretest covariate for both composite scores. Error bars represent standard errors of the mean. Planned contrasts revealed that both experimental groups outperformed the control group in working memory and visuospatial skills, and further, that the 20-day training group outperformed the 10-day training group in the visuospatial composite.

## Discussion

The present study revealed that older adults can significantly improve performance in an *n*-back task as a result of training for a period of either 10 or 20 sessions. But more important, we observed reliable transfer to nontrained composite measures of WM and visuospatial skills, thereby replicating but also extending earlier work with children and young adults (Jaeggi et al., 2008, 2010a, 2010b, 2011). Additionally, we found a positive relationship between training gain and transfer in both criterion measures, suggesting that the intervention was the contributing factor to transfer (cf. Chein & Morrison, 2010; Jaeggi et al., 2011; von Bastian & Oberauer, 2013; Zhao, Wang, Liu, & Zhou, 2011). Finally, higher training frequency resulted in a larger gain in visuospatial skills, which is consistent with previous findings as well (Basak et al., 2008; Dahlin, Bäckman, Neely, & Nyberg, 2009; Jaeggi et al., 2008).

As pointed out earlier, it has been difficult to demonstrate evidence for transfer that goes beyond tasks that are closely related to the training task in older adults (e.g. Buschkuehl et al., 2008; Dahlin et al., 2008; Li et al., 2008; Zinke et al., 2011). Nevertheless, the present results add to the evidence for the malleability of visuospatial skills (Uttal et al., 2013) and, more specifically, to the few studies reporting transfer on visuospatial skills following WM training in older adults. But our data also extend the previous literature in important ways. For example, Schmiedek, Lövdén, & Lindenberger (2010) trained their participants on a complex training regimen that consisted of episodic memory, processing speed, and WM for a period of several months. Although Schmiedek et al. found Gf improvements on one manifest variable in their older adult population, that is, in the Raven's Advanced Progressive Matrices (APM), the improvement was not significant on a latent level, which included three subtests of the Berlin Intelligence Structure Test (BIS; Jäger, Süss, & Beauducel, 1997) in addition to the APM.[5] The authors hypothesized that the timed nature of most of the Gf tasks used worked against transfer in these tasks (cf. also Hofland, Willis, & Baltes, 1981). This hypothesis is supported in so far as their older participants showed transfer on two tasks with less time pressure, one of them being the APM. In our study, we employed tests with less obvious (BD) or no (MR) time limit, which could have been a reason why we observed transfer. However, the studies of Borella et al. (2010) and Carretti et al. (in press) do not seem to support this notion, as these authors observed increased performance in a timed Gf measure, the Cattell Culture Fair Test of Intelligence (CFT; Cattell & Cattell, 1963). One has to keep in mind, though, that these three training studies differed considerably in their methodology, especially when it comes to the employed training paradigm (training on tasks from different domains by Schmiedek et al. vs. training on a single variant of a complex span task by Borella et al., 2010, and Carretti et al., in press) but also training time (> 100 days by Schmiedek et al. versus 3 days by Borella et al. and Carretti et al.). Given these and other differences between our study and the ones discussed previously, it is impossible to conclusively determine whether the timing of Gf tasks is important for transfer effects to occur. Nevertheless, our results, as well as the results from Schmiedek et al., Borella et al., and Carretti et al., are consistent with evaluations by Schaie & Willis (1986) and Lövdén, Bäckman, Lindenberger, Schafer, and Schmiedek (2010) in which they concluded that older adults are indeed capable of improving higher cognitive processes and that there are certain cognitive interventions that seem to be able to induce plasticity in old age.

Moody (2009) argued that the transfer effects on Gf as reported in Jaeggi et al. (2008) were most likely the result of the visuospatial nature of the dual *n*-back task that was used for training. Although this argument does not consider the fact that the dual *n*-back training task entailed a verbal component as well, one could still speculate that the visuospatial part was the main contributor to the transfer effects. The present study now provides evidence that the visuospatial nature of the task is not driving the transfer to visuospatial skills since the transfer emerged by training on a task that was clearly nonvisuospatial, suggesting that the mechanisms underlying *n*-back training and transfer do not rely on material-specific processes (see also Borella et al., 2010; Carretti et al., in press).

The observed dose-response effect, together with the positive correlation of *n*-back training gain and the improvement in both criterion measures, provides strong evidence that the transfer effects indeed resulted from the intervention and not from training unrelated factors such as an expectancy bias (see also Basak et al., 2008; Chein & Morrison, 2010; Jaeggi et al., 2008, 2011). But what could be the common underlying mechanism that drives both, the improvement in WM as well as the improvement in visuospatial skills? It has been argued before that process overlap between the training and transfer task is an essential prerequisite for transfer (Dahlin et al., 2008; Jaeggi et al., 2010b; Lustig et al., 2009). Since we observed transfer in both domains, it is likely that both domains share processes with the training task, and we speculate that specific WM-related processes such as storage and manipulation are the most likely candidates since they are necessary to perform well in both domains (e.g. storing and updating the order of the letters and numbers in WM during LNS, or holding and comparing multiple solution principles in WM during MR).

The question is now why there was a dose–response effect in visuospatial skills, but not in WM. Interestingly, a similar pattern was observed in a previous study in young adults, that is, a dose–response effect in Gf after training on WM skills, as well as a WM effect irrespective of dose (Jaeggi et al., 2008). One possible explanation for this result pattern is that the WM effects might be easier to obtain due to the domain specificity, that is, due to the fact that there is a stronger process overlap between the trained task and the WM tasks; thus, the improvements in WM occur regardless of training frequency. Alternatively, older adults might reach their WM capacity limit fairly quickly (Reuter-Lorenz & Cappell, 2008; Schneider-Garces et al., 2010), that is, within 10 days of training. In contrast, there might be more room to improve visuospatial skills because performance in such tasks relies on WM processes *in addition* to other processes that do not seem to reach a capacity limit so quickly. In the cognitive domain, such processes could be an improved ability to suppress distraction, attentional control, speed of processing, or strategy changes (Jaeggi et al., 2008; 2010b; Morrison & Chein, 2011). In addition, noncognitive processes might come into play as well, such as increased confidence, self-efficacy, reduced anxiety, and persistence to stick with the task and not giving up regardless of the difficulty of the task, which is certainly a requirement to perform well in the adaptive *n*-back training task but also in the reasoning measures (e.g. Borella et al., 2010; Carretti et al., in press; Chein & Morrison, 2010; Duckworth, Peterson, Matthews, & Kelly, 2007; Hayslip, 1989; Hayslip, Maloy, & Kohl, 1995; Jaeggi et al., 2011).

Despite the clear results, we acknowledge several limitations with our current study. First is the fact that our participants were relatively young (age range: 65–75 years), which could have facilitated the occurrence of transfer (Buschkuehl et al., 2008; Singer, Lindenberger, & Baltes, 2003; Zinke et al., 2011). Further, we found that our population performed on a relatively high level from the start, which might be regarded as detrimental for the generalizability of the current data. The high pretest scores of our participants might be indicative of a high education level (Salthouse, 1993), and indeed, our sample had higher levels of education than can be found in the Czech general population in this age group ($p < .001$), suggesting a self-selection bias towards highly functioning participants. However, this seems to be a common phenomenon with intervention studies in old age (Lezak, Howieson, & Loring, 2004, p. 296). On the other hand, such high initial ability levels could also imply that there may not be as much room for improvement as there could be in lower functioning participants, thus making our findings even more impressive (cf. Zinke et al., 2011). Yet, it might be of interest for future researchers to investigate whether participants with lower education and accom-

[5] Note that the improvement on Gf on the latent level was significant in their young adult sample.

panying lower pretest scores would obtain even larger gains than those in our well-educated group. Such a prediction is not without controversy though. Previous research has found that education level is not necessarily indicative for training outcome. For example, it has been shown that gains in speed of processing did not correlate with education (Ball, Edwards, & Ross, 2007) and education did not affect training-related gains in memory either (Stigsdotter Neely & Bäckman, 1995). Verhaeghen & Marcoen (1996) even reported that individuals with high initial WM capacity show the largest gains in episodic memory as a result of strategy training. It is important to note at this point that although our sample was performing very well already in the pretest, this did not preclude them from showing transfer because there was enough room for improvement in all four manifest tasks. Unfortunately, we do not know how long our transfer effects last due to a lack of follow-up assessment; however, there are previous reports that show lasting effects of training in children (e.g. Jaeggi, et al., 2011) as well as in older adults (Borella et al., 2010; Carretti et al., in press), suggesting that such effects might be retained (but see Buschkuehl et al., 2008). Nevertheless, it seems reasonable to predict that in order to obtain maximal retention, regular booster sessions might be necessary, especially in older adults (Ball et al., 2007; Lövdén et al., 2010). Finally, the lack of an active control group can be seen as a limiting factor. It has been argued that no-contact control groups do not control for the motivation of participants, which may lead to suboptimal performance and may wrongly magnify an intervention effect (Shipstead, Redick, & Engle, 2012); however, it has also been argued that no-contact control groups are adequate to control for test-retest effects (Chein & Morrison, 2010; see also Silverman & Hinshaw, 2008), and furthermore, studies that have used both placebo and no-contact control groups did not observe any significant group differences in motivation (Bergman Nutley et al., 2011). In our case, the no-contact control group performed as well as the experimental groups at pretest and also showed healthy test–retest effects. Therefore, we assume that even participants in the control group tried hard to achieve their best performance in the pre- and posttest sessions. Furthermore, the fact that we used an unsupervised training procedure rules out an unspecific experimenter effect that could indeed work against our hypothesis, as there is literature showing that (supervised) group sessions (i.e., socializing) can have a positive impact on cognitive performance (Verhaeghen et al., 1992; Ybarra et al., 2008). Thus, the fact that we actually *did* observe improvement in training (along with transfer effects) suggests that our effects go beyond unspecific effects of experimenter presence or socializing. In addition, as we pointed out earlier, the dose–response relationship as observed in visuospatial ability, as well as the positive relationship between training gain and transfer in general, suggests that the effects were most likely a specific result of the intervention. Finally, we would like to acknowledge that we used the same test versions in the pre- and posttest sessions. Unfortunately, there are no parallel-test versions available for the standardized clinical tests that we used here, and rather than splitting the tests in half and thereby reducing their excellent psychometric characteristics, we decided to use the same tests for both test occasions, a procedure that has been used by others before (cf. also Jobe et al., 2001; Klingberg et al., 2005; Rueda, 2005; Schellenberg, 2004; Schmiedek, Lövdén, & Lindenberger, 2010; von Bastian & Oberauer, 2013).

To conclude, our data demonstrate generalizing effects to composite scores reflecting WM and visuospatial skills in young-old healthy adults after a verbal *n*-back intervention. Furthermore, we observed a dose–response effect in that those participants who completed more training sessions showed more performance gain on visuospatial skills. Our work adds to the accumulating evidence for transfer effects in old adults by means of an easily accessible noncommercial computer-based program that can be used independently at home. Such unsupervised interventions are very important from an applied point of view and are very economical as they are cost-effective and only require a minimal amount of personnel for administration. Future projects may aim to further investigate the breadth of transfer by including a broader variety of tests, which would help to identify domain-specific and domain-general factors. Further, it is important to validate the reported effects on larger and more heterogeneous samples by including participants with a wider age and ability range in order to make further claims about the generalizability of WM interventions.

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