Training and transfer effects of N-back training for brain injured and healthy subjects.

Jonas Kristoffer Lindeløv^{1*}, MSc

Jonas Olsen Dall¹, MSc

Casper Daniel Kristensen¹, BSc

Marie Holt Aagesen¹, BSc

Stine Almgren Olsen¹, BSc

Therese Ruud Snuggerud¹, BSc

Anna Sikorska¹, BSc

Affiliations: Aalborg University, Department of Communication and Psychology

Corresponding author: Jonas Kristoffer Lindeløv, jonas@id.aau.dk, +4540331097.

Counts: 4288 words, 4 figures, and 3 tables.

Running title: No cognitive improvement from N-back training.

Keywords: cognitive rehabilitation, N-back, cognitive transfer, computer

Data, analysis, and supplementary materials are available at <https://osf.io/ftxip/>

Abstract:

Working Memory impairments are prevalent among patients with acquired brain injury (ABI). Computerized training targeting working memory has been researched extensively using samples from healthy populations but this field remains isolated from similar research in ABI patients. We report the results of an actively controlled randomized controlled trial in which 17 patients and 18 healthy subjects completed training on the N-back task. The healthy group had superior improvements on both training tasks (SMD=6.1 and 3.3) whereas the ABI group improved much less (SMD=0.5 and 1.1). Neither group demonstrated transfer to untrained tasks. We conclude that computerized training facilitates improvement of specific skills rather than high-level cognition in healthy and ABI subjects alike. The acquisition of these specific skills seems to be impaired by brain injury. The most effective use of computer-based cognitive training may be to make the task resemble the targeted behavior(s) closely in order to exploit the stimulus-specificity of learning.

Introduction

Randomized controlled studies on computer-based cognitive rehabilitation of brain injured patients goes back to at least Sturm et al. (1983) and more than 50 RCTs have been published so far (Chen, Thomas, Glueckauf, & Bracy, 1997; Lindeløv, in review). There is a much larger and growing literature on computerized cognitive training in healthy subjects (Melby-Lervåg & Hulme, 2012; Morrison & Chein, 2010; Shipstead, Redick, & Engle, 2012). However, these two fields have hitherto proceeded in parallel with no cross-talk or direct comparisons. Computerized cognitive neurorehabilitation could potentially expand its evidence base considerably if there are points of convergence to healthy subjects. After all, all subjects are humans and all would benefit from improved information processing capacities.

Unfortunately, enhancement of domain-general cognitive functions such as working memory and attention has proven difficult in brain injured (Lindeløv, in review; Park & Ingles, 2001) as well as healthy subjects (Melby-Lervåg & Hulme, 2012; Shipstead et al., 2012) where the sum of evidence show a dominance of domain-specific effects of rehabilitation efforts. In other words, there is relatively little transfer to untrained material and contexts. Still, there is an ongoing search for interventions which could promote far transfer, one of which is variations of computer-based cognitive rehabilitation. Efforts have been made to identify 'active ingredients' that promotes far transfer. *Adaptiveness* have generally been found to contribute positively in healthy subjects (Jaeggi et al., 2010; Klingberg, 2010; Morrison & Chein, 2010) with non-adaptive conditions now being used as active control groups (see e.g. Holmes, Gathercole, & Dunning, 2009) although adaptiveness in and of itself is not sufficient as demonstrated by several null-results (Chooi & Thompson, 2012; Jaeggi, Buschkuhl, Jonides, & Shah, 2011; Redick et al., 2012). Other factors such as training intensity, duration, and spacing of sessions could be influential but the results are ambiguous (Morrison & Chein, 2010).

There is a general distinction between training effects and transfer effects. This distinction is also known as domain-specific vs. domain-general and near-transfer vs. far-transfer. For example, working memory is said to be domain-general because it operates cross-modally on a wide range of stimuli and contexts (Baddeley, 2007; Cowan, 1988; Kane et al., 2004). An improvement of working memory would then by definition lead to an improvement on all behaviors which rely on working memory. If that improvement was brought about by training only a subset of behaviors, such an effect would be far transfer. In contrast, domain-specific processes apply to a narrow range of stimuli and contexts and the challenge for all studies is to provide convincing evidence that observed improvements are not mere training effects. For example Westerberg et al. (2007) and Lundqvist, Grundström, Samuelsson, & Rönnerberg (2010) administered CogMed training to ABI patients and observed improvements on digit span and a spatial span tasks but both were a part of the training program. Therefore, these results could be attributed to mere training effects even though they were interpreted as transfer to working memory as such. Others explicitly assess the testing-training similarity and find narrow transfer (Walter Sturm & Willmes, 1991). In a rehabilitation setting, the primacy of training effects over transfer effects supports a preference for compensatory interventions to remedial interventions.

In the present study we administered an N-back training procedure to a healthy sample and an ABI sample. The N-back has been used in numerous studies in healthy subjects with initial large positive transfer (Jaeggi et al., 2010; Jaeggi, Buschkuhl, Jonides, & Perrig, 2008) but later studies have yielded null results (Chooi & Thompson, 2012; Jaeggi et al., 2011; Redick et al., 2012). An N-back-like intervention has only been administered to ABI patients in a small study by Cicerone (2002) which yielded very large positive effects on untrained tasks in the order of $d > 2$. The Cicerone study was heavily therapist-directed and included efforts to transfer to the patient's everyday life.

From this, we derived three research questions: (1) can the Cicerone findings be attributed to

just doing an adaptive n-back task? (2) does the n-back task promote transfer in healthy subjects?
(3) To what extent can evidence be generalized from healthy subjects to ABI patients and vice versa
- at least for the N-back task?

Methods

Sample

We recruited 39 in-hospital ABI patients at Hammel Neurorehabilitation Centre and University Research Clinic. The choice of in-hospital patients was motivated by the fact that cognitive impairments reduce the intensity and duration of other in-hospital rehabilitation efforts. Therefore, an early cognitive improvement has the potential to support neurorehabilitation in general. Patients engaged in the training in addition to treatment as usual at a time of their own choosing, usually in the late afternoon. 17 patients completed (see Figure 1). Additional inclusion criteria were (1) no aphasia, deafness, blindness or other disabilities which would prevent testing and training, (2) the patient should be able perform reasonably ($d' > 1$) at N-back level 1 and Visual Search level 2 at the time of recruitment and (3) and that the training did not interfere with the standard treatment as judged by the patient's primary therapists.

We recruited 39 healthy participants who trained in their free time using their own personal computer. They were predominantly psychology students recruited using flyers and posts on virtual forums. Eighteen completed (see Figure 2). See Table 1 for descriptives on the recruited and final sample.

This study was approved by the local ethics committee, all participants signed informed consent and participation was voluntary. Participants were not reimbursed. Patients were informed that the training was designed to facilitate their cognitive recovery and healthy subjects were

informed that it was designed to boost their intelligence.

	Group	Included	Finished	N-back	Visual Search
Males / Females	ABI Healthy	31 / 8 18 / 21	13 / 4 8 / 10	6 / 2 3 / 6	7 / 2 5 / 4
Age in years	ABI Healthy	53.3 (10.4) 27.4 (10.3)	56.1 (6.3) 29.3 (11.3)	56.1 (5.6) 29.2 (11.1)	56.1 (7) 29.4 (11.9)
Days since injury	ABI	54 [28-94]	57 [33-95]	63 [35-89]	55 [33-95]
FIM cognitive^a (0-35)	ABI	24 [22-28]	26 [23-28]		
FIM motor^a (0-91)	ABI	82 [60-89]	82 [67-89]		

Table 1. Sample descriptives at baseline for each group as mean (SD) where normally distributed and frequencies [square brackets] where not. Included column are intention-to-treat participants. Finished column are participants who eventually finished. N-back and Visual Search columns subdivides the finished columns to the two treatments. ABI=Acquired Brain Injury group. FIM = Functional Independence Measure. ^aFIM scores at baseline were only available for 18 patients and should therefore be regarded as a rough indication of the patient group's functional level rather than a sample descriptive. It is not shown for individual treatments since there were only 4 of the patients in each group who finished that had a baseline FIM score.

Design and randomization

This is a parallel-groups design with a 2 (ABI/healthy) x 2 (N-back/Visual Search) design resulting in four treatment arms. Participants entered the study continuously and were pseudo-randomly allocated to N-back and VS so that pre-test scores on Raven's Advanced Progressive Matrices (see Outcome Measures) and ages were balanced. This allocation took place independently

for patients and healthy subjects and allocation of the first four participants within each of these groups was truly random. When 20 training sessions were completed, participants were scheduled for a post-test.

Figure 1. *Study design and flowchart for the patient group.*

Figure 2. Study design and flowchart for the healthy group.

N-back training

The single N-back task consisted of a series of stimuli presented at 3 second intervals. The participants were instructed to press a key when the presented stimulus was identical to the stimulus N back in the sequence. There were 25 % targets per block and at most two consecutive targets. In order to prevent the formation of stimulus-specific strategies there were a total of 137 different stimuli as shown in Figure 1: 3 types of audio and 4 types of visual. A random selection of 8 stimuli from a randomly selected stimulus type was chosen for each block (See Figure 3).

Figure 3. Top left: 4 trials of the N-back task illustrated at 2-back level with image stimuli. Top right: seven different stimulus types. Bottom: 4 trials of the Visual Search task at level 4 (4 x 4 grid) and with "E" as a target. For each block any of the six different shapes would be picked randomly as target. Participants were instructed to press a key on target trials.

Visual Search training

Participants were instructed to press a key if a target symbol was present in an $N \times N$ array of symbols. The target symbol changed from block to block but there were just 6 different symbols. The Visual Search (VS) task is unrelated to working memory (Kane, Poole, Tuholski, & Engle, 2006) and served as an active control condition. It has served this purpose in other training studies (Harrison et al., 2013; Redick et al., 2012). During training levels increased from $N=1,2,3,4...$ etc. but we will use the number of items to be searched ($N=1,4,9,16,...$) as a measure of difficulty level throughout in this paper.

Both training tasks

Tasks were kept similar in all other respects to maximize the purity of the contrast. Participants trained 12 blocks of $20 + N$ trials on an adaptive N-back task for 20 days. Less than 10 blocks was considered an incomplete training day. Interstimulus interval was 3 seconds. Thus the full intervention consisted of 4.4 hours of constant training, breaks not included. Participants trained unsupervised in a laptop web browser.

Visual correct/incorrect feedback was given on response (hit/false alarm) and on misses in the end of every trial. To increase motivation, participants were awarded points after each block and given progressively more attractive titles. Feedback sounds were played on level upgrade or downgrade. In addition, participants would see a graph of their own progress.

The difficulty level was adjusted after each block based on d-prime from Signal Detection Theory, i.e. the participant's ability to discriminate target trials from non-target trials. The level was downgraded if d-prime was below 1.2 and upgraded if d-prime was above 1.8.

All participants were given graphic instructions to the tasks and were telephonically encouraged to start training if they had two successive non-training weekdays. Patients initially trained three blocks under the guidance of a research assistant. All participants could call for technical help or to get an update on the task instructions.

Outcome measures

We investigate the hypothesis that n-back training improves working memory and perhaps fluid intelligence while visual search improves processing speed. The two intervention groups serve as each other's controls in this respect since the search task is unrelated to working memory (Kane et al., 2006) and signal detection in the n-back task is relatively unrelated to processing speed (Conway, Cowan, Bunting, Theriault, & Minkoff, 2002).

Participants were tested on the following measures before and after training. Although each is related to many cognitive abilities, they are grouped according to the cognitive labels usually assigned to them:

1. *Fluid intelligence*: Equal and unequal items from the Raven's Advanced Progressive Matrices (RAPM) was administered at pretest and posttest in counterbalanced order. Participants were given 10 minutes to solve the 18 items in each set. RAPM has excellent construct validity with respect to fluid intelligence as determined by latent variable analysis (Engle, Tuholski, Laughlin, & Conway, 1999).
2. *Working Memory*: the WAIS-IV Working Memory Index (WMI), calculated from forwards/backwards/ordered digit span, mental arithmetic and letter-number sequencing. The latter is optional and was skipped for some fatigued patients.
3. *Working Memory*: A computerized Operation Span (Unsworth, Heitz, Schrock, &

Engle, 2005) with 3 x span 2-4. Span 2-5 is standard for this test but span 5 was omitted in this experiment since all four pilot ABI patients experienced great distress from these trials to a degree where they gave up in advance during recall. Each participant was scored using the partial credit unit scoring method (Conway et al., 2005) which is the average proportion of items recalled in the correct location in each trial.

4. *Processing Speed*: the WAIS-IV Processing Speed Index (PSI), calculated from symbol search and digit-symbol coding. PSI and WMI have high internal consistencies and retest reliabilities (Iverson, 2001).

5. *Processing speed with inhibition*: 180 trials on a computerized Stroop task of which 20 % were incongruent. Participants responded verbally to maximize interference (Liotti, Woldorff, Perez III, & Mayberg, 2000) while pressing a key to register reaction time. The Stroop effect is classically regarded as a measure of inhibition task but the raw reaction time and to some extent also the Stroop effect itself aligns well with a processing speed construct (Salthouse & Meinz, 1995; Verhaeghen & De Meersman, 1998). Both are of interest with respect to the visual search training.

The operation span test and the stroop test were computerized using PsychoPy v. 1.79 (Peirce, 2007) and administered in a separate test session.

Statistical models and inferences

Outcome data was modeled as mixed models with main effects of time, treatment and group and their interactions in R 3.1.2 using the lmer (Bates, Maechler, Bolker, & Walker, 2014) 4.1.1 and BayesFactor (Morey & Rouder, 2015) 0.9.10 packages. There was a random intercept per participant to account for correlations between repeated measures. Inference was based on model selection between a full model and a null model. The null model was the full model less the fixed

effect in question, e.g. the three-way interaction.

Training data was modeled using a power function ($N = k + a x^b$) for task level (N) as a function of time (block number) with random intercepts (k) per participant and random a and b parameters for intervention and group respectively to reflect differences in gain.

p-values from the chi-square statistic of a likelihood ratio tests (LRT) (Barr, Levy, Scheepers, & Tily, 2013) were reported to comply with current publishing practices. It has long been known that p does not quantify evidence for/against the null and therefore has poor inferential value (Berger & Sellke, 1987; Sellke, Bayarri, & Berger, 2001; Wetzels et al., 2011). Bayes factors

(BF) with a relatively uninformative Cauchy($0, \frac{\sqrt{2}}{2}$) prior on each covariate were used to quantify the relative evidence for each model (Rouder & Morey, 2012) with the exception of the power function where a more uninformative unit information prior (Wagenmakers, 2007) was used for computation convenience. A Bayes Factor is the odds ratio between two models. For example, BF=5 means that this data shifts the odds 5:1 in favor of the full model and simultaneously shifts the odds $5^{-1}=0.2$ for the null.

Results

Training tasks

See how participants progressed on the training in Figure 4 and effect sizes in Table 2. The participants mostly trained on consecutive days: 72 % of the training sessions were on consecutive days and 93 % within three days. Block number was used as time unit with a total of 200-240 blocks for completers (20 days x 12 blocks per day). The power fits for individual participants are superposed on the data in Figure 4. This model was by far preferred to an intercept-only model for

all four group x treatment cells ($p_{LRT} \ll 0.001$, $BF_{BIC} \gg 1000$) providing evidence that there was improvement on the training task in all conditions. I.e. the power model is much more than 1000 times more likely to have generated the data than the intercept-only model. This is of crucial theoretical importance since it establishes the training as a potential source of transfer to the outcome measures. It was also preferred to a linear model with the same random-effects structure ($p_{LRT} \ll 0.001$, $BF_{BIC} \gg 1000$).

It is apparent from Figure 4 that the healthy group had a much greater numerical improvement on the training tasks than the ABI group. This was confirmed by a substantial group-specific testing the random effects of group on a and b ($p_{LRT} \ll 0.001$, $BF_{BIC} \gg 1000$).

Figure 4. Level of training task as a function of block number (out of 240 in total) for completers in

each group by task. The ABI group was consistently at a lower level and improved little on the training tasks whereas the healthy group improved on both tasks. The thick black line is the average of the predictions from a fitted power function. The thin gray line and the gray area are the means and 95% bootstrapped confidence intervals for the mean level at each block number. The transparent ‘+’ symbols are the data from individual participants which the above represents. Two healthy completers improved to N=27 and N=45 on the N-back task and were not plotted. The break in the power function around block 200 is caused by participants who stopped training there. Plots for all participants are available in supplementary Figure S1.

	N-back			Visual Search		
group	pre	post	SMD	pre	post	SMD
ABI	2.1 (1)	2.6 (0.8)	0.45	13.9 (8.1)	22.5 (11.9)	1.06
Healthy	2.7 (0.8)	7.5 (3.5)	6.11	20.6 (8.7)	49.7 (24.3)	3.34

Table 2. Pre- and post-test means, standard deviations and standardized differences for task level N. “pre” is computed from the the first 12 blocks starting from the first block where the participant did not increase a level. “post” was is computed from the last 12 blocks. Given the large sample of blocks, even very small effects would trivially would fall under the full model ($p_{LRT} < 0.001$ and $BF_{BIC} > 1000$) so we consider effect sizes is the most informative statistic here.

Outcome measures

See Table 3 for descriptives and inferences on outcome measures. A difference in effect between the Visual Search training and the N-back training was not supported by the data on any outcome measure, neither in the ABI group nor the healthy group. All Bayes Factors favored the

model without the treatment x time interaction with a BF of about 2:1 against the full model. This BF is suggestive but too weak to say anything definitive about the direction of the effect as it still puts around 1/3 posterior probability on the full interaction model. Furthermore, the controlled standardized mean difference ($SMD_c = SMD_{nback} - SMD_{search}$) were in the zero-to-small range (around 0.0 to 0.3) with no signs of a differential effect between single tests or cognitive domains.

Interestingly, the data suggests that the ABI and healthy group did not differ in their gains on any outcome as revealed by inferences on the group x time x test interaction term (see Table 3). Bayes Factors again favored the null more than the alternative and no interactions were statistically noticeable ($p > 5\%$).

As expected, an analysis of all 39 ABI patients and 39 healthy subjects at baseline revealed that the patients performed worse than the healthy subjects on all outcome measures (all ps below 0.01 and all BFs above 3) except for Operation Span where, surprisingly, patients performed better than the healthy subjects (mean difference=10.9 %, CI=0.6 – 21.2 %, $p_{ttest}=0.038$, BF=1,9). The strength of the evidence for the latter is, however, anecdotal – especially in light of the other results.

test	group	N-back				Visual Search				time x treat			ti x tr x grp	
		pre	sd	post	sd	pre	sd	post	sd	SMD	p	BF_g	p	BF_g
digit span	abi	7.25	4.53	8.38	4.17	7.78	3.19	8.22	2.99	0.18	0.41	1.92*		
	healthy	10.44	3.43	10.67	2.83	10.50	2.39	9.88	2.30	0.29	0.32	1.74*	0.89	2.34*
arithmetic	abi	8.75	3.69	9.88	4.02	9.88	3.27	9.75	2.76	0.37	0.14	1.22*		
	healthy	9.56	2.70	10.00	2.50	9.75	2.87	10.25	2.87	-0.02	0.92	2.49*	0.2	1.21*
num-let	abi	10.67	5.09	12.00	4.10	9.38	2.33	9.88	2.10	0.23	0.58	2.26*		
	healthy	8.56	2.46	10.00	3.16	9.50	3.02	9.62	3.11	0.49	0.09	1.07	0.76	2.05*
WMI index	abi	88.00	22.06	92.75	23.29	91.56	16.19	89.67	14.75	0.36	0.14	1.17*		
	healthy	96.78	15.75	101.44	15.39	99.22	18.34	101.22	14.77	0.16	0.43	1.94*	0.47	2.08*
s. search	abi	7.12	2.03	8.25	1.83	7.00	2.40	7.56	2.60	0.26	0.36	1.89*		
	healthy	11.56	2.55	13.00	4.30	11.88	3.14	13.62	3.70	-0.11	0.81	2.41*	0.53	1.92*
coding	abi	6.88	3.23	8.12	2.53	7.00	2.62	8.25	2.43	0	1	2.42*		
	healthy	11.89	3.06	13.89	2.98	10.62	3.46	12.75	3.85	-0.04	0.88	2.16*	0.91	2.94*
PS index	abi	84.12	13.86	92.00	10.10	83.00	14.00	87.00	14.90	0.29	0.23	1.66*		
	healthy	109.56	15.47	121.00	15.27	107.00	13.87	119.00	18.73	-0.04	0.88	2.34*	0.38	1.93*
RAPM	abi	3.75	2.66	3.25	2.71	4.11	1.90	4.11	1.36	-0.23	0.59	2.33*		
	healthy	8.89	2.80	10.67	2.50	8.78	4.76	9.89	3.79	0.18	0.66	2.37*	0.51	2.29*
OSPAN	abi	74.69	21.94	74.07	15.16	71.30	9.15	70.52	19.80	0.01	0.99	2.36*		
	healthy	59.26	16.85	64.40	20.30	65.02	10.35	69.75	12.42	0.03	0.94	2.38*	0.98	2.28*
log(stroop)	abi	0.42	0.13	0.35	0.11	0.42	0.03	0.39	0.05	-0.52	0.44	1.68*		
	healthy	0.31	0.12	0.28	0.12	0.26	0.07	0.25	0.12	-0.17	0.7	2.4*	0.84	1.88*
log(str RT)	abi	6.89	0.04	6.94	0.09	6.97	0.11	6.95	0.04	0.7	0.33	1.42*		
	healthy	6.52	0.27	6.41	0.29	6.66	0.19	6.54	0.11	0.03	0.93	2.6*	0.65	1.87*

Table 3. (OBS: this should probably be printed on a landscape page). Means, standard deviations and univariate inferential results for all outcome measures. n is the number of completers for this group $\times$ treatment cell. SMD_c is the controlled standardized effect size, computed using pretest standard deviation. p -values are from the likelihood-ratio test of the critical interaction and BF_g =Bayes Factor with g -priors on regression coefficients (Rouder & Morey, 2012). * asterix indicates that the BF_g is in favor if the null ($1/BF_g$). Briefly, “time $\times$ treat” answers the question: “is the tasks associated with different gains for this group?” and “time $\times$ treat $\times$ group” answers the question “is the controlled N-back gain different between groups?”. Stroop reaction times are reported in logarithmic units since they were approximately log-normal.

Discussion

We observed an improvement on the training task but no transfer to untrained tasks. Therefore, with respect to our first research question, the positive findings of Cicerone's (2002) n-back intervention on ABI patients do not seem to be attributable to the n-back task itself. In light of the present results, the Cicerone results are more likely to have arisen from the therapist-directed activities tailored to each individual patient. Such activities were absent in the present study.

With respect to our second research question about the N-back literature on healthy adults, these results fail to replicate some positive findings by Jaeggi and colleagues (2008; 2010) but are in line with several null findings (Chooi & Thompson, 2012; Jaeggi et al., 2011; Redick et al., 2012). The former studies used passive control groups while the latter used active control groups. It is possible that the Jaeggi et al. results were caused by nonspecific factors in the training, such as expectation and motivation, which cannot be attributed to the task itself. If that is the case, these studies do not constitute evidence for a transfer effect.

The remainder of the discussion pertains to our third research question: To what extent can evidence be generalized from healthy subjects to ABI patients and vice versa?

Convergence: computerized training yield specific effects.

N-back and Visual Search training did not differentially improve performance on neuropsychological tests which are thought to reflect working memory, processing speed or fluid intelligence. Since previous research has shown that N-back performance reflects working memory and that VS does not (Kane et al., 2006) and we observe no selective effect of N-back improvement on working memory measures, we conclude that participants developed specific strategies to solve the N-back task during the course of training.

How specific? Answer: So specific that N-back training on digits did not transfer to digit span (part of WAIS Working Memory Index), N-back training on letters did not transfer to Operation Span letter recall as was also found by Jaeggi et al. (2010) and Redick et al. (2012), Visual Search training did not transfer to Symbol Search (part of WAIS processing speed index) and N-back training on locations in a 3 x 3 grid did not transfer to the RAPM 3 x 3 grid as opposed to Jaeggi et al. (2008; 2010). These findings resonate with other computer-based rehabilitation studies on ABI patients. For example, Åkerlund, Esbjörnsson, Sunnerhagen, & Björkdahl (2013) who found a delayed positive effect on one trained outcome measure on working memory but not three other untrained outcome measures as compared to the control group. Lundqvist et al. (2010) similarly demonstrated improvements on measures which resembled the training, although a missing analysis of interaction with the control group renders it unassessed to what extent these improvements were superior to the improvements in the control group. Possibly the most explicit assessment of near- versus far-transfer effects was carried out by Walter Sturm & Willmes (1991) who ordered the outcome measures according to their hypothesized similarity with the training. Here too, a specific-

effects dominance was observed with no transfer to dissimilar outcome measures.

It is clear that these data pose problems for the naïve view that abstract cognitive abilities were acquired during the training and even the view that something intermediate was trained, such as recalling sequential items or scanning visual grids. Instead, we believe that computerized training follows the well-known learning principle that the efficiency of the decoding/use of a skill is proportional to the similarity of the decoding context to the encoding/learning context of this skill (Perkins & Salomon, 1989; Tulving & Thomson, 1973). The repetitive nature of computerized training make up a highly stable context and thus the N-back and VS skills become “locked” to this context in a way that does not generalize to a neuropsychological test setting or even the computerized tests.

This is evidence that the development of specific strategies that do not transfer to untrained tasks might be a possible point of convergence between healthy subjects- and ABI patients. Although specific improvement may seem unflattering compared to generalized improvement, it could actually be thought of as a very efficient information processing strategy where the cheap local strategies are preferred to the slow and costly high-level cognition (Clark, 2013; Friston, 2010). As such, the tendency and ability to develop specific strategies could be regarded as a property of a healthy cognitive system.

Divergence: the formation of specific skills.

The healthy group improved 2.5-5.5 SMD more on the training tasks than the ABI group. We interpret the training data to reflect an impaired ability to form specific strategies in the ABI group. One explanation for this observation is that well-functioning domain-general cognition is necessary for the effective formation of specific strategies. However, 5 out of 17 ABI patients had a baseline Working Memory Index score over 100 and two patients had a Processing Speed Index

score over 100. But neither of these improved nearly as much on the training task as the average person in the healthy group, thus discrediting this hypothesis.

The group differences in etiologies could be confounded by the age difference. However, Dahlin, Nyberg, Bäckman, & Neely (2008) did computerized training on healthy young and elderly adults and found no difference in gain on the training task. A meta-analysis on 26 computer-based parallel-groups RCT on ABI patients similarly found no effect of age on improvement (Lindeløv, in review). Thus both within-study and between-study evidence discredits age as the sole explanation for the observed discrepancy between the ABI and the healthy group.

This leaves us in a limbo with no single candidate explanation for the observed difference between groups. There is a vast literature on the topic of specific learning impairments following acquired brain injury (Schmitter-Edgecombe, 2006; Vanderploeg, Crowell, & Curtiss, 2001). However, it has almost exclusively investigated verbal and motor learning within single sessions. Four weeks of training on the N-back task and the Visual Search tasks do not readily subsume under these categories so the present study may contribute new evidence. It is up to future studies to narrow in on the mechanisms driving this effect. To support this effort, we encourage authors to report and interpret training data explicitly when doing this kind of study.

Limitations

The final sample size per condition is small even though a total of 78 participants initiated the training. The present study should not be considered as basis for clinical guidelines but rather as preliminary evidence which puts a few ideas about the mechanisms underlying computer based training on the table. Even then, is not smaller than the two most cited studies on the topic (Walter Sturm, Willmes, Orgass, & Hartje, 1997; Westerberg et al., 2007).

The sample size was influenced by a large dropout rate of 50% which was not biased with

respect to age, gender, baseline scores or (for patients) FIM score. The dropout rate suggests that low adherence should be expected for fully self-initiated training without monetary reward for healthy subjects and ABI patients alike. Lack of motivation (too hard/boring) was the primary reason for drop-out. Thus the final sample might be biased towards higher motivation and consequently the effect sizes reported here might be somewhat positively biased.

As with any experiment, the results could be attributed to the specificities of the treatments. In particular, 20 minutes of unsupervised training per day is relatively short. One study which had more than 500 1-hour therapist-assisted training sessions showed no generalized improvement (Middleton, Lambert, & Seggar, 1991), demonstrating that more training does not necessarily promote transfer.

The optimal evidence support for the conclusions above would have been obtained if the ABI group and the healthy group had been matched on all nuisance parameters such as age, education and socioeconomic status.

Future directions

We suggest that future research on computerized cognitive rehabilitation may progress along two different routes: (1) Prevent specific learning. This is not easy. Simply training on a large array of different tasks may not be sufficient as demonstrated by several null findings who used this strategy (Chen et al., 1997; Middleton et al., 1991). A true context-breaking intervention would constantly present novel problems, shifts between devices, change colors, be trained at different locations etc. We expect that this approach is too chaotic to be feasible with ABI patients. Alternatively, (2) exploit the context-specific effects and make the training task as similar to the transfer target as possible, i.e. practice reading television subtitles, doing mental arithmetic on

shopping costs etc. For example, Yip & Man (2013) successfully improved real-life shopping performance after training in a matching virtual reality environment. This is a much less ambitious target than high-level cognition but may also be more realistic.

References

- Åkerlund, E., Esbjörnsson, E., Sunnerhagen, K. S., & Björkdahl, A. (2013). Can computerized working memory training improve impaired working memory, cognition and psychological health? *Brain Injury*, 27(13-14), 1649–1657. <http://doi.org/10.3109/02699052.2013.830195>
- Baddeley, A. (2007). *Working Memory, Thought, and Action* (1st ed.). Oxford University Press, USA.
- Barr, D. J., Levy, R., Scheepers, C., & Tily, H. J. (2013). Random effects structure for confirmatory hypothesis testing: Keep it maximal. *Journal of Memory and Language*, 68(3), 255–278. <http://doi.org/10.1016/j.jml.2012.11.001>
- Bates, D., Maechler, M., Bolker, B., & Walker, S. (2014). *lme4: Linear mixed-effects models using Eigen and S4*. Retrieved from <http://CRAN.R-project.org/package=lme4>
- Berger, J. O., & Sellke, T. (1987). Testing a Point Null Hypothesis: The Irreconcilability of P Values and Evidence. *Journal of the American Statistical Association*, 82(397), 112. <http://doi.org/10.2307/2289131>
- Chen, S. H. A., Thomas, J. D., Glueckauf, R. L., & Bracy, O. L. (1997). The effectiveness of computer-assisted cognitive rehabilitation for persons with traumatic brain injury. *Brain Injury*, 11(3), 197–210.
- Chooi, W.-T., & Thompson, L. A. (2012). Working memory training does not improve intelligence in healthy young adults. *Intelligence*, 40(6), 531–542. <http://doi.org/10.1016/j.intell.2012.07.004>
- Cicerone, K. D. (2002). Remediation of “working attention” in mild traumatic brain injury. *Brain Injury*, 16(3), 185–195. <http://doi.org/10.1080/02699050110103959>
- Clark, A. (2013). Whatever next? Predictive brains, situated agents, and the future of cognitive

science. *Behavioral and Brain Sciences*, 36(03), 181–204.

<http://doi.org/10.1017/S0140525X12000477>

Conway, A. R. A., Kane, M. J., Bunting, M. F., Hambrick, D. Z., Wilhelm, O., & Engle, R. W.

(2005). Working memory span tasks: A methodological review and user's guide.

Psychonomic Bulletin & Review, 12(5), 769–786.

Conway, A. R. ., Cowan, N., Bunting, M. F., Theriault, D. J., & Minkoff, S. R. . (2002). A latent

variable analysis of working memory capacity, short-term memory capacity, processing speed, and general fluid intelligence. *Intelligence*, 30(2), 163–183.

[http://doi.org/10.1016/S0160-2896\(01\)00096-4](http://doi.org/10.1016/S0160-2896(01)00096-4)

Cowan, N. (1988). Evolving conceptions of memory storage, selective attention, and their mutual

constraints within the human information-processing system. *Psychological Bulletin*, 104(2), 163–191. <http://doi.org/10.1037/0033-2909.104.2.163>

Dahlin, E., Nyberg, L., Bäckman, L., & Neely, A. S. (2008). Plasticity of executive functioning in

young and older adults: Immediate training gains, transfer, and long-term maintenance.

Psychology and Aging, 23(4), 720.

Engle, R. W., Tuholski, S. W., Laughlin, J. E., & Conway, A. R. A. (1999). Working memory, short-

term memory, and general fluid intelligence: A latent-variable approach. *Journal of Experimental Psychology: General*, 128(3), 309–331. [http://doi.org/10.1037/0096-](http://doi.org/10.1037/0096-3445.128.3.309)

[3445.128.3.309](http://doi.org/10.1037/0096-3445.128.3.309)

Friston, K. (2010). The free-energy principle: a unified brain theory? *Nature Reviews Neuroscience*,

11(2), 127–138. <http://doi.org/10.1038/nrn2787>

Harrison, T. L., Shipstead, Z., Hicks, K. L., Hambrick, D. Z., Redick, T. S., & Engle, R. W. (2013).

Working Memory Training May Increase Working Memory Capacity but Not Fluid Intelligence. *Psychological Science*, 24(12), 2409–2419.

<http://doi.org/10.1177/0956797613492984>

Holmes, J., Gathercole, S. E., & Dunning, D. L. (2009). Adaptive training leads to sustained

enhancement of poor working memory in children. *Developmental Science*, 12(4), F9–F15.

<http://doi.org/10.1111/j.1467-7687.2009.00848.x>

- Iverson, G. L. (2001). Interpreting change on the WAIS-III/WMS-III in clinical samples. *Archives of Clinical Neuropsychology*, 16(2), 183–191.
- Jaeggi, S. M., Buschkuhl, M., Jonides, J., & Perrig, W. J. (2008). Improving fluid intelligence with training on working memory. *Proceedings of the National Academy of Sciences*, 105(19), 6829–6833.
- Jaeggi, S. M., Buschkuhl, M., Jonides, J., & Shah, P. (2011). Short- and long-term benefits of cognitive training. *Proceedings of the National Academy of Sciences*, 108(25), 10081–10086. <http://doi.org/10.1073/pnas.1103228108>
- Jaeggi, S. M., Studer-Luethi, B., Buschkuhl, M., Su, Y. F., Jonides, J., & Perrig, W. J. (2010). The relationship between n-back performance and matrix reasoning—implications for training and transfer. *Intelligence*, 38, 625–635.
- Kane, M. J., Hambrick, D. Z., Tuholski, S. W., Wilhelm, O., Payne, T. W., & Engle, R. W. (2004). The Generality of Working Memory Capacity: A Latent-Variable Approach to Verbal and Visuospatial Memory Span and Reasoning. *Journal of Experimental Psychology: General*, 133(2), 189–217. <http://doi.org/10.1037/0096-3445.133.2.189>
- Kane, M. J., Poole, B. J., Tuholski, S. W., & Engle, R. W. (2006). Working memory capacity and the top-down control of visual search: Exploring the boundaries of “executive attention”. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 32(4), 749–777. <http://doi.org/10.1037/0278-7393.32.4.749>
- Klingberg, T. (2010). Training and plasticity of working memory. *Trends in Cognitive Sciences*, 14(7), 317–324. <http://doi.org/10.1016/j.tics.2010.05.002>
- Lindeløv, J. (in review). Computer-based cognitive rehabilitation following acquired brain injury: a meta-analysis of transfer effects. *Clinical Psychology Review*.
- Liotti, M., Woldorff, M. G., Perez III, R., & Mayberg, H. S. (2000). An ERP study of the temporal course of the Stroop color-word interference effect. *Neuropsychologia*, 38, 701–711.
- Lundqvist, A., Grundström, K., Samuelsson, K., & Rönnerberg, J. (2010). Computerized training of working memory in a group of patients suffering from acquired brain injury. *Brain Injury*, 24(10), 1173–1183. <http://doi.org/10.3109/02699052.2010.498007>

- Melby-Lervåg, M., & Hulme, C. (2012). Is working memory training effective? A meta-analytic review. *Developmental Psychology*, 49(2), 270–291. <http://doi.org/10.1037/a0028228>
- Middleton, D. K., Lambert, M. J., & Seggar, L. B. (1991). Neuropsychological rehabilitation: microcomputer-assisted treatment of brain-injured adults. *Perceptual and Motor Skills*, 72(2), 527–530.
- Morey, R. D., & Rouder, J. N. (2015). *BayesFactor: Computation of Bayes Factors for Common Designs*. Retrieved from <http://CRAN.R-project.org/package=BayesFactor>
- Morrison, A. B., & Chein, J. M. (2010). Does working memory training work? The promise and challenges of enhancing cognition by training working memory. *Psychonomic Bulletin & Review*, 18(1), 46–60. <http://doi.org/10.3758/s13423-010-0034-0>
- Park, N. W., & Ingles, J. L. (2001). Effectiveness of attention rehabilitation after an acquired brain injury: A meta-analysis. *Neuropsychology*, 15(2), 199–210.
- Peirce, J. W. (2007). PsychoPy--Psychophysics software in Python. *Journal of Neuroscience Methods*, 162(1-2), 8–13. <http://doi.org/10.1016/j.jneumeth.2006.11.017>
- Perkins, D., & Salomon, G. (1989). Are Cognitive Skills Context-Bound? *Educational Researcher*, 18(1), 16–25. <http://doi.org/10.3102/0013189X018001016>
- Redick, T. S., Shipstead, Z., Harrison, T. L., Hicks, K. L., Fried, D. E., Hambrick, D. Z., ... Engle, R. W. (2012). No Evidence of Intelligence Improvement After Working Memory Training: A Randomized, Placebo-Controlled Study. *Journal of Experimental Psychology: General*. <http://doi.org/10.1037/a0029082>
- Rouder, J. N., & Morey, R. D. (2012). Default Bayes Factors for Model Selection in Regression. *Multivariate Behavioral Research*, 47(6), 877–903. <http://doi.org/10.1080/00273171.2012.734737>
- Salthouse, T. A., & Meinz, E. J. (1995). Aging, Inhibition, Working Memory, and Speed. *The Journals of Gerontology Series B: Psychological Sciences and Social Sciences*, 50B(6), P297–P306. <http://doi.org/10.1093/geronb/50B.6.P297>
- Schmitter-Edgecombe, M. (2006). Implications of basic science research for brain injury rehabilitation: A focus on intact learning mechanisms. *The Journal of Head Trauma*

- Rehabilitation*, 21(2), 131–141.
- Sellke, T., Bayarri, M. J., & Berger, J. O. (2001). Calibration of p Values for Testing Precise Null Hypotheses. *The American Statistician*, 55(1), 62–71.
<http://doi.org/10.1198/000313001300339950>
- Shipstead, Z., Redick, T. S., & Engle, R. W. (2012). Is working memory training effective? *Psychological Bulletin*, 138(4), 628–654. <http://doi.org/10.1037/a0027473>
- Sturm, W., Dahmen, W., Hartje, W., & Willmes, K. (1983). Ergebnisse eines Trainingsprogramms zur Verbesserung der visuellen Auffassungsschnelligkeit und Konzentrationsfähigkeit bei Hirngeschädigten. *Archiv Für Psychiatrie Und Nervenkrankheiten*, 233(1), 9–22.
- Sturm, W., & Willmes, K. (1991). Efficacy of a reaction training on various attentional and cognitive functions in stroke patients. *Neuropsychological Rehabilitation*, 1(4), 259–280.
<http://doi.org/10.1080/09602019108402258>
- Sturm, W., Willmes, K., Orgass, B., & Hartje, W. (1997). Do specific attention deficits need specific training? *Neuropsychological Rehabilitation*, 7(2), 81–103.
- Tulving, E., & Thomson, D. M. (1973). Encoding specificity and retrieval processes in episodic memory. *Psychological Review*, 80(5), 352.
- Unsworth, N., Heitz, R. P., Schrock, J. C., & Engle, R. W. (2005). An automated version of the operation span task. *Behavior Research Methods*, 37(3), 498–505.
- Vanderploeg, R. D., Crowell, T. A., & Curtiss, G. (2001). Verbal Learning and Memory Deficits in Traumatic Brain Injury: Encoding, Consolidation, and Retrieval. *Journal of Clinical and Experimental Neuropsychology (Neuropsychology, Development and Cognition: Section A)*, 23(2), 185–195. <http://doi.org/10.1076/jcen.23.2.185.1210>
- Verhaeghen, P., & De Meersman, L. (1998). Aging and the Stroop effect: A meta-analysis. *Psychology and Aging*, 13(1), 120–126. <http://doi.org/10.1037/0882-7974.13.1.120>
- Wagenmakers, E.-J. (2007). A practical solution to the pervasive problems of p values. *Psychonomic Bulletin & Review*, 14(5), 779–804. <http://doi.org/10.3758/BF03194105>
- Westerberg, H., Jacobaeus, H., Hirvikoski, T., Clevberger, P., Östensson, M.-L., Bartfai, A., & Klingberg, T. (2007). Computerized working memory training after stroke—A pilot study.

Brain Injury, 21(1), 21–29. <http://doi.org/10.1080/02699050601148726>

Wetzels, R., Matzke, D., Lee, M. D., Rouder, J. N., Iverson, G. J., & Wagenmakers, E.-J. (2011).

Statistical Evidence in Experimental Psychology: An Empirical Comparison Using 855 t Tests. *Perspectives on Psychological Science*, 6(3), 291–298.

<http://doi.org/10.1177/1745691611406923>

Yip, B. C., & Man, D. W. (2013). Virtual reality-based prospective memory training program for people with acquired brain injury. *Neurorehabilitation*, 32(1), 103–115.