Reply

Working memory training remains a work in progress

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A R T I C L E   I N F O

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In recent years, understandable enthusiasm (e.g., Sternberg, 2008) regarding landmark publications such as Klingberg, Forssberg, and Westerberg (2002) and Jaeggi, Buschkuehl, Jonidas, and Perrig, (2008) has been met with reasonable skepticism (Melby-Lervåg & Hulme, 2012; Moody, 2009; Shipstead, Redick, & Engle, 2012). We do not view this skepticism as an attempted rebuttal of the hope that psychologists will develop methods for reliably training working memory (WM) and associated abilities. Rather, it is a simple acknowledgment that not everybody finds the available data to be persuasive.

In general, research is progressing as it should. Experimental methods are improving (Gathercole, Dunning, & Holmes, 2012) and researchers are developing training techniques that recognize the complexity of WM (Gibson, Gondoli, Johnson, Steeger, & Morrissey, 2012) and the complexity of humans that are engaged in training (Shah, Buschkuehl, Jaeggi, & Jonides, 2012). Yet, while most of the commentators provide valid reasons for believing that WM training will one day become a reliable method of cognitive remediation, few argue that current WM training techniques represent a finished project.

And this is the thrust of our concern. The most accurate description of the state of WM training is that the fundamental techniques remain a work in progress. From a scientific perspective, this is completely normal. However, outside of academia it is clearly problematic. Cogmed sells their product to school systems and to unhealthy individuals under the guise of scientific validation (Cogmed, 2012). Lumosity has begun to advertise their products on television and claim to have 20 million users (Lumosity, 2012).

The claims that these and other companies make have no bearing on whether or not WM training works; only continued research can provide that answer. However, it does increase the importance of arriving at supportable conclusions. While the statements made by commercial providers have no bearing on the science of WM training (Jaeggi, Buschkuehl, Jonides, & Shah, 2012), the converse is not true. Interesting findings are rapidly turned into advertisements. Moreover, providers of commercial products are not subject to peer review and can thus present results selectively, advertise through insinuation (e.g., Cogmed, 2011a, 2011b), or make unsubstantiated claims (e.g., Jungle Memory, 2011; Lumosity, 2011).

From this perspective, skeptics can be forgiven if their reaction seems overly conservative. WM training is an area of cognitive psychology in which the general public has clear interest. Thus the role of the skeptic is not simply to provide peer review, but also a mechanism through which the public can receive the other side of the story.

1. Near transfer, generally speaking

The most fundamental question regarding Cogmed is whether or not it actually leads to increases in WM capacity. We can bog the discussion down by examining the cases where simple span tasks do and do not provide adequate measurement of WM capacity (e.g., Jaeggi et al., 2012), or whether or not visuo-spatial simple spans have a special relationship to fluid intelligence or ADHD.
2. Does WM need to change?

On a related point, Jaeggi et al. (2012) question whether WM needs to be the mechanism of near transfer. In other words, could training affect some other process? Part of this discussion depends on what you mean by “WM”. If we define WM through complex span tasks (e.g., Fig. 1a from the target article), then for n-back training, it is almost certainly the case that something other than WM is changed by training. It is becoming clear that individual differences in complex span and n-back performance are, at best, weakly related (Jaeggi, Buschkuehl, Perrig, & Meier, 2010; Kane, Conway, Miura, & Colflesh, 2007). Thus it would be unrealistic to expect n-back training to transfer to complex span performance. Nor would we expect complex span research to be very useful in generating predictions regarding the effects of n-back training.

When discussing training techniques such as Cogmed (which employ span tasks, rather than the n-back) the issue of transfer is one of coherence. A principled approach to training assumes that a WM training task will only train processes that are critical to performing that task. At the very least, the proposed mechanism of transfer should be related to the method of training. Thus, while it remains possible that training will only affect certain subcomponents of WM (e.g., Gibson et al., 2012; Logie, 2012), transfer results cannot be accepted willy-nilly. Transfer should fit within predictions that are generated from the greater literature. Otherwise, mundane explanations like posttest motivation and type I error provide parsimonious accounts of training effects.

3. Control groups: an important concern with no clear answer

Our discussion regarding non-adaptive control groups elicited diverging opinions. Morrison and Chein (2012) summarized our concern much better than we did when they stated that non-adaptive control conditions are problematic, due to their “repetitive and unchallenging nature”. Jaeggi et al. (2012), on the other hand, remain unconvinced. For instance, they note that a recent study by Bergman Nutley et al. (2011) found no evidence of differential motivation between adaptive and non-adaptive groups. Although this is a valid point, we note that the transfer results found by Bergman Nutley et al. (2011) were limited to near transfer to tasks that resembled the training tasks. Our concern with control group-related motivation regards broad transfer effects (e.g., Klingberg et al., 2005, 2002).

Regardless, we acknowledge that our position on non-adaptive control groups is speculative. And in reality, this may not have been the best forum to express these concerns. After all, our argument is that, regardless of control group, the evidence that Cogmed training has any effect on cognition is unconvincing.

However, for the greater literature, it remains important that researchers attempt to equate training and control groups in terms of experiences. As Orne (1962) pointed out, the testing environment is a social situation. The people in that situation probably do not know much about psychologists, but they do have a notion that psychologists are qualified to make judgments about human mental competency. The pretest–posttest situation only makes such judgments more obvious. Therefore, in order to equate posttest motivation, researchers should ensure that the expectation of change has been communicated to both training and control groups. While it is true that giving feedback and rewards to a participant in a non-adaptive condition equates the groups in terms of explicit feedback (Jaeggi et al., 2012), the non-adaptive control condition does not equate implicit communication that arises from the difficulty of the training task.

4. The synergy of qualitative and quantitative approaches

In parallel to our own work, Hulme & Melby-Lervåg (2012; Melby-Lervåg & Hulme, 2012) find little reason to conclude that Cogmed training produces reliable improvements to WM or any other mental processes. Our own avoidance of using meta-analytic techniques stems from concern that, while effects may be present, the source of those effects needs to be understood as well. Nonetheless, we have arrived at remarkably similar conclusions. Hulme and Melby-Lervåg (2012) conclude that there is no effect of Cogmed training on Stroop performance. This comports well with our argument that improved performance on the Stroop tasks that are included in Cogmed studies cannot be explained by WM training (i.e., WM has little relation to Stroop performance when congruent trials are excluded). Far transfer to these Stroop tasks appears to be type I error.

Moving forward, Cogmed studies should not give up on testing controlled attention. Our argument is that this potential effect has yet to be adequately explored. Although no effect has been produced in the extant literature, this does not mean that no effect can be produced. However, controlled attention tasks need to be selected with greater care. We do not recommend the continued use of the Stroop task, as it requires a large number of trials and the interaction with WM capacity can be quite small (Kane & Engle, 2003). Instead, we suggest that going forward the antisaccade task (cf. Kane, Bleckley, Conway, & Engle, 2001) and flanker task (cf. Redick & Engle, 2006; Shipstead, Harrison, & Engle, 2012) are more appropriate, as the relationship of these tasks to WM capacity is not as dependent upon overall task congruency (see target article).

5. A note on Green et al. (2012)

Since the acceptance of the target article, an interesting new study by Green et al. (2012) has been published. These researchers trained ADHD–diagnosed children on Cogmed and measured transfer through a battery of five relevant behaviors. Critically, the behaviors were scored by trained raters, who were blind to condition assignment. Relative to a non-adaptive control group, Cogmed training was associated with reductions in the number of times two of five behaviors were performed.

Whether transfer to these two behaviors is meaningful (or the luck of the draw) is a question that can only be addressed through

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1 For those who are interested in our opinion on the first two issues, see Shipstead, Redick, et al. (2012, pp. 634–635).
replication. However, this study does provide a valid model for future research on the effect of Cognmed training on ADHD-related symptoms. In particular, the use of objective measures of transfer, coupled with blind raters is more appropriate than asking parents or teachers for their subjective evaluations of behavioral changes. Indeed, although Green et al. (2012) found differential transfer to their objective measures, parents of both the adaptive and the non-adaptive groups reported equivalent improvement of symptoms.

6. Is working memory capacity fixed? Is this a meaningful question?

As Gibson et al. (2012) point out, existing theories of WM capacity are designed to explain individual differences, rather than change after training. Stability of WM capacity is not directly affected by training, if every known WM training program were shown to be completely ineffective, this says nothing about future interventions. Thus, our position should not be misinterpreted as favoring a fixed-system WM. Our concern regards the quality of evidence that has been collected in support of specific training techniques.

As Klingberg (2012) states that our target article article contradicts the proper scientific question of whether or not WM capacity can be increased. This question is off-base. Even if every known WM training program were shown to be completely ineffective, this says nothing about future interventions. Thus, Klingberg’s question cannot be answered in the negative. The only question that can be properly addressed is whether or not a given technique is adequate to increase WM capacity. In that respect, our target article is both properly focused and meaningful to researchers and to people who are considering paying for Cognmed training.

References


