Commentary
The controversy over Cogmed
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Both within and beyond the academic community we are fascinated with the notion of improving mental capacity. This becomes more than simple fascination when tools for cognitive enhancement show promise as an intervention directed toward clinical populations with deficits in core mental capacities such as attention and working memory (WM). It is thus unsurprising that the earliest work with Cogmed-style training, which found that children with ADHD improved in untrained measures of cognitive ability following the intervention (Klingberg et al., 2005; Klingberg, Forssberg, & Westerberg, 2002), elicited a great deal of intrigue and optimism. Now, roughly ten years after the publication of Klingberg et al. (2002), Cogmed has been investigated in an impressively large number of empirical studies (with several more in press or in progress), and is being actively marketed to parents, school systems, and other social agencies. Amidst the attention Cogmed has received, Shipstead, Hicks, and Engle (2012) ask the apt question, does Cogmed work? And the more nuanced question, does it work in the way that is claimed by those who advocate and market it?

Shipstead et al. (2012) present a careful evaluation of the body of studies testing Cogmed. They detail successful training outcomes, like demonstrations of improvements on measures of attentional stamina (Brehmer, Westerberg, & Backman, 2012; Westerberg et al., 2007), and also shortcomings, like failures to reproduce the early evidence that Cogmed training evinces positive transfer to measures of fluid reasoning (Holmes, Gathercole, & Dunning, 2009; Holmes et al., 2010). Based on their evaluation, Shipstead et al. (2012) conclude that “the claims made by Cogmed are largely unsubstantiated” (p. 2), and that “the only unequivocal statement that can be made is that Cogmed will improve performance in tasks that resemble Cogmed training” (p. 19).

Though we hold an optimistic view of the potential for WM training to produce meaningful and generalizable gains in certain cognitive abilities, our own conclusions regarding the current status of work with Cogmed largely echo those of Shipstead et al. There is clear evidence that Cogmed training can sometimes improve performance on specific measures of cognitive ability (see Shipstead et al., 2012, Table 1), but the most exciting results have proven difficult to reproduce – perhaps reflecting real, but fragile, psychological phenomena. Strong claims about the enhancement of general cognitive abilities, and about the meaningfulness of such enhancements to performance in ecological settings have not yet been adequately supported.

To put it simply, some good science has been conducted, but much more is needed. Even though Cogmed and other WM training paradigms may be shown to have efficacy in specific instances, the stamp of scientific approval cannot yet be placed on this (or any other) WM training product that is offered in the public arena. Even as relative optimists, we contend that there remains too extensive a list of questions we do not yet have answers to: What conditions are necessary in order to get positive transfer? What individual characteristics moderate receptivity to training? What cognitive mechanisms are actually targeted by training? What procedures optimize or diminish the return on investment in training? With so many fundamental questions still to be answered, we wonder if the most informative and impactful Cogmed studies have yet to be conducted.

So, in our view, the jury is still out on whether Cogmed training can live up to the far-reaching potential that is implied in its
commercial marketing. It has neither failed nor passed the most critical tests of its efficacy. Still, the investigators who have worked on the development of this product should be acknowledged for their honest and scientifically rigorous exploration of its potential. The preliminary findings were not overstated, and have been followed-up in a sizable number of subsequent studies. Successive iterations demonstrate increasing attentiveness to the standards of scientific experimentation – e.g., random assignment into treatment groups, blind coding of data, inclusion of an active control group, sensitivity to concerns over small sample size, etc. In published research, those presenting evidence supporting the efficacy of Cogmed have typically demonstrated restraint in not over-interpreting the data, and have been forthcoming in instances where the outcomes did not meet expectations. These investigators should also be credited for their effort to garner converging evidence using an array of alternative methodological approaches (laboratory studies, ecological studies, neuroimaging, genetic sampling, etc.).

The problem, perhaps, is that with each careful step forward, the impressiveness of the behavioral outcomes seems to diminish. And, as highlighted by Shipstead et al. (2012), there remain several reasons to approach claims about the scope and magnitude of transfer from Cogmed training with a degree of skepticism.

The question of whether Cogmed actually alters WM capacity, rather than specific subsidiary processes that are engaged by particular WM tasks, is arguably the most critical of the issues raised by Shipstead et al. (2012). The extant WM training literature operationalizes the expansion of WM capacity as improved performance on specific WM tasks. Cogmed achieves this initial benchmark with demonstration of greater gains for trained relative to control participants on multiple untrained measures of WM, including forward and backward digit span and span board measures (see Shipstead et al., 2012, Table 1). However, we agree with Shipstead et al. (2012) that the bar should be set higher. To convincingly demonstrate improved WM capacity, trained participants should be shown to outperform a suitable active control group on tests of WM that deviate more substantially from the tasks used during training. If Cogmed training were found to yield improvements on measures of WM that assess more than just the simple short-term storage of materials used during training – e.g., complex span (Conway et al., 2005), change detection (Luck & Vogel, 1997), and running memory span (Broadway & Engle, 2010) – then the case for an effect on the central capacity of WM would be much harder to refute. Some work with Cogmed (Holmes et al., 2009) approaches this standard, but to date there is not a single study using Cogmed (or any other training paradigm) to show universal improvements on working measures that have little resemblance to the training tasks (despite the availability of such measures).

Of course, the “blame” for this state of affairs does not lie with the investigators exploring Cogmed. After decades of research, the scientific literature on WM still contains widely varying claims regarding the size, structure, and even the nature of WM capacity, and there is no consensus regarding the best way in which to measure it. So, even if Cogmed training were shown to improve WM at the construct level, there would still likely be debate about whether the improvement constitutes a true increase in capacity. Another important topic addressed by Shipstead et al. (2012) surrounds the use of non-adaptive training as the active control condition in most Cogmed studies (e.g., Holmes et al., 2009; Klingberg et al., 2005; Thorell, Lindqvist, Nutley, Bohlin, & Klingberg, 2009). This non-adaptive control condition may be problematic since its reproductive and unchallenging nature makes it considerably less engaging and motivating than the adaptive version – inducing a type of learned helplessness. Finding an appropriate control group has proven to be a difficult challenge for this field (Morrison & Chein, 2011), but we have recently witnessed the development of more compelling approaches (trivia training (Jaeggi, Buschkuehl, Jonides, & Shah, 2011), adaptive visual search (Redick et al., 2012)) than the non-adaptive training that has been the staple of Cogmed studies.

A study of Cogmed that includes adequate assessment of WM capacity at the construct level as well as a control condition that matches the adaptive training condition in terms of engagement and believability is simply not present in the literature. To reinforce an earlier point, Cogmed has neither achieved, nor failed, the critical benchmark. Accordingly, we find ourselves in agreement with Shipstead et al. (2012) in their assessment that the conversion of Cogmed into a commercial product was premature, and that the most sweeping claims about its efficacy are unsubstantiated. Of course, the actions in creating a commercial product cannot be undone, and we understand that decisions at the intersection of research and business are often controversial. Our best current course of action as researchers is to closely examine existing empirical evidence, as Shipstead et al. (2012) have done, and to work toward the development and implementation of new studies that address the current gaps in methodology.

Since the earliest discoveries using the Cogmed were, in large part, the inspiration for the development and testing of many other approaches to WM training, one might ask: If it turns out that those findings do not hold up to further scrutiny and testing, shouldn’t the whole endeavor of exploring WM training as a tool for general cognitive enhancement be abandoned? Our conclusion is that the endeavor has been, and will remain, worthwhile. Even if Cogmed as currently composed turns out not to be the right tool, some variation on Cogmed, or some other promising approach to have emerged from the field of WM training (Chein & Morrison, 2010; Jaeggi et al., 2011; Schmiedek, Lovden, & Lindenberger, 2010) may survive the rigors of scientific scrutiny. Moreover, we expect that continued research in this area will continue to fuel the development and refinement of basic theoretical work on the nature of working memory, and its place in the broader cognitive landscape.

References


