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Epilogue

Don't Buy the Snake Oil

Michael R. Dougherty and Randall W. Engle

This volume aims to summarize the state of the art in the working memory (WM) training literature. The idea for the book was conceived in 2011 and at the time the notion that general cognitive abilities could be improved through training was exciting yet controversial. The early evidence, although imperfect, was tantalizing: Several studies had seemingly illustrated that people's performance on untrained measures of intellectual abilities could be improved after just a few hours of training on cognitively demanding tasks (Chein & Morrison, 2010; Jaeggi, Buschkuel, Jonides, & Perrig, 2008; Klingberg et al., 2005; Mahncke et al., 2006). Proponents of training pointed to these early successes as reason for optimism and funding agencies released a flood of money to support the work, while critics pointed to shortcomings in experimental design as reason to remain skeptical.

In most areas of science, progress in resolving open scientific questions is slow, sometimes requiring decades of careful research. However, this has not been the case for research on WM training, which has exploded over the past decade. The primary open question addressed by the explosion of research was whether WM training is indeed effective in improving intellectual abilities. Researchers tackled the question by addressing several methodological issues: Do the effects persist when placebo (i.e., active) control conditions are used as comparisons? Are the effects present when the sample size is adequate? Are the effects dependent on randomization to condition (training versus control)? Are the observed transfer effects dependent on the overlap between training and transfer tasks? How long do training effects persist?

Getting to the bottom of these questions has meant diving into the nitty-gritty details to discern when, where, and why training effects transfer. Such work lacks the "pizzazz" and excitement that comes with grandiose claims that IQ can be increased, but it's a necessary step in our understanding of human nature. Unfortunately, the outcome of the systematic study of WM training has not been favorable to the proponents: We now know that many of the most promising findings in the WM training literature were likely false starts. The imperfections cited by the critics of some of the seminal studies have proven to be crucial to their outcome (Simons et al., 2016).

A complete review of the literature is well beyond the scope of this brief epilogue, but it may be useful to point to a few recent meta-analyses to address some of the above questions. Meta-analyses are useful because they address one of the main methodological challenges that has plagued the WM training literature, namely, the issue of small samples.

Are the Effects of WM Training Dependent on the Type of Control Condition Used in the Study Design?

The motivation for this question stems from the long history of research documenting that people's behavior often changes in response to placebos and/or expectations implied or otherwise communicated to participants (Boot, Simons, Stothart & Stutts, 2013; Kirsch, 1985). Controlling for participants' expectation is important for differentiating between causal mechanisms: Do transfer effects arise because the cognitive mechanisms improve with training, or do such effects arise due to expectations? This question has been addressed in a number of recent meta-analyses of WM training. For instance, in a comparison of N-back training studies using active versus passive controls, Dougherty, Hamovitz, and Tidwell (2016) observed that the meta-analytic effect for improvements on measures of fluid intelligence was nearly half a standard deviation for passive control designs but approximately zero for active control designs. Analysis of active control studies using Bayesian methods revealed strong support for the null hypothesis. An analysis by von Bastian, Guye, and De Simoni (see Chapter 4 of this volume) offers a similar conclusion, as do several other recent meta-analyses (see Melby-Lervag, Redick, & Hulme, 2016; Sala & Gobet, 2017, among others). The issue of whether transfer of training depends on type of control group is settled. Effects quite clearly manifest when training is compared to a passive control but there is little to no effect when active controls are used.

Does Random Assignment to Condition Matter?

Randomization is a bedrock principle of sound methodology. Without it, any statistical differences (or lack thereof, for that matter) observed between groups can be attributable to pre-existing group differences, self-selection into groups, or biases in how experimenters assign participants to groups. Randomization is the one tool that researchers have at their disposal to ensure that the participants assigned to one group are, on average, identical to those in another group on all possible dimensions. Of course, randomization works best when sample sizes are large, an issue that we've already noted as problematic for many WM training studies. So, does randomization matter? In a meta-analysis of children age 3 to 16, Sala and Gobet (2017) observed that the meta-analytic effect size for far transfer dropped from $G = 0.27$ for nonrandomized samples to $G = 0.07$ for randomized samples. The effect size for studies using both active controls and randomization was 0.03. Clearly, these results indicate that randomization does indeed matter and that transfer effects are negligible when sizable randomized samples are tested.

Are Training Effects Dependent on the Overlap Between Training and Transfer Task Operations or Stimuli?

The allure of WM training as a cure for intellectual deficits and cognitive decline is the promise that training on one task transfers to an ostensibly different task that bears little resemblance to the trained task. There are two orthogonal dimensions to consider in this regard. One dimension concerns the relation between the underlying theoretical constructs. Unsurprisingly, there is clear evidence that transfer effects are larger for tasks that measure a construct closely related to the task that is trained (see Melby-Verlag et al., 2016; Simons et al., 2016). The second dimension concerns the types of stimuli or operations included in the tasks that measure the construct. For instance, an N-back training task that uses letters as stimuli and an episodic memory task that uses letters as stimuli may ostensibly reflect different theoretical constructs, yet share task variability due to the overlap between stimuli. As far as we know, research addressing this distinction is relatively underdeveloped, but it may account for some observed transfer effects (see Soveri, Antfolk, Salo, & Laine, 2017; Sprenger et al., 2013). One thing is for sure, however: the ideal approach would involve an assessment of transfer using a latent variable approach, which addresses the issues of measurement error and task-specific variability (see Chapter 3 of this volume, by Schmiedek, Lövdén, & Lindenberger).

What Might a Convincing Study Look Like?

Taking the above considerations into account, the ideal study is one that uses a double-blind procedure with an active control group, collects multiple measures of cognitive abilities pre- and posttest, randomly assigns a large sample, collects measures of prior expectations, and uses statistical tests that allow one to quantify evidence for or against the null hypothesis. De Simoni and von Bastian (2018) conducted a study that closely approximates this idealized standard, and once again the results are no friend of the proponents. In their study, which included roughly 60 participants per condition, De Simoni and von Bastian's data showed evidence for the null hypothesis of no transfer of training across all measured constructs, with most Bayes factors for far transfer ranging from 5.76 to 11.36 in favor of the null.

But What About the Brain?

Of course, this volume is about more than transfer. Various chapters discuss the neural underpinnings of WM training and postulate ways in which neuroscience methods may allow identification of the neural markers associated with training as well as potential methods for modulating training benefits (e.g., tCDS). As interesting as these chapters may be, the usefulness of these methods as they pertain to transfer always comes back to the issue of behavioral change. It is trivial to note correlations between brain and behavior, or to observe changes in brain activation, structure, or synchrony as a function of training. Naturally, one would expect changes in brain to accompany any practiced activity. Even placebos elicit changes in brain activity (see Zunhammer, Bingel, & Wager, 2018). However, in the absence of sustained behavioral improvements on untrained measures of cognitive ability, there is not much for neuroscience to explain. That's not to say that the neuroscience data aren't valuable in other ways, but rather that neuroscience data cannot and do not substantiate claims of transfer that aren't observed behaviorally.

Summary

There's a saying in investigative research: "Follow the evidence." Unfortunately, the evidence now strongly, if not decisively, indicates that WM training does not transfer to performance on nontrained tasks. The critics were right: WM training does not lead to long-lasting generalizable improvements in cognitive functioning. We're sure that this conclusion will be disappointing to many readers of this book, as well as to the many individuals who have purchased commercial brain-training products. We're sorry to offer such a gloomy outlook on the state of the art, but sometimes science leads us where it leads us. Every parent of a child struggling in school will tell you that they would do almost anything to see their child succeed and love school. This has led parents of children classified as "reading disabled" to purchase glasses with colored lenses (Irlen lenses) because some optometrists told them the problem was in their child's vision. Likewise, promoters of WM and brain training have franchised sales of their products through psychiatrists and psychologists with the lure to distraught parents that their child could be "fixed" by several hours of playing video games. Military agencies that need their enlistees to learn incredibly complex and dangerous jobs, such as demolition experts for explosive devices (as depicted in the film *The Hurt Locker*), are desperate to have recruits who are able to read, to study, and to learn the vast volumes necessary to successfully perform those duties. The prospect that WM and brain training might do that has led to enormous efforts to find the effects promised by the proponents of WM and brain training. With that vast amount of money and effort, if the effects were there, they would have been found, would have been shown to be robust to random assignment and the use of placebo controls, and would have been replicated. They were not.

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