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■ COMMENTARY

# The Who, What, Why and How of Intervention Programmes: Comments on the DDAT Evaluation

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The study by Reynolds, Nicolson and Hambly has highlighted many of the methodological challenges for those conducting practical intervention studies. These have been mentioned by previous commentators and need not, therefore, be rehearsed. I choose to focus here on three key issues where a little further discussion might prove useful, especially for 'non-specialists', including many parents and teachers who are not practised in the interpretation of research in applied cognitive psychology. These are my how, who, what questions: How might the training impact on literacy skills, who does it seem to work for, what is the effect of the training—what is the training doing?

The first of these issues concerns what the authors refer to as 'transfer'—how improvements in some skills brought about through training might produce improvements in other skills that have not been directly trained. It is one thing to give a programme of exercises or activities which enable people to do better on tests that involve the same kind of activity; if I practice balancing on a wobble-board, I will probably get better at doing tests that involve balance. But how might that improvement affect other skills? Will practising balancing make me a better reader? If it does make me a better reader, how might that happen? Perhaps I have got more confident about taking tests, perhaps I have become more willing to take risks and more prepared to 'have a go', perhaps the training activities have taken the place of things that were proving stressful. Perhaps I have become 'test smart'—better at predicting what is going to happen and better at orienting to the key demands and tuning out the distractions. All of these are possibilities that must be considered before suggesting that the training has brought about a fundamental change in the way that my cerebellum or some other part of my brain is working.

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The second issue concerns the description of the participants in the research. Reynolds, Nicolson, and Hambly (2003) say, in the title, that their treatment is for children with reading difficulties. But by the first line of the abstract it has become 'an approach to remediation of dyslexia-related disorders'. By the time they are well into the study, they are evaluating a claim (claim 1) that 'a high percentage of children with dyslexia, dyspraxia or attention deficit will show cerebellar or vestibular problems'. The problems of the children seem to have been getting worse as the paper progresses!

So what then can we say about the sample? Firstly, they are not very far behind in reading; the intervention group being 10.6 months delayed on average and the no-intervention group 4.4 months delayed on average at the start of the study. Some of the participants were reading in advance of their age level. Second, only a few had a formal diagnosis of specific difficulties. Out of the 18 people in the intervention group, two were dyslexic and one was dyspraxic; out of the 17 people in the no-intervention group, four were dyslexic, one dyspraxic and one had ADD. The main point is that the vast majority of participants had not been identified as having specific difficulties. A second point is that the no-intervention group contained twice as many (or 100% more) participants with identified difficulties. Any advantage to the intervention group on follow-up tests could be the result of that group having within it fewer children with severe difficulties.

In fairness, the researchers formed their groups using the Dyslexia Screening Test rather than existing information; they used a cut off of 0.4 and the average 'at risk quotient' (ARQ) was about 0.7 for both groups. An ARQ of 0.9 or above is considered to be a strong indicator of risk, with scores 0.5 or below classified as 'not at risk' and scores in between (0.6 up to 0.9) classified as 'mild risk'. Thus, the sample as a whole were not showing strong risk signs for dyslexia; some were 'not at risk'.

The next question is the what question: what does the programme do, what are its effects? Reynolds *et al.* argue that the exercises produce 'clear and significant improvements in cerebellar/vestibular performance', but we need to take a moment to see what that really means. The children in the intervention group did seem to improve on the measures of eye tracking and balance which is encouraging as they were practising eye tracking and balance activities. They also seemed to improve more than the no-intervention group on three of the 11 sub-tests from the DST (a re-analysis by Nicolson in this issue clarifies this). The measures were the speed of bead threading, the number of animal names that could be produced in a minute and the number of words read correctly in a minute. It was only on these three measures that the gains made by the intervention group were significantly better than the gains made by the no-intervention group. It is interesting to see (in Figure 2 of Reynolds *et al.*) that the main source of this statistical effect appears to be that the no-intervention group were better than the intervention group at the start and that this difference was reduced at the end. It was not the case that the groups were matched at the start and that the intervention group were better at the end. Indeed, on the 1-minute reading, 2-minute spelling and nonsense passage reading tests, the mean score for the no-intervention group remained higher than the mean score for the intervention group. The study provides no before and after data for performance on standardized reading tests for the intervention and

no-intervention groups. So in answer to the question 'what' are the effects of the programme on literacy skills, the answer is 'nothing', or at best 'we don't know'.

The final question is 'why?' There are obviously many facets to this question and it would not be appropriate to go into most of these here. However, it is perhaps worth considering one set of reasons why this is an area attracting so much attention. One of the most interesting challenges of cognitive psychology is in trying to interpret performance on behavioural tests in terms of an underlying model of mental processes—what is sometimes called 'cognitive architecture'. Going beyond that, neuropsychologists seek to provide an interpretation in terms of the operation of specific structures or systems in the brain. Such an understanding would open the door to the possibility—exciting to some and frightening to others—that treatment at the level of brain functioning could be given to improve cognitive performance, particularly learning. Linked to this is the 'value for money/effort' argument: if treatment or training can be given at a 'deep' level in the system then the effects of that will potentially be much more widespread compared to treatment at the 'surface' level.

This discussion may provide a partial answer to the 'why' question; it is certainly easy to see why the DDAT group—amongst others—are drawn to the idea of 'training the brain'. However, it is important to resist the immediate conclusion that a change in performance on a behavioural test has come about because the performance of specific brain functions has improved. What has improved is the performance on the test—everything else is speculation. So, to return to my first, the 'how', question, there are lots of other explanations that need to be considered before one accepts the more 'exciting' speculation that training has brought about fundamental changes in physiology.

In summary, the participants in this study were a mixed group of children containing a few with recognized specific difficulties but most of whom were at the level that would be expected. The study really has nothing to contribute to our knowledge of the possible effects of the exercise programme for children with dyslexia, dyspraxia and ADD, because there were very few children with these conditions in the study. The group who took part in exercises, gained from those exercises in terms of performance on some tests, but there is no evidence that this was a specific effect of the treatment programme, rather than some general improvement in ability to perform on tests which emphasize speed.

The authors, quite rightly, sound a note of caution at the end of the paper and suggest a need for further research. One must, however, sympathize with non-specialists who might be swayed by the more positive spin on the results that comes through at other times and has been picked up in the media. Let people be clear that the paper by Reynolds *et al.* provides no evidence that the DDAT programme has any specific effect on reading, spelling, memory or language performance for people with dyslexia, dyspraxia or ADD.

## References

- Reynolds, D. E., Nicolson, R. I., & Hambly, H. (2003). Evaluation of an exercise-based treatment for children with reading difficulties. *Dyslexia: An International Journal of Research and Practice*, 9(1), 48–71.