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## ■ RESPONSE

# Sound Findings and Appropriate Statistics: Response to Snowling and Hulme

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Our study (Reynolds, Nicolson, & Hambly, 2003) has led to several commentaries, both in this issue and the next issue of the journal. A series of point by point rebuttals of the various criticisms made would lead to unhelpful repetition and engender a regrettable adversarial debate. Consequently we have chosen to provide a composite response in the next issue, addressing the range of issues raised, and identifying areas of agreement and disagreement and issues for further research. Given that it is a quarterly journal, we consider it valuable to provide the further analysis suggested by Snowling and Hulme (2003) in the single page allowed us in this issue.

Snowling and Hulme make two major criticisms of the study. The first criticism is that the study fails to meet several of the 'critical criteria' posited by Troia (1999) for intervention studies. It is worth noting that using his set of criteria Troia concluded that all 39 articles—including many considered classics—published in the literature on phonemic awareness were fatally flawed in at least one critical criterion. We are at least in good company. However, the more important point is that, as explained in the original article, we considered that Troia's randomized 'clinical' trial paradigm was not appropriate either for the key issue under investigation or the form of intervention used. The study was designed to investigate whether the DDAT intervention did indeed provide value to the children undertaking it, and if so, whether the value transferred to reading. It seems unfair to criticise us for failing to adhere to criteria for a design that we did not use. Several commentators raise this issue and we discuss it at greater length in the following issue. However, the more substantive criticism, and one we acknowledge as a weakness of the study, was the fact that, although the two groups were matched on the mean DST 'at risk quotient', it turned out that the intervention group's reading was initially worse than that of the control group. Snowling and Hulme point out correctly that this weakens the inferences that can

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be made from statistical analyses of group differences, in that at least part of the effects obtained might be attributable to statistical artifact rather than a 'true' effect of the intervention. They advocate use of analysis of covariance on the raw scores, taking post-test score as dependent variable and pre-test score as covariate. This is intended to eliminate any statistical artifacts arising from differences in pre-test performance and hence to reveal the 'true' effect of the intervention. We undertook the analyses of covariance as suggested. The results for the DST sub-tests were as follows. There was significantly greater effect for the intervention group on bead threading, reading and semantic fluency [ $F(1,32) = 7.2, p < 0.05$ ;  $F(1,32) = 4.8, p < 0.05$ ;  $F(1,32) = 8.5, p < 0.0001$ , respectively]. No other sub-tests reached significance. The pattern of significant results is therefore the same as that found in our published two factor analyses of variance.

In summary, Snowling and Hulme were entitled to claim that, despite the solid effect sizes, the case for the effectiveness of the DDAT treatment was 'not proven' owing to the imbalance in initial reading scores. The analyses of covariance fully resolve this issue, confirming that the significant differences remain even when the effects of any initial differences are removed. The treatment did therefore lead to significant effects, and our conclusions were indeed sound.

## References

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- Snowling, M. J., & Hulme, C. (2003). Lies, Damn Lies and (Inappropriate) Statistics?—A critique of claims from Reynolds, Nicolson and Hambly (2003) that DDAT is an effective treatment for children with reading difficulties. *Dyslexia: An International Journal of Research and Practice*, 9(2), 127–133.
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