

Appraisal

Correspondence: Author response to Vaughan-Graham et al

We acknowledge the letter received from Vaughan-Graham and colleagues that discusses aspects of our recent systematic review.¹ We believe it is essential that reviews are rigorously conducted and that authors are transparent in any decisions made whilst conducting the review so that, as the letter states, readers are able to interpret findings appropriately.

Many of the points raised in the letter have been addressed within the review itself.¹ As examples, we explain the rationale for conducting the review and meta-analyses in the third paragraph of the introduction on page 226 and we discuss in detail the methodological quality of the included studies in the second paragraph on page 234.¹ However, we would like to address the following points more specifically.

The review was submitted to PROSPERO (CRD42019112451) prior to the commencement of the review.

Three publications from the same study were reported in this review. We are, however, confident that we have managed the data appropriately, as outlined in the third paragraph on page 227, never including different data from the same study in one meta-analysis.¹

It was suggested that we might have omitted a study by Cooke et al (reference 6 in the letter). However, that study did not meet our inclusion criteria, because it is a review protocol involving elite athletes.

We acknowledge that we devised a criterion to determine whether each trial's intervention was, or was not, defined as Bobath (described under Intervention on page 226).¹ We used a broad definition of Bobath therapy and contacted two authors to seek additional information about the intervention within their trials. When one author provided additional information it was determined that it did not meet the inclusion criteria for this review.²

Despite recent work by Vaughan-Graham herself and others that attempts to define a more modern Bobath therapy,^{3,4} it remains very difficult for most therapists and researchers to distinguish what the changes are and precisely when these changes in Bobath occurred. If Bobath therapy has changed markedly over time, it would be useful to rename this new therapy approach. The lack of clarity around what constitutes a specific named approach highlights the fact that as a profession we need to move away from named 'approaches' and describe the specific components of an intervention. In the second

paragraph on page 234 we discuss the challenge for us as researchers, and for therapists generally, in understanding what Bobath therapy is within the included trials.¹

Complex physiotherapy interventions are often reported poorly in randomised controlled trials;⁵ this is not a new challenge for authors of systematic reviews. We challenge physiotherapy researchers to describe in detail the components of their intervention using the TIDier reporting guidelines,⁶ to help researchers and therapists understand what was delivered.

If, as suggested, an intervention such as Bobath therapy requires significant postgraduate training to be implemented successfully, then its application to a wider clinical setting is likely to be limited. We are fortunate to have high levels of evidence for many interventions in stroke rehabilitation that do not require additional postgraduate training and can therefore be implemented broadly.⁷

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