Letter to the Editor re: Pathak A, Gyanpuri V, Dev P, Dhiman NR. The Bobath Concept (NDT) as rehabilitation in stroke patients: A systematic review. J Family Med Prim Care. 2021 Nov; 10(11):3983-3990. doi: 10.4103/jfmpc.jfmpc_528_21. Epub 2021 Nov 29. PMID: 35136756; PMCID: PMC8797128

Systematic reviews are important tools for estimating the efficacy of interventions.[1] However, persistent flaws in the studies included in the systematic review by Pathak *et al.* limit the ability to draw useful clinical conclusions on the clinical application of the contemporary Bobath concept.

Our primary criticism is the lack of up-to-date theoretical and clinical frameworks used in the included studies [15-18; 20, 22-27, 31]. Therefore, the results are not reflective of how the Bobath concept is currently implemented by skilled clinicians. In addition, Pathak *et al.* only refer to the original Bobath literature from 1978, 1985, and 1990 in their introduction. A contemporary description of the Bobath concept[2] is not utilized, nor is an appreciation of its evolution. A clear understanding of the evolution of the Bobath concept would influence the eligibility criteria of the review and, thus increase its clinical relevance. Unfortunately, 13 of the 19 studies included Bobath interventions on outdated interpretations of the concept. Six studies [18, 23, 24, 27, 28, 31] refer to Bobath 1990, three studies [15, 20, 25] refer to Bobath 1978, one study [17] refers to a 1970 Bobath publication, and five studies [16, 21, 22, 26, 32] refer to sources other than Bobath or Bobath instructors. The remaining four studies [19, 29, 30, 33] refer to sources written by Bobath instructors or Bobath-trained researchers.

No study reported clinical adherence, an important aspect of study fidelity.[3] Fourteen of the 19 studies reported Bobath as the control [15-22; 25-28; 30-31], which was therefore considered standard care, and thorough consideration of the "control Bobath interventions" was lacking.[4] Only four studies [24,29,30,32] explicitly stated that the therapists responsible for the Bobath intervention had undertaken formal education in the Bobath concept. This is surprising given should a study on the effectiveness of neurosurgical interventions be undertaken, one would assume that those providing the interventions would be adequately trained. Studies investigating the effectiveness of neurorehabilitation interventions should be no different.

The overlap of studies included in two previous reviews by Diaz-Arribas *et al.* [35] and Scrivener *et al.* [36] is substantial, with eight studies [15, 17, 18, 21, 23-25, 33] being common to all three systematic reviews, four studies [19, 20, 22, 39] overlapping with Diaz-Arribas *et al.*, five studies [26-29, 32] overlapping with Scrivener *et al.*, with only two studies [16,31] exclusive to Pathak *et al.* In addition, the authors did not assess how the inclusion criteria of the different reviews may affect the reliability of the conclusions reached.

Additionally, the poor methodological quality of some of the included studies as measured by the Physiotherapy Evidence Database (PEDro) scale[5] and the discrepancies in PEDro scores compared with previous reviews [36, 35] requires consideration. Six [15, 16, 17, 21, 28, 33] of the 19 studies scored 4 or less on the PEDro scale. Although four studies [18, 19, 29, 31] scored 8 or more, only one of those [29] refers to a contemporary understanding of the Bobath concept and concluded that Bobath intervention was more efficient in improving gait velocity compared to task practice.

Based on the above, it would appear that good quality studies investigating the effectiveness of the Bobath concept are very limited. Therefore, an excellent contribution to the evidence base would be the development of good-quality Bobath intervention studies, especially considering the limited availability of rehabilitation science research funding.

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Conflicts of interest

There are no conflicts of interest.

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