Hyperbaric Oxygen Treatment

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As a senior clinician involved in the largest randomized trial of hyperbaric oxygen treatment (HBOT) for children with cerebral palsy (CP)[1] as well as in the previous pilot study on the same subject,[2] I would like to comment on the editorial by Novak and Badawi [3] published in *Annals of Neurology*. Based on the studies by Collet et al[1] and Lacey et al,[4] they seem to bring a definitive answer to questions about the effectiveness of oxygen used under hyperbaric conditions in the treatment of children with CP.

As mentioned in an editorial in the *Lancet*[5] at the time of the publication of the study by Collet et al, "Although the results did not indicate that hyperbaric oxygen had any benefit over slightly pressurized air, they showed that both groups of children improved *substantially* with respect to gross motor function, speech, attention, memory and functional skills. The researchers postulate that either the two treatments were equally effective or the mere act of participating in a trial that promoted communication with other motivated children and parents had a positive effect." The US Agency for Healthcare Research and Quality, analyzing this study, concluded that "The possibility that pressurized room air had a beneficial effect on motor function should be considered the *leading* explanation."[6]

This point is also supported by in a recent editorial in *Undersea and Hyperbaric Medicine*.[7] Mychaskiw also finds it disconcerting that this study is still held up as proof of HBOT's lack of efficacy in CP. Based on these different points, I strongly believe that it is a blatant misrepresentation to refer to Collet et al's study to claim that HBOT is not effective. In this trial, the motor improvements that were measured with the Gross Motor Function Measure (GMFM) were more important and were obtained over a shorter period than most of the changes found in any other studies evaluating the effects of recognized therapies.[8]

Lacey et al have conducted a study in which they compared 2 different hyperbaric treatments, 1 of which (14% oxygen at 1.5ATA) has never been used on CP children before.[4] Despite this condition simulating 21% oxygen at room air, this treatment must not be considered as a placebo treatment, because no one exactly knows the potential physiologic effects of this hyperbaric treatment. Furthermore, Lacey decided to end the study prematurely due to lack of measured effects. However, the change in GMFM in the HBOT group was 1.5 in 2 months, which is more than most changes observed with recognized treatments in CP.[8] It would have been impossible to reach the objective of 5% improvement in the GMFM in such a short period of time with any treatment used in children with CP. The actual study design used by Lacey would have shown, if applied to those treatments, an absence of results and might also have also led to questionably self-aborted studies.

I find it disconcerting that, without any strong evidence, Novack and Badawi can draw such drastic negative conclusions and think that they should counterbalance the overwhelming results presented in the 10 positive studies[8] conducted with HBOT on >600 children with CP.

Potential Conflicts of Interest

Nothing to report.

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DOI: 10.1002/ana.23888

Reply

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We respectfully note Dr Marois' concerns. Likewise we are interested in research that finds new, effective, and clinically meaningful treatments for cerebral palsy.

We systematically searched the published and gray literature, in Central, Medline, and ebase, with no language restrictions, as per Cochrane recommendations.[1] We sought low-bias studies, including randomized controlled trials (RCT) and controlled trials, and excluded other levels of evidence as per convention. Participants needed to have cerebral palsy and to be

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