LETTER TO THE EDITOR

Manual Therapy in Children

To the Editor:

The recent editorial by Huijbregts on manual therapy (MT) in children[1] offers an admirable overview on both the science and art of applying MT to infant and pediatric populations by a variety of clinical professions. With regards to the chiropractic literature and its final assessment of the advisability of considering manipulation as a viable treatment alternative for children, however, some of its assertions and conclusions are problematic.

To begin, several of the pediatric outcomes indicated in my monograph[2] were not subjective, in contrast to what was indicated by Dr. Huijbregts in his editorial. Otitis media outcomes, for instance, could be assessed by objective tympanograms, displaying a distinct pattern of either positively or negatively configured eardrums depending upon the type of agent present in the Eustachian tube[3]. As reported recently[4] asthmatic patients specifically responded to spinal manipulation with distinct hormonal (Cortisol) and immunological (Immunoglobulin A) changes. Finally, colicky infants displayed a substantial decrease in the number of hours of crying as reported by their parents following spinal manipulation but not medical treatment[5]. All these parameters (tympanography, determination of hormonal and immunological levels, and time measurement) are clearly objective rather than subjective benchmarks.

Segmental specificity remains a well-taken issue as raised by Dr. Huijbregts concerning issues of motion palpation[5]. However, it also needs to be recognized when identifying clinical responses, for it is the lack of a clear indication as to *which* regions of the spine were adjusted when comparing the divergent results of Wiberg et al[6] and Olafsdottir et al[7], as done in Huijbregts' editorial but without regard to this particular issue.

Finally, one must review the relevance of randomized clinical trials to the application of clinical practice, as evident in the final paragraph of the editorial--but lacking the more complete perspective. Given the fact that fastidious RCTs are performed in a strictly defined setting with specialized populations, one must realize that not all patient groups under all circumstances can ever be expected to have been previously evaluated in an RCT. Such is the situation, for example, in the common use of off-label medications that have not been adequately tested--let alone for the specific populations to which they are applied[8]. This point, in particular, creates a double standard when Huijbregts' editorial berates what is considered to be the premature use of MT in children[1]. It must never be forgotten, also, that the very existence of RCTs is dependent upon the preliminary data gathered from case reports in the doctor's office.

Questions about the safety of the use of MT in children raised by the editorial apparently are not a concern for Biedermann, who in addition to his article cited[9] has edited an entire textbook devoted to manual therapy in pediatric populations[10]. In comparison to MT, far more common complications have been reported in the use of vaccines, of obvious relevance when considering treatment alternatives for children[11,12]. For these reasons, it would seem ill advised to discuss any potential harms of MT out of proportion to other common pediatric procedures and especially to deny what appear to be clear benefits of MT to populations in the early years of their lives.

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