

Differing Views in Pediatric Chiropractic Research

Claudia Anrig, DC

The journal *Chiropractic & Osteopathy* recently [published a thematic series](#) on the chiropractic care of children. While I'm glad that many academics in our profession are interested in the growing arena of chiropractic pediatrics, I found the thematic series limited in the number of participant authors and feel it would have been better served with more differing views.

Dr. Joel Alcantara is the director of research for the International Chiropractic Pediatric Association (ICPA). In this capacity, he directs all research activities and oversees the largest and most successful practice-based research networks in chiropractic. Recently, Dr. Alcantara and colleagues [published a landmark study](#) in the *Journal of Alternative and Complementary Medicine* on chiropractic care for children. In addition to characterizing pediatric chiropractic, they estimated that approximately 80 million pediatric visits are made to chiropractors each year. The authors acknowledge that this may be an overestimation, but it does support the study by Barnes and colleagues from the National Institutes of Health that chiropractic is the most popular CAM-based therapy for children. In this interview with Dr. Alcantara, I asked him to share his thoughts on the thematic series in *Chiropractic & Osteopathy*.

Before I ask your opinion regarding the articles published in *Chiropractic & Osteopathy*, can you give us a synopsis of the thematic series? *Chiropractic & Osteopathy* commissioned a thematic series on the chiropractic care of children and invited their key people in the field of pediatric chiropractic to provide an up-to-date review on the chiropractic care of children. You have Sharon Vallone and her colleagues providing their opinion on the chiropractic approach to the management of children; Ferrance and Miller addressing the diagnosis and chiropractic management of musculoskeletal conditions in children and adolescents; Kim Humphreys discussing the safety of pediatric chiropractic; Fay Karpousiz and her colleagues exploring [care of the child with ADHD](#); and finally, [Leboeuf-Yde and Hestbaek](#) addressing the question: "Is research enough?"



Overall, the articles addressed aspects of pediatric chiropractic we should all be aware of. I didn't necessarily agree with all the opinions made, but the editorial commentary by [Simon French](#), [Bruce Walker](#) and [Stephen Perle](#) was especially disturbing from my point of view as a researcher and a practitioner.

In their article, they posed the question: "Should we be treating children at all?" and based this on the limited amount of research currently available. Please explain why you disagree with the position they took in their editorial commentary? Everyone would agree that the chiropractic profession needs more research. In terms of pediatric chiropractic, this is painfully true. French and colleagues questioned, given the sparse research evidence available on the chiropractic care of children, whether we should be treating children at all. They caution clinicians who accept without question the suggestion that a trial of chiropractic care is warranted, particularly with children presenting with non-musculoskeletal conditions.

I don't disagree with this cautionary statement. I think it prudent for every clinician to critically appraise the literature in the context of their clinical experience and expertise and the request of the patient or parent. This is essentially evidence-based practice. However, they go a bit further and, using the chiropractic care of children with colic as an example, quote the Bronfort Report by stating, "There is no evidence that chiropractic care for infant colic is more effective than sham therapy." They then propose: "It may also be reasonable to suggest that a short trial of 'placebo treatment' is warranted." Their comment was not only unjustified, but irresponsible. French and colleagues acknowledge the principles of evidence-based medicine, but they throw it out the window if it doesn't support their point of view.

The decision to pursue or recommend a trial of chiropractic care is based on many factors, with safety and effectiveness at the core of this decision and the principles of evidence-based medicine providing the overall guiding principle. So, what is a clinician to do when the research is not available? Evidence-based medicine allows you to rely on your clinical expertise and the needs and wants of the patient or the parent (in the case of pediatric chiropractic).

Sackett and colleagues, in defining evidence-based medicine, stated: "Evidence-based medicine is not restricted to randomized trials and meta-analyses. It involves tracking down the best external evidence with which to answer our clinical questions." When RCTs are not available, they recommend to "follow the trail to the next best external evidence and work from there." Sackett and colleagues said it best: "External clinical evidence can inform, but can never replace, individual clinical expertise, and it is this expertise that decides whether the external evidence applies to the individual patient at all and, if so, how it should be integrated into a clinical decision." This is true not only in chiropractic, but also for orthodox medicine.

Going back to the colic issue, French and colleagues based their comment on the recent review by Bronfort, et al., on the effectiveness of manual therapies for various conditions. First of all, I believe Bronfort and colleagues are wrong in their conclusion regarding the evidence for colic. If one closely examines the clinical trials on chiropractic SMT and infantile colic, you will find that no study exists comparing chiropractic SMT versus sham therapy.

Now, sham therapy has been defined as a procedure that closely mimics the active procedure, but remains inert with respect to the specific effects of the active treatment. Wiberg and colleagues compared the effects of chiropractic SMT versus simethicone, a common medication for infantile colic. Browning and colleagues compared the effects of chiropractic SMT and occipito-decompression in infantile colic. Finally, Olafsdottir and colleagues compared an unproven chiropractic technique versus "no treatment."

Wiberg and colleagues found chiropractic superior to simethicone; Browning and colleagues found both techniques decreased the hours of crying compared to baseline; and Olafsdottir and colleagues found their chiropractic technique as ineffective. So, the bottom line is, there is some evidence in support of chiropractic care for infantile colic.

If you look at safety, the two major studies examining the safety of pediatric SMT are the systematic review of the literature by Vohra and colleagues and our publication on the safety and effectiveness of pediatric chiropractic, the latter of which is based on our findings from the ICPA practice-based research network. Vohra essentially found (in more than 100 years of literature) only a handful of cases documenting adverse events with chiropractic SMT in children. When you closely examine the literature, these involve mostly minor adverse events like soreness and stiffness at the site of the adjustment. It was difficult to find fault with the chiropractor since the patients described had a pre-existing condition and/or suffered from trauma with symptoms corresponding to their supposed adverse event.

Our ICPA study surveyed chiropractors and parents of children receiving chiropractic care. The chiropractors indicated three adverse events in 5,438 office visits involving the care of 577 children. The parents indicated two adverse events in 1,735 office visits involving the care of 239 children.

Compare this to the medical care of children with infantile colic. The literature essentially indicates that medications like simethicone are no more effective than placebo and are associated with severe

adverse events. Add to this the research that indicates parents may have thoughts of infanticide due to their crying baby. As a researcher and clinician, I think a trial of chiropractic care is warranted, in line with the principles of evidence-based medicine and the principles of biomedical ethics. That is, respecting a patient's autonomy, avoiding harm, placing their interests and well-being first and allowing them access to essential care like chiropractic.

As a clinician, you've essentially explained the principles of evidence-based medicine and affirmed what thousands of chiropractors are doing in practice. What about the other articles in the thematic series? Do you have any comments about them? When it comes to safety, we need to keep doing the research. [As Dr. Humphreys pointed out](#) in his article, severe adverse events are rare in pediatric chiropractic, but that may also be underreported. We need to find out what these adverse events are, what's causing them and develop a means of preventing them. We need to continue our research on the safety of pediatric chiropractic. The ICPA PBRN has been established to continue the research on pediatric safety. It's our ethical and professional responsibility.

Of course, as a researcher, I agree with Drs. Leboeuf-Yde and Hestbaek that we need more research, not only in quantity but also in quality. We differ in our direction and focus for research, but this is understandable given our different interests and methodologies.

What about the article on the management approach to children? [Dr. Vallone and colleagues](#) are respected members of the chiropractic community and key in education, research and clinical practice in the field of pediatric chiropractic. Given their expertise, I was quite surprised that they would recommend "hypermobility" as an absolute contraindication for manual therapy for children. They are essentially recommending that chiropractic SMT should not be performed on children. If you look at the unique biomechanical features of the pediatric spine, hypermobility is one, in addition to malleability, changing spinal contours, an immature neuromusculoskeletal system, etc. Essentially, this would eliminate the practice of pediatric chiropractic as we know it, since the chiropractic adjustment is the primary approach to the care of children.

On another point: Vallone, et al., recommend that the inversion maneuver for infants and young children is a relative contraindication. My question is: On what basis do these authors make this recommendation? The scientific literature does not support them in terms of adverse events due to the procedure. Furthermore, their stated reasons for not performing the procedure (possible undiagnosed clinical entities) just does not make clinical sense. It takes clinical uncertainty to a different level and how it affects one's decision to perform a diagnostic test or treatment procedure.

Finally, they qualified that, based on their experience, mechanical lesions are "expected to respond within approximately three to six treatments, depending on the duration of the problem." We need to look at this and accept that this is based on their clinical experience and not necessarily the experience of all chiropractors. Additionally, comments like this are unfounded given the heterogeneity of the clinical presentations that chiropractors address in the care of children. If there's one thing certain about clinical practice, it is clinical uncertainty.

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