

### ■ Manual Therapy in Children: Role of the Evidence-Based Clinician

Like many of you reading *JMMT*, I had always associated the use of manual therapy (MT) interventions with musculoskeletal complaints in adults. Although I have successfully treated orthopaedic problems in some adolescents with MT, until a recent visit to Europe it had never so much as crossed my mind to use this intervention in young children, let alone infants. However, in several Northern European countries, a significant number of our medical and physical therapy colleagues are treating infants, 0-12 months old, with MT interventions based on an etiologic model that links a great number of [non] musculoskeletal signs and symptoms to functional disorders of the upper cervical spine<sup>1</sup>.

Biedermann<sup>1</sup> introduced a two-category diagnostic classification that proposes that birth trauma-induced upper cervical dysfunction has immediate and—if left untreated—long-term consequences. Proposed signs and symptoms of the Kinetic Imbalance due to Suboccipital Strain (KISS) syndrome include but are not limited to torticollis, frequent vomiting, problems swallowing, scoliosis, plagiocephaly, facial asymmetry, and colic with excessive crying<sup>2</sup>. The second category of KISS-induced Dyspraxia and Dysgnosis (KIDD) syndrome has been associated with slow development of fine and gross motor skills, poor posture and equilibrium, delayed language development, restlessness, and insecurity in older children<sup>2</sup>. KISS-syndrome has even been implicated as a causative factor in attention deficit disorder (ADD)<sup>3</sup>. Biedermann<sup>1</sup> proposed impulse manipulation of the upper cervical spine in the direction of sidebending and, at times, rotation as the treatment for both syndromes. He also noted that scientific methods normally used for verification of short-term effects of MT interventions in adults have only limited usefulness in determining the effects of the manipulative treatment of children with KISS- and KIDD-syndrome because these tests fail to capture the long-term interdependencies proposed in his etiologic model<sup>1</sup>.

We know that the newborn cervical spine is insufficiently able to protect the spinal cord, vessels, nerves, and brain from traction and rotation forces<sup>4</sup>. Koch<sup>5</sup> reported facial and whole-body flushing, diaphoresis, crying, bradycardia, and temporary respiratory arrest in children treated for KISS-syndrome with impulse manipulation. Severe bradycardia was seen more frequently in the group of 1- to 3-month-old infants<sup>5</sup>. Perhaps relevant even despite the admitted difference in techniques applied, very similar symptoms have been reported in an infant who died after Vojta therapy to correct a congenital torticollis<sup>6</sup>. The lack of relevant outcome studies<sup>2,7</sup>, the assertion that the etiologic model does not lend itself to such studies<sup>1</sup>, and the suggested potential for adverse effects<sup>2</sup> has understandably led to a heated debate in multiple countries on the topic of KISS and KIDD-syndrome<sup>2,7,8</sup>.

However, not all MT clinicians who treat children ascribe to this etiologic model. Nor do these clinicians all use thrust-type manipulation techniques. Osteopathic pathophysiologic models for functional problems in children center on cranial entrapment neuropathies affecting cranial nerves IX-XII but they also attribute a role to dysfunctions affecting the pelvic region and the thoraco-abdominal diaphragm. These interventions generally consist of gentle, non-thrust techniques<sup>9</sup>. Chiro-

practic interventions in children include thrust manipulation, craniosacral techniques, and nutritional counselling<sup>10</sup>. Most physical therapists treat children, even when diagnosed with KISS-syndrome, with non-thrust interventions<sup>7</sup>.

Rosner<sup>10</sup> addressed the pathophysiologic rationale and research behind infant and child chiropractic care. As this monograph was not a systematic review of the literature, many of the studies included were of a quasi-experimental design. Some studies reported positive albeit mainly subjective outcomes for chiropractic management of otitis media, colic, nocturnal enuresis, asthma, scoliosis, and headache. Rossner noted that the evidence for chiropractic management of epilepsy, autism, and ADD and hyperactivity disorder was totally anecdotal. The quasi-experimental design obviously does not allow for inferring a cause-and-effect relationship, but it should be noted that reported cure rates for otitis media with chiropractic far exceeded established rates for resolution based on natural history<sup>11</sup>. However, chiropractic cure rates did not compare favorably to natural history for nocturnal enuresis and infantile colic<sup>12,13</sup>.

The placebo effect is an obvious and important possible explanation for the effect of any MT intervention. In a randomized clinical trial (RCT) of chiropractic versus placebo treatment for infantile colic in which the parents providing the subjective outcome measure were blinded to the treatment received, no significant between-group differences were noted<sup>14</sup>. In contrast, another RCT without this blinding showed superior effects of manipulation over medication for colic<sup>15</sup>. Positive effects on patient-reported but not on objective outcome measures led Bronfort et al<sup>16</sup> to suggest that improvements from chiropractic in patients with pediatric asthma might not be related to manipulation but rather to other aspects of the clinical encounter.

Within the evidence-based practice paradigm, the clinician combines research data on diagnostic accuracy, outcomes, and risk of harm with clinician expertise and patient (or in this case, parent) values when choosing a management strategy. We can justifiably criticize the tests needed for a segment-specific MT diagnosis in adults<sup>17</sup>; to my knowledge, no research has even been done on reliability, validity, or responsiveness of segmental motion tests in children. As noted above, outcomes research is limited and seems to indicate either an important placebo effect or no benefit of intervention over natural history. A systematic and comprehensive review with regard to the risk of harm faces substantial challenges<sup>18</sup>. Relying on case reports<sup>6,19</sup> and other anecdotal evidence<sup>7</sup> when discussing risk of harm is subject to criticism.

So what is the role of the evidence-based clinician faced with desperate parents, an at-times seemingly plausible pathophysiologic rationale, a lack of anything but anecdotal evidence, and uncertainty regarding risk of harm to these young patients? Brand et al<sup>2</sup> suggested that MT interventions in children with signs and symptoms indicative of the proposed KISS-syndrome should not be used outside the context of randomized, double-blind controlled trials. Should we all exercise this same level of caution, especially considering the age of the little patients involved and their inability to make informed decisions? Is it ethical for an evidence-based clinician to continue to advocate and provide unproven and potentially harmful treatments with parents desperate to find help for their children?

On the other hand, we have no clear evidence of harm. Considering the diversity of MT approaches in this area, research on outcome and harm for one treatment approach cannot be applied to all. And absence of evidence with regard to efficacy is not evidence of absence. It is clearly time for the advocates of MT in children to describe and delineate their approach and provide society and the profession with high-quality research evidence to substantiate claims of efficacy and safety. Until that time, as a clinician, I will continue to educate parents based on the lack of available evidence regarding outcome and the potential for harm. And most importantly, as a parent I will not expose my children to such interventions.

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