

As the premier scholarly publication of the osteopathic medical profession, *JAOA—The Journal of the American Osteopathic Association* encourages osteopathic physicians, faculty members and students at colleges of osteopathic medicine, and others within the healthcare professions to submit comments related to articles published in the *JAOA* and the mission of the osteopathic medical profession. The *JAOA*'s editors are particularly interested in letters that discuss recently published original research.

Letters to the editor are considered for publication in the *JAOA* with the understanding that they have not been published elsewhere and that they are not simultaneously under consideration by any other publication. Although the *JAOA* welcomes letters to the editor, readers should be aware that these contributions have a lower publication priority than other submissions. As a consequence, letters are published only when space allows.

All accepted letters to the editor are subject to editing and abridgement. Letter writers may be asked to provide *JAOA* staff with photocopies of referenced material so that the references themselves and statements cited may be verified.

Readers are encouraged to prepare letters electronically in Microsoft Office Word (.doc) or in plain (.txt) or rich text (.rtf) format. The *JAOA* prefers that readers e-mail letters to jaoa@osteopathic.org. Mailed letters should be addressed to Gilbert E. D'Alonzo, Jr, DO, Editor in Chief, American Osteopathic Association, 142 E Ontario St, Chicago, IL 60611-2864. Mailed submissions and supporting materials will not be returned unless letter writers provide self-addressed, stamped envelopes with their submissions.

Letter writers must include their full professional title(s) and affiliation(s), complete preferred mailing address, day and evening telephone numbers, and preferred fax number and e-mail address. In addition, writers are responsible for disclosing financial associations and other conflicts of interest. No unsigned letters will be considered for publication.

Although the *JAOA* cannot acknowledge the receipt of letters, a *JAOA* staff member will notify writers whose letters have been accepted for publication.

All osteopathic physicians who have letters published in the *JAOA* receive continuing medical education (CME) credit for their contributions. Writers of original letters receive 5 hours of AOA Category 1-B CME credit. Authors of published articles who respond to letters about their research receive 3 hours of Category 1-B CME credit for their responses.

Survey Results: OMT and CAM

To the Editor:

We would like to highlight a recent study by the National Center for Complementary and Alternative Medicine, a branch of the National Institutes of Health, containing important and relevant information for the osteopathic medical community, including osteopathic physicians, educators, and students.

In the study, which is based on data

collected in the Centers for Disease Control and Prevention's 2007 National Health Interview Survey (NHIS), Barnes et al¹ identify a large number of metrics related to the use of complementary and alternative medicine (CAM), including osteopathic manipulative treatment (OMT), in the United States.

A review of these data¹ allow for greater understanding of the demographic characteristics of individuals who use CAM, as well as the types of

CAM most likely to be used and the illnesses and conditions for which CAM is used. These data also allow for comparisons with 2002 data.² Therefore, changes in CAM use in the United States between 2002 and 2007 can be analyzed.

The 2007 NHIS data incorporated results of 23,393 completed interviews with adults aged 18 years or older.¹ From a list of 36 types of CAM, 10 of which required the services of a practitioner (eg, acupuncturist, chiropractor, osteopathic physician, traditional healer), nearly 4 of 10 adult respondents (38.3%) reported using some form of CAM within the previous 12 months.¹

Among all CAM types, "chiropractic or osteopathic manipulation" ranked fourth in use by respondents; one or the other of these types of manipulation was used by 8.6% of surveyed adults during the previous 12 months.¹ (Unfortunately, the survey results did not distinguish between chiropractic manipulation and OMT.) The use of chiropractic or osteopathic manipulation ranked behind that of "nonvitamin, nonmineral, natural products" (used by 17.7% of respondents), "deep breathing exercises" (used by 12.7% of respondents), and "meditation" (used by 9.4% of respondents).¹

These data suggest that OMT is among the most widely used types of CAM, though—surprisingly—not as widely used as some types of CAM that have questionable therapeutic value for such common conditions as neck, back, and joint pain.

The reasons that the use of OMT ranks behind the other CAM types are not clear. We believe that these reasons may include lack of awareness, greater difficulty in accessing OMT, financial cost—or some combination of these factors. In light of the finding that almost 40% of surveyed adults reported using some form of CAM in 2007,¹ these data indicate that greater

public awareness of OMT is critically needed, as is greater ease of access to OMT.

According to the 2007 NHIS survey,¹ individuals with diverse conditions and illnesses—including anxiety, arthritis, diabetes mellitus, fibromyalgia, hypertension, and migraine headache—used CAM during the previous 12 months. The conditions and illnesses reported as most commonly affecting individuals who used CAM were “back pain or problems” (affecting 17.1% of respondents), “neck pain or problems” (affecting 5.9% of respondents), and “joint pain or stiffness or other joint conditions” (affecting 5.2% of respondents).¹ Combining these conditions with “arthritis” and “other musculoskeletal conditions,” which were reported by 3.5% and 1.8%, of respondents,¹ respectively, reveals that more than 30% of adults who used CAM in 2007 did so to manage musculoskeletal-related problems and pain.

Thus, these results indicate that though more than 30 of 100 CAM users have musculoskeletal problems and pain, fewer than 9 of 100 CAM users seek OMT. This finding, in turn, suggests that individuals with musculoskeletal problems and pain constitute an important patient population for the osteopathic medical profession to target with greater dissemination of information regarding the therapeutic potential of OMT.

We believe that other data contained in the 2007 survey results reported by Barnes et al¹ would also be of interest to members of the osteopathic medical community. Such data include results related to CAM use by children; the age, race, and other demographic characteristics of CAM users; and changes in CAM use between 2002 and 2007. (In the 2002 NHIS survey,² however, respondents were asked about their use of chiropractic manipulation but not about their use of osteopathic manipulation.) In addition, data

on lifestyle and risk factors (eg, alcohol consumption, body weight, tobacco use) of individuals who use CAM are documented in both studies by Barnes et al,^{1,2} offering greater understanding of those seeking CAM care.

An important and ongoing challenge for the osteopathic medical profession is the promotion of osteopathic medical philosophy, including its rich tradition and history as well as the science of OMT. How can progress in this important mission be evaluated and measured? One way is through the examination of empirical data on the use of CAM, such as that reported by Barnes et al.^{1,2}

Designing novel approaches for increasing public awareness of OMT will rely on further studies that use this modality in the greater context of CAM—including studies that distinguish between OMT and chiropractic manipulation. Increasing public awareness of OMT will also require increased monitoring of possible changes in the attitudes and healthcare needs of CAM users over time.

We strongly believe that the osteopathic medical community must take an active role in, and perhaps be at the forefront of, this important area of study.

Raddy L. Ramos, PhD
Sanjeev Sharma, BA
Danielle M. Lipoff, MS, OMS III
 Department of Neuroscience and Histology,
 New York College of Osteopathic Medicine of
 New York Institute of Technology, Old West-
 bury

References

1. Barnes PM, Bloom B, Nahin RL. Complementary and alternative medicine use among adults and children: United States, 2007. *Natl Health Stat Report*. December 10, 2008;1-24. Available at: <http://nccam.nih.gov/news/2008/nhsr12.pdf>. Accessed June 4, 2009.
2. Barnes PM, Powell-Griner E, McFann K, Nahin RL. Complementary and alternative medicine use among adults: United States, 2002. *Adv Data*. May 27, 2004;1-19. Available at: <http://www.cdc.gov/nchs/data/ad/ad343.pdf>. Accessed June 4, 2009.

Brachial Plexus Injuries in Neonates

To the Editor:

I read with great interest—and also dismay—the February clinical practice article by David C. Mason, DO, and Carman A. Ciervo, DO,¹ regarding the use of osteopathic manipulative treatment (OMT) for neonates with brachial plexus injuries.

In the “Treatment Options” section of their article, Drs Mason and Ciervo¹ do not readily distinguish “muscle strains” from plexopathy. I fear that such lack of clarity could mislead the reader.

Moreover, in the “Osteopathic Manipulative Treatment” section of the article,¹ I was concerned by the authors’ citation and application of three articles that I wrote on the treatment of adults with thoracic outlet syndrome (TOS).²⁻⁴

It must be clearly understood that brachial plexus injuries in neonates are *not* considered a form of TOS. Drs Mason and Ciervo¹ are correct to note that “the etiologic processes involved in thoracic outlet syndrome and neonatal brachial plexus injuries are clearly different.” In fact, the diagnosis of neonatal brachial plexus injuries relies on observation of substantial loss of limb function,⁵ which is quite the opposite of conditions typically seen in patients with TOS. The primary pathophysiologic condition that characterizes TOS is a chronic mild compression, whereas brachial plexus injury in neonates involves acute stretch disruption of axons from traction.⁵ The latter injury does not result in tight tissues amenable to myofascial release, but requires relative rest and time for axonal regeneration. Any aggressive OMT maneuvers to release the thoracic outlet in a neonate would inherently risk further irritation or disruption of already injured and fragile axons. Thus, such maneuvers need to be avoided at all

costs in neonates.

It is unfortunate that Drs Mason and Ciervo¹ suggest applying the myofascial release approach I described for adults with TOS to neonates who have the Erb-Duchenne type of paralysis (ie, brachial plexus injuries). The approach described for adults with TOS was never intended for use in neonates with brachial plexus injuries. The initial description of the form of TOS covered in my article on pathology and diagnosis² specified that the patients in these cases had no documented nerve injuries. Many of the cases were considered “disputed” or “nonspecific neurogenic” TOS, because—despite persistent symptoms—no nerve damage could be demonstrated, and most electromyographic examinations produced normal results.²

It is also disturbing that Drs Mason and Ciervo¹ characterize my manual approach as “gentle myofascial stretching.” Nothing could be further from an accurate description of the type of OMT that I used, which I described as an “aggressive...powerful form of myofascial release manipulation and stretching.”^{2,3} There is nothing gentle about these techniques, which are expected to produce substantial discomfort in the patient as part of the release process.

The manual approach I described in my article on treatment³ involved “deep myofascial release” and “vigorous, controlled stretch” maneuvers that “require greater stretching force...to break up adhesions.” These maneuvers are designed to be applied to adults who can provide immediate verbal feedback regarding possible effects of treatment, such as numbness or tingling and perceived discomfort. Such verbal feedback, which is essential for the operator to optimally monitor response to treatment, would obviously be lacking in the neonate population. Furthermore, these maneuvers typically irritate the neurovascular structures of patients and would be harmful when applied to nerve tissues that are already dam-

aged—as in neonates with brachial plexus injuries. The maneuvers are intended to treat patients with irritative forms of TOS—not the true neurogenic form.

In conclusion, I advise exercising extreme caution in any attempts to apply vigorous OMT to neonates, whether these individuals have documented nerve injuries or not. The treatment approach to TOS is completely different from the treatment approach to neonatal brachial plexus injury—and this difference needs to be recognized.

Benjamin M. Sucher, DO

Medical Director, EMG Labs of Arizona Arthritis and Rheumatology Associates, Paradise Valley, Ariz

References

1. Mason DC, Ciervo CA. Brachial plexus injuries in neonates: an osteopathic approach. *J Am Osteopath Assoc.* 2009;109:87-91. Available at: <http://www.jaoa.org/cgi/content/full/109/2/87>. Accessed April 10, 2009.
2. Sucher BM. Thoracic outlet syndrome—a myofascial variant: part 1. Pathology and diagnosis. *J Am Osteopath Assoc.* 1990;90:686-696,703-704.
3. Sucher BM. Thoracic outlet syndrome—a myofascial variant: part 2. Treatment. *J Am Osteopath Assoc.* 1990;90:810-812,817-823.
4. Sucher BM, Heath DM. Thoracic outlet syndrome—a myofascial variant: part 3. Structural and postural considerations [published correction appears in *J Am Osteopath Assoc.* 1993;93:649]. *J Am Osteopath Assoc.* 1993;93:334,340-345.
5. Wilkins KE. Special problems with the child's shoulder. In: Rockwood CA, Matsen FA. *The Shoulder*. Philadelphia, Pa: WB Saunders Co; 1990:1033-1087.

To the Editor:

In the clinical practice article by David C. Mason, DO, and Carman A. Ciervo, DO,¹ the authors comment on the use of osteopathic manipulative treatment (OMT) for neonates who have Erb-Duchenne palsy, or brachial plexus injuries. The authors thoroughly discuss the anatomic and diagnostic characteristics of this condition. They are also to be complimented for their diagnostic skills. As an osteopathic physician who has participated in the care of infants for more than 15 years, I doubt that many other practitioners could perform the palpatory, visual, and range-of-motion examinations in the unsedated infant that are described by the authors.¹ Certainly, such exam-

inations are beyond my capability. Nevertheless, the potential for using OMT in this condition is interesting.

Unfortunately, Drs Mason and Ciervo¹ present no evidence regarding why OMT techniques useful for thoracic outlet syndrome should also be considered efficacious for Erb-Duchenne palsy. Although their hypothesis that myofascial release can ameliorate this condition is compelling, they present no data—even from their clinical practice—that this treatment approach would be of benefit.

Given the prevalence of brachial plexus injuries in the neonate population² and the novelty of the osteopathic medical approach in such cases, it should be rather simple to undertake a prospective, randomized controlled trial of the authors' suggested techniques with well-defined endpoints to validate their assumptions, which are currently unfounded.

The authors are correct in noting that brachial plexus injuries carry emotional weight for parents and constitute a source of malpractice litigation.¹ In light of this consideration, it is of particular importance that OMT modalities undergo the same scrutiny of the scientific method that is required of more conventional treatment options.

Finally, I fail to understand the point of including unsupported and nonreferenced statements in *JAOA*—*The Journal of the American Osteopathic Association* about the alleged benefit of therapeutic touch to “facilitate the body's innate ability to heal.”¹ Such statements are not worthy of scientific journals and do nothing to enhance the wider medical community's acceptance of osteopathic medicine.

All scientific discovery begins with unproven assumptions, but it is incumbent on those of us in the osteopathic medical profession to present properly tested and validated data before encouraging the use of new treatment modalities in our patients.

(continued on page 377)

(continued from page 348)

George Mychaskiw II, DO

Professor, Department of Anesthesiology;
Director, Pediatric Anesthesiology, Blair E.
Batson Hospital for Children, University of Mis-
sissippi School of Medicine, Jackson

References

1. Mason DC, Ciervo CA. Brachial plexus injuries in neonates: an osteopathic approach. *J Am Osteopath Assoc.* 2009;109:87-91. Available at: <http://www.jaoa.org/cgi/content/full/109/2/87>. Accessed April 10, 2009.
2. Foad SL, Mehlman CT, Ying J. The epidemiology of neonatal brachial plexus palsy in the United States. *J Bone Joint Surg Am.* 2008;90:1258-1264.

Response

Dr Sucher seems to express concern primarily about two topics in our clinical practice article¹—one related to our recommendations on the use of osteopathic manipulative treatment (OMT) in neonates, the other to our citations of three articles that he wrote. We would like to take this opportunity to respond to these concerns. Afterward, we will respond to Dr Mychaskiw's letter.

In regard to Dr Sucher's first concern, he states that we "do not readily distinguish 'muscle strains' from plexopathy." He further implies that we do not understand "that brachial plexus injuries in neonates are *not* considered a form of TOS [thoracic outlet syndrome]." However, the following complete statement from our article¹ (which Dr Sucher quotes only in part) is a clear acknowledgment that brachial plexus injuries are not of the same etiology as TOS—though the anatomic area is the same in both conditions:

Although the etiologic processes involved in thoracic outlet syndrome and neonatal brachial plexus injuries are clearly different, the osteopathic principle of restoring form to improve function—assisting in the body's natural ability to heal itself—suggests that this OMT technique may improve neonatal function in patients with less severe injury as well.

As this statement suggests, it is important to apply basic osteopathic princi-

ples in the treatment of patients with either condition. The application of such principles is what makes osteopathic physicians unique.

Later in our article,¹ we discuss muscle strains and the mechanisms that may lead to somatic dysfunction:

Muscle strains cause a reflexive relative shortening of the affected muscles, which acts as a protective mechanism. Therefore, adults and infants alike may have hypertonic muscles, reduced range of motion, and tissue edema.

Stretch injury in neonates as in adults initiates inflammatory response as well as myospasm and scar tissue formation. In addition, when used, involuntary splinting also results in limited range of motion that will require adjunctive exercise.

Treatment of any patient with a muscle strain should be directed toward restoring symmetry and removing areas of potential nerve impingement by addressing diagnosed somatic dysfunctions.

Furthermore, we carefully stratify the levels of treatment that are appropriate for patients with brachial plexopathy in the following passage¹:

The mainstay of treatment for neonates and infants with brachial plexus injury is conservative, particularly when no evidence of substantial vascular compromise or motor loss is present.² Standard treatment options include splinting and range-of-motion exercises.³

For neonates with severe injuries, such as nerve avulsion or rupture, invasive treatment options such as neurolysis or nerve transfer may be required. Infants with mild injuries who have not responded to standard treatment by age 3 months may also need surgical attention.^{4,5}

A fundamental point that must be respected when using OMT is that there are indications and contraindications for its use. The only acceptable indication for which to use OMT is to remove

somatic dysfunction. The use of OMT is appropriate only after a complete physical examination, as described in our article,¹ and identification of any TART (Tissue Texture Abnormality, Asymmetry, Restriction of Motion, Tenderness) findings associated with somatic dysfunction. If there are no indications for using OMT in particular cases of brachial plexopathy or TOS, each of these conditions can be managed with conservative and surgical approaches—as widely recommended in the literature.

Dr Sucher's second concern is expressed by his statement, "Moreover, in the 'Osteopathic Manipulative Treatment' section of the article,¹ I was concerned by the authors' citation and application of three articles that I wrote on the treatment of adults with [TOS].⁶⁻⁸" We did not intend to state that one should consider using in neonates the same aggressive, often painful direct myofascial stretching techniques used to treat adults. Dr Sucher clearly did not suggest using gentle techniques in his articles on the treatment of adults with TOS.⁶⁻⁸ However, during the editing process, the context of our original, intended meaning was inadvertently altered. A formal correction of this error appears on page 388.

In light of these errors, we understand Dr Sucher's concern. Our intention was to suggest that the osteopathic physician assess the neonate patient for somatic dysfunction and then consider using gentle myofascial stretching, along with careful patient monitoring, to remove the somatic dysfunction and restore normal anatomic relationships around the thoracic inlet area. The corrected version of our article more accurately reflects our original intent.

Common sense and constant monitoring of a patient's condition should direct the osteopathic physician's initial decision to use OMT, and tactile and visual feedback should direct the application of OMT. To blindly apply any technique to a patient without such

careful monitoring is negligent. Our February article expressed these concepts in the following paragraph¹:

Stretching of any myofascial structure on the restricted side can be achieved by restricting motion of one attachment and applying a gentle force longitudinally through hypertonic structures. ... To reduce the possibility of injury, physicians should constantly monitor the tissues. In addition, osteopathic physicians should palpate for tissue texture changes or muscle spasms and watch for patient response to treatment such as grimacing or analgesic posturing.

Any physician guided by the basic tenets of osteopathic medicine will not simply use a “cookbook” approach to either brachial plexopathy or TOS. The complete physical examination and diagnosis of somatic dysfunction will allow the osteopathic physician to develop a rational individualized treatment approach for each patient. This treatment may include OMT in certain cases but not in others. The treatment technique chosen in each situation is relevant only to that patient.

Regarding Dr Mychaskiw’s letter, he begins with a backhanded compliment that is unnecessary:

They are also to be complimented for their diagnostic skills. As an osteopathic physician who has participated in the care of infants for more than 15 years, I doubt that many other practitioners could perform the palpatory, visual, and range-of-motion examinations in the unseated infant that are described by the authors.¹ Certainly, such examinations are beyond my capability.

We note that, in a previous letter to the editor, Dr Mychaskiw⁹ wrote: “In my practice of pediatric cardiac anesthesiology, I do not use OMT.” Thus, perhaps he should not be expected to have the palpatory or observational skills needed to perform the examinations we described in our article.

Dr Mychaskiw next expresses concern about the lack of data presented in our article. Often, an idea for a research project is stimulated by a discussion, such as that represented by the current series of letters. We believe it would be quite appropriate for someone who may agree with our approach or a modification thereof to perform the research that Dr Mychaskiw is requesting.

However, if every aspect of clinical practice had to be based on double-blind placebo-controlled trials, there would be no room for individualized treatment or innovation leading to new research. A profession that allowed only the application of the results from such studies would quickly become stagnant. Furthermore, disciplines in the profession would become susceptible to takeover by less qualified paraprofessionals who could input data on signs and symptoms into a computer, read the resulting information on the computer screen, and mindlessly apply any treatment suggested on screen.

We do agree that the osteopathic medical profession should continue to support clinical research into the physiologic mechanisms and clinical outcomes associated with applying OMT adjunctively to patients who have identifiable somatic dysfunction.

We found the following comment by Dr Mychaskiw to be especially disturbing: “Finally, I fail to understand the point of including unsupported and nonreferenced statements in *JAOA*—*The Journal of the American Osteopathic Association* about the alleged benefit of therapeutic touch to ‘facilitate the body’s innate ability to heal.’”¹ We did not believe it was necessary to reference a statement that is so widely accepted by our profession that it is considered a basic tenet of osteopathic medicine. We believe that any DO who has remained in touch with the osteopathic medical profession would be aware of these tenets, which can be found in the latest edition of *Foundations for Osteopathic Medicine*.¹⁰

David C. Mason, DO

Department of Osteopathic Manipulative Medicine

Carman A. Ciervo, DO

Department of Family Medicine
University of Medicine and Dentistry of New Jersey-School of Osteopathic Medicine, Stratford

References

1. Mason DC, Ciervo CA. Brachial plexus injuries in neonates: an osteopathic approach. *J Am Osteopath Assoc*. 2009;109:87-91. Available at: <http://www.jaoa.org/cgi/content/full/109/2/87>. Accessed May 1, 2009.
2. Leffert RD. Thoracic outlet syndrome. In: Omer GE, Spinner M, eds. *Management of Peripheral Nerve Problems*. Philadelphia, Pa: WB Saunders Co; 1980:593-601.
3. Grossman JA. Early operative intervention for birth injuries to the brachial plexus [review]. *Semin Pediatr Neurol*. 2000;7:36-43.
4. Terzis JK, Kokkalis ZT. Shoulder function following primary axillary nerve reconstruction in obstetrical brachial plexus patients. *Plast Reconstr Surg*. 2008;122:1457-1469.
5. Gilbert A, Pivato G, Kheiralla T. Long-term results of primary repair of brachial plexus lesions in children. *Microsurgery*. 2006;26:334-342.
6. Sucher BM. Thoracic outlet syndrome—a myofascial variant: part 1. Pathology and diagnosis. *J Am Osteopath Assoc*. 1990;90:686-696,703-704.
7. Sucher BM. Thoracic outlet syndrome—a myofascial variant: part 2. Treatment. *J Am Osteopath Assoc*. 1990;90:810-812,817-823.
8. Sucher BM, Heath DM. Thoracic outlet syndrome—a myofascial variant: part 3. Structural and postural considerations [published correction appears in *J Am Osteopath Assoc*. 1993;93:649]. *J Am Osteopath Assoc*. 1993;93:334,340-345.
9. Mychaskiw G II. Will the last DO turn off the lights [letter]? *J Am Osteopath Assoc*. 2006;106:252,302. Available at: <http://www.jaoa.org/cgi/content/full/106/5/252>. Accessed May 27, 2009.
10. Seffinger M, King HH, Ward RC, Jones JM III, Rogers F, Patterson MM. Osteopathic philosophy. In: Ward RC, ed. *Foundations for Osteopathic Medicine*. 2nd ed. Baltimore, Md: Lippincott Williams & Wilkins; 2003:5-12.

Cranial Palpation Pressures Used by Osteopathy Students

To the Editor:

I read with interest the original contribution by Rafael Zegarra-Parodi, DO (England), MEd, and colleagues¹ in the February issue of *JAOA*—*The Journal of the American Osteopathic Association*. Contrary to the authors’ claim, “craniosacral therapy” is *not* a technique that is widely used among osteopathic physicians. Rather, osteopathy in the cranial field, first described by William Garner Sutherland, DO,² is the system of diagnosis and treatment using the primary respiratory mechanism and balanced membranous tension that is an accepted part of the history and prac-

tice of osteopathic medicine.

The forces required in osteopathic manipulative treatment (OMT) are dictated by each patient's needs, responses, and medical condition—as well as by information that the treating individual's hands and mind perceive while conducting the treatment. Standardizing palpation is not possible with so great a range of patient variables and training levels among practitioners. Variance among “experienced cranial manipulation practitioners” (in the words of the authors¹) would likely be great, as would—I expect—variance among different types of practitioners. Thus, I agree with the authors that OMT does not lend itself well to outcome studies.

However, the authors' lack of distinction among “techniques” practiced by massage therapists, chiropractors, physical therapists, foreign-trained osteopaths, and US-trained osteopathic physicians is of concern.¹ The failure to make such a distinction suggests a lack of understanding regarding the variations of training and thought process among these different practitioners.

Dr Sutherland³ meant osteopathy in the cranial field to be applied to treatment of the whole body when he wrote, “Allow physiologic function within to manifest its own unerring potency rather than apply a blind force from without.” Other kinds of craniosacral therapies used by non-DO practitioners do not necessarily share this holistic concept.

I feel strongly that upholding the historic teaching standards of osteopathic medicine remains important, or else there is no distinction between the treatments that we provide as osteopathic physicians and the techniques that are applied by other kinds of practitioners. Our teachers must continue to be held to these high standards.

Daniel Kary, DO
Lewiston, Me

References

1. Zegarra-Parodi R, de Chauvigny de Blot P, Rickards LD, Renard E-O. Cranial palpation pressures used by osteopathy students: effects of standardized protocol training. *J Am Osteopath Assoc*. 2009;109:79-85. Available at: <http://www.jaoa.org/cgi/content/full/109/2/79>. Accessed May 14, 2009.
2. Glossary Review Committee for the Educational Council on Osteopathic Principles and the American Association of Colleges of Osteopathic Medicine. *Glossary of Osteopathic Terminology*. July 2006. Available at: https://www.do-online.org/pdf/sir_collegegloss.pdf. Accessed May 14, 2009.
3. Becker RE. Foreword. In: Sutherland WG. *Teachings in the Science of Osteopathy*. Fort Worth, Tex: Sutherland Cranial Teaching Foundation; 1990.

To the Editor:

I found the original contribution on cranial palpation pressures by Rafael Zegarra-Parodi, DO (England), MEd, and colleagues (2009;109:79-85) very disturbing. Osteopathy in the cranial field is *not* merely a “technique” or “therapy,” as indicated by the authors, but rather a medical treatment modality. Furthermore, it cannot be compared with cranial-sacral techniques, which are often performed by individuals who have no medical training. The process of applying osteopathic principles and practice involves a cohesive system of diagnosis, and osteopathic manipulative treatment (OMT) is different from any techniques performed by practitioners who are not trained in osteopathy in the cranial field.

As an assistant professor of osteopathic manipulative medicine (OMM), I see the premise behind the kind of research described by Mr Zegarra-Parodi and colleagues as opposing the basic principles of OMM and osteopathy in the cranial field. One cannot measure nor “standardize” a quantifiable amount of pressure to use for a manipulative technique, as suggested by the authors. The main reason that osteopathic medicine is so effective is that the osteopathic physician continuously diagnoses as he or she treats the patient. The appropriate amount of force used must always be individualized for each patient during each treatment session. Safe and effective OMM is not something that can be

performed or taught as a series of protocols or standardized techniques.

It takes years of clinical practice—on top of the years of osteopathic medical training—to get a proper sense of how best to apply effective hands-on treatment for each patient in each case. This evolutionary process should not be taken so lightly by the editors of *JAOA—The Journal of the American Osteopathic Association*.

Because the application of OMT is so individualized, it would be useful to conduct outcome studies comparing results obtained in treatment of patients with various ailments by osteopathic medical students and osteopathic physicians who have varying amounts of OMT experience.

Reem Abu-Sbaih, DO

Assistant Professor of Osteopathic Manipulative Medicine/Neuromusculoskeletal Medicine, New York College of Osteopathic Medicine of New York Institute of Technology, Old Westbury

To the Editor:

I feel obligated to express my disappointment with your decision to publish the original contribution by Rafael Zegarra-Parodi, DO (England), MEd, and coauthors (2009;109:79-85).

I would like to believe that the *JAOA* is “the premier scholarly publication of the osteopathic medical profession,” as you clearly state in the introduction to the Letters section of *THE JOURNAL*. However, the article by Zegarra-Parodi et al is written by foreign-trained osteopaths who are not licensed osteopathic physicians (as I understand the credentials), and it does not represent my profession. I would have hoped, at least, that the following statement by the authors would have been edited to correct the inaccuracy that osteopathic physicians practice a cranial manipulation “technique” rather than an applied science of osteopathy in the cranial field: “Cranial manipulation, or craniosacral therapy, is a widely practiced technique used by osteopathic physicians, foreign trained osteopaths, chiro-

practitioners, physical therapists, and massage therapists.”

I hold a masters of science degree in physical therapy from the University of California at San Francisco, and I am a certified athletic trainer. I am also a third-year osteopathic medical student at Touro University College of Osteopathic Medicine—California in Vallejo. If there had been no need for me to attend osteopathic medical school to learn my profession, and if I could have achieved my goals by simply being trained in a cranial manipulation technique, I would have saved myself many years of study and financial strain. However, before I became an osteopathic medical student, I tried that route, attending numerous courses in craniosacral therapy offered to nonphysicians. Even as a physical therapist who was trained at one of the premier schools in the United States, I felt a gap not only in the quality and skill of my delivery of the techniques, but also in the results I observed in my patients.

The reality is that treatment using cranial manipulation is not a technique to be learned and then practiced. Instead, in training to become an osteopathic physician, many aspects of anatomy, physiology, neurology, cellular biology, biochemistry, and biomechanics must first be learned as a basis before one can practice osteopathy in the cranial field. That is the main difference between osteopathic physicians and the other professions that the authors so blindly clumped together in the previously mentioned statement.

The road to becoming an osteopathic physician is long for a good reason. I and other osteopathic medical students study the complex sciences that help us understand the amazing biologic processes of the human body so that we can then use the marvelous tool of osteopathic medicine as a complete science—not merely as a technique—to help our patients. I urge the *JAOA* to keep these

points in mind and to respect the chosen paths and dedication of all osteopathic medical students by better guarding the distinctiveness of the osteopathic medical profession.

Nevena Zubcevik, OMS III, MSPT, ATC
Touro University College of Osteopathic Medicine—California, Vallejo

To the Editor:

Osteopathy in the cranial field provides an important contribution to the practice of medicine as a clinical application of osteopathic principles. The educational rigor, knowledge base, and skill development required to practice osteopathy in the cranial field are unparalleled. The osteopathic medical profession sets standards and carries responsibilities that are unsurpassed by other kinds of practitioners of manual techniques.

So it was with great interest that I read the original contribution on “cranial palpation pressures” by Rafael Zegarra-Parodi, DO (England), MEd, and colleagues.¹ I was initially very pleased to see a cover article in the *JAOA* discussing palpation of cranial anatomic function. Research is essential in providing evidence-based support for the practice of osteopathic medicine.

However, the article by Zegarra-Parodi et al¹ is fraught with problems.

Particularly problematic is the article’s opening statement, “Cranial manipulation, or craniosacral therapy, is a widely practiced technique used by osteopathic physicians, foreign-trained osteopaths, chiropractors, physical therapists, and massage therapists.”¹ Equating all manual practice regardless of its origin provides insurance companies with a rationale for refusing reimbursement of osteopathic physician services. Furthermore, publishing this article in the *JAOA* implies an American Osteopathic Association endorsement of equal status for all practitioners who may place their hands on the body.

As Brian F. Degenhardt, DO,² noted

in his accompanying editorial, 40 minutes of training is profoundly insufficient as preparation for developing examiner reliability in palpatory techniques.

As a research fellow in osteopathic manipulative medicine while an osteopathic medical student at Michigan State University College of Osteopathic Medicine in East Lansing, I spent many months and countless hours working with William L. Johnston, DO, in interexaminer reliability studies. We actively calibrated our touch and refined our palpatory tests for the specific purpose of ensuring palpatory synchronization. We did not begin our research study until we were certain that we were capable of attending to identical palpatory cues in the testing environment.

Given the limited preparation provided for each examiner in the study by Zegarra-Parodi et al,¹ it is understandable that differences in findings between “trained” and “untrained” examiners could not be demonstrated.

As Dr Degenhardt² also indicated, it is inappropriate to define a standardized pressure in the application of osteopathic manipulative treatment (OMT).

Effective practice of OMT requires an application of forces that match inherent forces within each individual patient. As the therapeutic process is engaged, the inherent forces within the patient will fluctuate, thus requiring the operator to constantly moderate applied forces. This is the very reason that extensive training is essential for the effective practice of osteopathy in the cranial field.

The fundamental set of assumptions underlying the research protocol used by Zegarra-Parodi et al¹ bears little resemblance to the actual practice of osteopathic medicine.

Further, the assumptions and the quality of the research protocol used by Zegarra-Parodi and colleagues¹ call into question the appropriateness of this article. I would appreciate greater

rigor being exerted in the future by the JAOA editors in the vetting of materials for publication.

Mark E. Rosen, DO

President, The Cranial Academy, Indianapolis, Ind

References

1. Zegarra-Parodi R, de Chauvigny de Blot P, Rickards LD, Renard E-O. Cranial palpation pressures used by osteopathy students: effects of standardized protocol training. *J Am Osteopath Assoc.* 2009;109:79-85. Available at: <http://www.jaoa.org/cgi/content/full/109/2/79>. Accessed May 19, 2009.
2. Degenhardt BF. New horizons for research and education in osteopathic manipulative medicine [editorial]. *J Am Osteopath Assoc.* 2009;109:76-78. Available at: <http://www.jaoa.org/cgi/content/full/109/2/76>. Accessed May 19, 2009.

Response

We have read with great interest the four letters regarding our February article.¹ We are encouraged by readers' lack of criticism directed at the actual methodology of our study or at our interpretation of the results. For constructive discussion concerning the subject of our article,¹ we refer readers to the excellent editorial by Brian F. Degenhardt, DO,² in the same edition of *JAOA—The Journal of the American Osteopathic Association*.

As scientists, we welcome the submission of any scientific data that would help us to refine our work. Unfortunately, the authors of all four letters have neglected to include references to such data, so we must conclude that they are unaware of any literature that would support their assertions.

Concern was raised in all four letters regarding the use of manual techniques of osteopathic origin within nonosteopathic professions. We share this concern and lament that manual techniques of osteopathic origin are currently being used by chiropractors, physical therapists, and massage therapists. We did, however, also note in our article¹ that these techniques are used by both types of osteopathic practitioners (ie, osteopaths and osteopathic physicians), as defined by the World Health Organization's (WHO) draft report *Guidelines*

on Basic Training and Safety in Osteopathy.³ Although the opening statement of our article¹ appears to have been a primary motivating factor for the authorship of the letters, at no point in the article did we imply that osteopathic manipulative procedures should be divorced from the osteopathic paradigm.

We remind readers that the aim of our study was to assess the effectiveness of training methods typically used in imparting the technical parameters of manual diagnosis and therapy to osteopathy students. We did not attempt to demonstrate that a single magnitude of palpatory pressure is sufficient for all clinical applications of osteopathic cranial manipulation. Providing osteopathy students with an objective "benchmark" for some parameters of a manual technique should not be confused with advocacy of rigid specifications for the application of that manual technique.

We agree that extensive clinical experience may be necessary to provide optimal manual treatment. However, we consider it plausible that the use of an objective reference standard for biomechanical palpatory parameters may aid and accelerate the transmission of correct application of manual techniques to osteopathy students. The claim that "One cannot measure...a quantifiable amount of pressure to use for a manipulative technique," as Dr Abu-Sbaih makes, is obviously false, since we have clearly demonstrated in our study¹ that this is possible. The usefulness of such measurements within educational and clinical settings remains to be examined, but we strongly disagree with the argument that research in this area cannot yield benefits for the development of osteopathic philosophy and practice.

Regarding claims that the clinical efficacy of osteopathic cranial manipulation is dependent on patient-specific modifications in palpatory pressure, we suggest that these claims need to be demonstrated by high-quality clinical

research before being made.⁴ Currently, no substantive evidence exists to show that osteopathic cranial manipulation is clinically effective for any condition.⁵⁻⁸ Anecdotal reports and unsupported assertions, such as those included in the four letters, are entirely unsatisfactory for substantiating the clinical efficacy, or the precise role, of individualized alterations in palpatory pressure during osteopathic cranial manipulation.

We would like to thank the editors of *JAOA—The Journal of the American Osteopathic Association* for acknowledging the potential applicability of our research to osteopathic medicine and for publishing our article¹ after it met full compliance with the *JAOA's* rigorous peer-review process.

Rafael Zegarra-Parodi, DO (England), MEd
Pierre de Chauvigny de Blot, DO (England)
Edouard-Olivier Renard, DO (France), MEd
 Research Department, Centre Européen d'Enseignement Supérieur de l'Ostéopathie, Paris and Lyon, France
Luke D. Rickards, M Osteo (Australia)
 Adelaide, Australia

References

1. Zegarra-Parodi R, de Chauvigny de Blot P, Rickards LD, Renard E-O. Cranial palpation pressures used by osteopathy students: effects of standardized protocol training. *J Am Osteopath Assoc.* 2009;109:79-85. Available at: <http://www.jaoa.org/cgi/content/full/109/2/79>. Accessed June 8, 2009.
2. Degenhardt BF. New horizons for research and education in osteopathic manipulative medicine [editorial]. *J Am Osteopath Assoc.* 2009;109:76-78. Available at: <http://www.jaoa.org/cgi/content/full/109/2/76>. Accessed June 8, 2009.
3. Directories. Osteopathic International Alliance Web site. Available at: <http://www.oialliance.org/directories.htm>. Accessed June 8, 2009.
4. Moran RW. Osteopathy in the cranial field—moving towards evidence for causality and effectiveness [editorial]. *Int J Osteopath Med.* 2005;8:79-80.
5. Green C, Martin CW, Bassett K, Kazanjian A. A systematic review of craniocervical therapy: biological plausibility, assessment reliability and clinical effectiveness. *Complement Ther Med.* 1999;7:201-207.
6. Hartman SE, Norton JM. A review of King HH and Lay EM, "Osteopathy in the cranial field," in *Foundations for Osteopathic Medicine*, 2nd ed. *Sci Rev Alt Med.* Fall/winter 2004-2005;8:24-28.
7. Downey PA, Barbano T, Kapur-Wadhwa R, Sciote JJ, Siegel MI, Mooney MP. Craniocervical therapy: the effects of cranial manipulation on intracranial pressure and cranial bone movement. *J Orthop Sports Phys Ther.* 2006;36:845-853.
8. Hartman SE. Cranial osteopathy: its fate seems clear. *Chiropr Osteopath.* 2006;14:10. Available at: <http://www.chiroandosteo.com/content/14/1/10>. Accessed June 8, 2009.

(continued)

Editor's Note: The *JAOA* received numerous letters on the February publication of "Cranial palpation pressures used by osteopathy students: effects of standardized protocol training" by Rafael Zegarra-Parodi, DO (England), and colleagues (2009;109:79-85)—in fact, the most on one piece in more than 10 years. The *JAOA's* editors appreciate the passion many of our readers have for osteopathy in the cranial field—and we thank them for the opportunity to address their concerns.

First, we would like to reassure readers that the article by Zegarra-Parodi et al was reviewed by three world-class experts in the topic area.

In addition, we would like to reiterate on behalf of the authors that the study investigated a technique—not a treatment. In the first sentence of the fifth paragraph on page 82, Zegarra-Parodi et al wrote: "[Primary respiratory mechanism] 'entrainment' models propose that palpation of expression of the [primary respiratory mechanism] at varying levels is dependent on a complex interaction of multiple biological oscillators between the patient and the practitioner." Nevertheless, the purpose of the study was to determine whether training can minimize variation in palpation pressure, not to see if every student used the same force on the same patient.

While it is true that a European-trained osteopath is not the same as an osteopathic physician trained in the United States, both practitioners follow the same basic tenets and use the same manual methods.

With regard to the importance of demonstrating reduced interoperator variation, though many readers believe that the authors' direction of inquiry is incorrect, others do not agree with this assumption about the study's scientific validity.

We and the authors acknowledge that the study has flaws. Several limitations are addressed by the authors on page 84 of that study. Others are discussed by Brian F. Degenhardt, DO, our guest editorialist for that issue (2009;109:76-78). Nevertheless, the *JAOA* will not be retracting the article by Zegarra-Parodi and coauthors.

Phantom Arrhythmia: Is It a Clinical Myth?

To the Editor:

In the February issue of *JAOA—The Journal of the American Osteopathic Association*, Kathryn G. Kolonic, MPH, OMS IV, and three MD coauthors¹ described the case of an 81-year-old woman with a permanent artificial pacemaker who had a condition initially interpreted as phantom arrhythmia (ie, arrhythmia that exists but has not been documented by an electrocardiogram). This patient was later diagnosed as having paroxysmal atrial tachycardia. The authors¹ were left with the opinion that the woman had comorbid conditions of atrial fibrillation and phantom arrhythmia, which they proposed redefining as "a cluster of symptoms suggestive of an arrhythmia that are perceived by a patient with a cardiac device but cannot be verified clinically."

I believe that the article by Kolonic et al¹ highlights the importance of considering psychiatric disorders in differential diagnoses, particularly in cases of cardiovascular disease. The osteopathic medical profession has always emphasized clinical evaluation of "the whole person," and our literature has established that there is a direct relationship between psychiatric conditions and cardiovascular disease.^{2,3}

In my own clinical experience, I have read the discharge diagnosis of "atypical chest pain" for many patients. These patients were typically admitted into intensive care units, evaluated, and ultimately diagnosed as having no observable cardiac condition or other documented medical pathologic condition. Among patients diagnosed as having atypical chest pain, I have never seen a discharge disposition for psychiatric care. On interviewing such patients, I have often discovered that they have experienced some or many of the following symptoms: chest discomfort, derealization, dizziness, dyspnea, fear of dying, gastrointestinal dis-

tress, lightheadedness, palpitations, paresthesia, pounding heart, shaking, sweating, trembling, and vasomotor instability. Unfortunately, I have found that the differential diagnosis of panic disorder was not even considered in most of these cases. Frequent comorbid conditions of panic disorder that should be kept in mind include mood disorder and generalized anxiety disorder.^{3,4}

The World Health Organization⁵ has noted that psychiatric disorders are among the most disabling conditions. The article by Kolonic et al¹ strongly suggests that the patient who appears with atypical chest pain or phantom arrhythmia requires a psychiatric evaluation that is as meticulous as the cardiologic evaluation. Such psychiatric disorders are manageable conditions, but if they are not properly addressed, they can increase lifelong morbidities and mortality rates in patients.

Edward H. Tobe, DO

Distinguished Fellow of American Psychiatric Association, Clinical Associate Professor, University of Medicine and Dentistry of New Jersey—School of Osteopathic Medicine, Stratford

References

1. Kolonic KG, Avina LG, Shoho A, Ismail A. Phantom arrhythmia: is it a clinical myth? *J Am Osteopath Assoc*. 2009;109:98-100. Available at: <http://www.jaoa.org/cgi/content/full/109/2/98>. Accessed May 12, 2009.
2. Haffey TA. How to avoid a heart attack: putting it all together. *J Am Osteopath Assoc*. 2009;109(suppl 1):14-20. Available at: http://www.jaoa.org/cgi/content/full/109/5_suppl_1/S14. Accessed May 26, 2009.
3. Aina Y, Susman JL. Understanding comorbidity with depression and anxiety disorders. *J Am Osteopath Assoc*. 2006;106(suppl 2):9-14. Available at: http://www.jaoa.org/cgi/content/full/106/5_suppl_2/S9. Accessed May 26, 2009.
4. American Psychiatric Association. *Diagnostic and Statistical Manual of Mental Disorders, 4th Edition, Text Revision*. Washington, DC: American Psychiatric Press; 2000.
5. Janca A, Kastrup M, Katschnig H, Lopez-Ibor JJ Jr, Mezzich JE, Sartorius N. The World Health Organization Short Disability Assessment Schedule (WHO DAS-S): a tool for the assessment of difficulties in selected areas of functioning of patients with mental disorders. *Soc Psychiatry Psychiatr Epidemiol*. 1996;31:349-354.

Stop Smoking, Save Money, Get Free OMM

To the Editor:

I write this letter to share some recent experiences of success in encouraging patients to quit smoking.

I am an osteopathic physician who

practices at a walk-in urgent-care/wellness osteopathic manipulative medicine (OMM) facility. Patients visiting this facility may pay by cash, credit card, or personal check. On payment, they are given a CMS (Centers for Medicare & Medicaid Services) 1500 claim form generated by our electronic medical record (EMR) system in addition to a receipt, which they can submit to their insurance company—if they have one and choose to do so. This arrangement allows us to offer significant discounts in charges for our services. It also allows patients to actively participate in many of the decisions involving their healthcare and their “healthcare dollars.”

By necessity, the services at our facility are limited to what is affordable for patients. We practice a great deal of empiric diagnoses and treatments with follow-up via telephone to ensure adequate patient response. If we do not deliver quality care for a reasonable price, patients will go elsewhere.

We have about 100 patients with chronic health problems and no insurance who depend on our facility as their main source of healthcare. Many of these patients with chronic problems are smokers. Between November 2008 and May 2009, seven of these patients had quit smoking and maintained that abstinence for at least 3 months. In addition, about a dozen more smokers have cut their cigarette consumption in half. These success stories are the result of anti-smoking measures that we instituted last fall.

At that time, we began to make sure that every smoker carries the diagnosis of tobacco dependency (ie, nicotine addiction) and that this diagnosis is printed on his or her EMR-generated patient notes at checkout. Because we do not bill the patient’s insurance company, we are able to include all relevant diagnoses related to each particular care visit on the patient’s paperwork—even if there are more than the arbitrary limit of four diagnoses per claim form imposed by the current system of

US healthcare “financiers” and “profiteers.” Thus, every patient with a chronic smoking condition gets the written “smoking-is-bad-for-you” message, in addition to the diagnosis of tobacco dependency, clearly printed on his or her take-home papers at every visit.

I was pleasantly surprised when patients spontaneously began reporting their efforts to quit smoking. My favorite story involves a woman who taught herself sign language as a way to quit! She learned to spell a new word or phrase in sign language each time she felt a nicotine craving—thereby occupying her hands and mind, as well as learning a marketable new skill. It was a win-win-win trifecta for her! I was glad to share her success story (but not her patient health information, of course) with the other smokers at our facility.

Several smokers at our facility have been able to quit “cold turkey.” Others have availed themselves of the bupropion hydrochloride 17-pill starter pack offered by the pharmacy at the local “big box” store. This antidepressant medication seems to be effective in helping some smokers quit, though we are still tracking patient outcomes to evaluate sustained results of using this drug as an antismoking aid. One patient who quit smoking reported that he has \$300 more in his pocket each month, and his savings increase each time the state raises the “sin tax” on a pack of cigarettes.

Another aspect of our program is our offer of two free OMM treatments to any patient who quits smoking for at least 2 months. These patients still must pay the evaluation-and-management charge for their visits but not the charge for their next two OMM treatments.

I hope that these encouraging experiences will provoke useful dialogue regarding experiences that other osteopathic physicians might have with efforts to get patients to quit smoking.

Thomas R. Byrnes, Jr, DO
Southern Light Osteopathic Wellness & Healthcare Associates, Inc—REDICARE, Richmond Hill, Ga

Does Prenatal Ultrasound Increase Risk of Autism?

To the Editor:

In his February letter to the editor, Christopher D. Olson, DO,¹ suggests that ultrasonographic examinations during pregnancy may be etiologically related to the development of autism. He cites a report by the Centers for Disease Control and Prevention (CDC)² that seems to suggest a recent, large increase in the prevalence of autism spectrum disorders (ASD). If this large increase were true, Dr Olson’s suggestion might make sense. However, despite the commonly held belief that many more children are currently being diagnosed with ASD than in the past, there is good evidence that the true incidence and prevalence of ASD may not have increased.^{3,4}

Clarifying this matter goes beyond splitting the proverbial hair. Understanding the true incidence and prevalence of any disorder is important because it helps healthcare providers determine those resources needed to devote to treatment. In addition, medical and public health professionals must educate the public about this matter so that parents have a realistic understanding of the health problems that can potentially affect their children. Poor or misleading information can be worse than no information at all.

Dr Olson¹ quite appropriately cites the controversy regarding autism and childhood vaccinations as an example of poor information leading to inappropriate withholding of vaccines.

Similarly, the idea of an increase in ASD may well be another result of selective reading of the literature—which actually offers much less definitive conclusions than proponents of an increase generally acknowledge.

Barbresi et al³ examined diagnoses of ASD that were made in Olmsted County, Minn, from 1976 to 1997. They concluded that an observed increase in incidence and prevalence of clinically diagnosed ASD during this period may

have been related to various confounding factors rather than a true increase. These factors included “diagnostic shifting” (ie, changes in the diagnostic criteria used in analyses), as well as increased availability of educational services to the public, resulting in increased public awareness of autism.³

Furthermore, the same CDC report² cited by Dr Olson¹ notes that, of the six sites for which prevalence data were available in 2000 and 2002, autism rates were stable in four of the sites. Although rates in the other two sites increased between these 2 years, the increase was described as statistically significant at only one of these sites.²

Latif and Williams⁴ found that, during a 16-year period (1988-2004), prevalence rates of ASD in several districts of South Wales, United Kingdom, rose based only on “increased referral rates and improved diagnosis of childhood autism at an earlier age.”

Of course, proponents of an increase in the incidence of ASD can cite equally compelling data to support their contention. The main point to remember in this debate is that published literature on the epidemiologic factors of autism and related disorders is not definitive—for either an increased or unchanged incidence.

Despite our preference for definitive answers to questions, readers of *JAOA—The Journal of the American Osteopathic Association* may want to refrain from jumping to premature conclusions regarding the epidemiologic factors—including a possible relation to ultrasonography—of autism and related disorders.

Jed G. Magen, DO

Associate Professor and Chair, Department of Psychiatry, College of Human Medicine, Michigan State University College of Osteopathic Medicine, East Lansing

References

1. Olson CD. Does prenatal ultrasound increase risk of autism [letter]? *J Am Osteopath Assoc*. 2009;109:71-72. Available at: <http://www.jaoa.org/cgi/content/full/109/2/71>. Accessed July 6, 2009.
2. Autism and Developmental Disabilities Monitoring Network Surveillance Year 2002 Principal Investigators;

Centers for Disease Control and Prevention. Prevalence of autism spectrum disorders—autism and developmental disabilities monitoring network, 14 sites, United States, 2002. *MMWR Surveill Summ*. 2007;56:12-28. Available at: <http://www.cdc.gov/mmwr/preview/mmwrhtml/ss5601a2.htm>. Accessed May 4, 2009.

3. Barbaresi WJ, Colligan RC, Weaver AL, Katusic SK. The incidence of clinically diagnosed versus research-identified autism in Olmsted County, Minnesota, 1976-1997: results from a retrospective, population-based study [published online ahead of print September 13, 2008]. *J Autism Dev Disord*. 2009;39:464-470.

4. Latif AH, Williams WR. Diagnostic trends in autistic spectrum disorders in the South Wales valleys. *Autism*. 2007;11:479-487. Available at: <http://aut.sagepub.com/cgi/reprint/11/6/479>. Accessed May 4, 2009.

To the Editor:

I am writing in response to the letter by Christopher D. Olson, DO.¹ The idea that prenatal ultrasonographic examinations may increase the risk of autism poses a significant danger to public health. I want to share my concerns regarding Dr Olson’s hypothesis, which has no evidence to support it. However, I would first like to address a related unsupported hypothesis—the false idea that autism is associated with vaccination.

As a pediatrician with subspecialty training in developmental and behavioral pediatric medicine, I exercise special caution in the diagnosis of autism spectrum disorders (ASD). To many parents, these conditions are a complete mystery because of unknown etiologic factors. Parents often become desperate to find “the cure” for their child’s condition, making them willing to accept any proposals that may seem to have sound scientific logic behind them, including the concept that vaccines can cause autism. The fearfulness of these parents may even drive them to believe in a conspiracy between the pharmaceutical companies and the medical professions to convince them of the need for vaccination or certain other interventions.

Such “conspiracy theories” require no evidence to support their claims. In fact, any evidence that contradicts the theories is typically considered to be fraudulent or fabricated. The phenomenon of the conspiracy theory has contributed substantially to the belief that vaccination is the direct cause of autism.

The origins of the supposed link between autism and the measles-mumps-rubella (MMR) vaccine can be traced to a 1998 article by Andrew J. Wakefield, MD, and colleagues.² Although that article no longer has any scientific merit, the hypothesis of the autism-vaccine link has become deeply rooted among many parents who have children with autism. To this day, these parents accuse the MMR vaccine, thimerosal (a mercury-containing preservative formerly used in many childhood vaccines), or merely “many shots” of causing the onset of autism in their children. To such individuals, it does not matter how many scientific studies disprove the alleged associations or how much money was spent to conduct those studies. Thus, we are witnessing a disaster in public health caused by Dr Wakefield’s unsupported claims.²

The mistrust of parents toward pharmaceutical companies, government health agencies, and physicians who prescribe vaccination continues to fertilize conspiracy theories. Furthermore, when celebrities publicly condemn the American Academy of Pediatrics’ recommended vaccination schedule³ and express disrespect toward pediatricians—as some have recently done⁴—the situation spins even more out of control. The present state of public ignorance regarding autism has reached a point similar to a metastatic cancer within the body of the pediatric population.

Pertinent to this matter is the February court judgment, concerning the US Department of Health and Human Services’ Vaccine Injury Compensation Program, against compensation for three alleged autism-vaccine cases.⁵ Also pertinent were the reports in 2008 of five cases of invasive *Haemophilus influenzae* type b (Hib) disease—including one death—in children in Minnesota.⁶ Three of these children had received no vaccinations at all for Hib due to parental refusal. One wonders whether any vaccine opponents were

convinced by the provaccine evidence of these reports, or whether they simply viewed the evidence as more conspiratorial fabrication.

Recently, in my son's elementary school, a case of mumps was diagnosed. Fortunately, the affected child had been fully immunized. Yet, I am concerned that some other children may not have received complete immunization. I am also concerned that our "herd immunity" may be breaking down, and that we may soon witness a reemergence of severe bacterial diseases in our school-age population, as well as in the adult population. As a parent, I am worried about my own children's safety.

I am also frustrated by those parents who refuse vaccinations for their children because of their false, though comforting, belief that disease will never come into their own household. Such families may benefit somewhat from health protections conferred to them by others who have received vaccination, while they themselves are like "ticking time bombs" that can explode on society at any time.

I believe that a similarly dangerous public health situation can arise from Dr Olson's bold statements¹—with no supporting scientific evidence—claiming that ultrasonographic examinations may cause the onset of ASD. Dr Olson did not conduct any form of investigation, such as a case report, case control study, retrospective chart review, or meta-analysis of the current evidence. In fact, Dr Olson¹ stated, "I am not in a position to conduct research into such a possible connection, and I am also well aware of potential roadblocks to conducting such research." He then proposed several research ideas and "challenged" our osteopathic medical colleagues to conduct such studies for him. By comparison, Dr Wakefield² at least conducted a systemic study and presented his results in a peer-reviewed journal.

Of course, every US citizen has the Constitutional right to free speech. But is it ethical to make the unsupported

claim of an autism-ultrasound association without considering the damage it can cause to the general public and the osteopathic medical profession?

I believe that Dr Olson's letter¹—containing an assumption with great public health risk but no solid scientific data to support it—is an unacceptable way to present ideas of possible autism causes. I am quite disappointed in *JAOA—The Journal of the American Osteopathic Association* for publishing such a letter without considering the consequences for the greater public health and safety. I view the *JAOA* as a representation of the osteopathic physician within the scientific community. Do the *JAOA*'s editors not realize the potential harm that such an unsupported claim can inflict on our public image?

Such a claim will promote public fear and enhance the growth of conspiracy theories. However—for the sake of argument—let us say that the association between ultrasound and autism is possible. If that were the case, then it would logically follow that physicians should also worry about ultrasonographic examinations causing other developmental abnormalities, including attention deficit hyperactivity disorder, dyslexia, hypotonia, mental retardation without genetic inheritance, and seizure disorder. Furthermore, based on Dr Olson's assumption,¹ the normal development of body organ systems would be placed in jeopardy as a result of ultrasonographic examinations. In fact, the list of potential health problems related to ultrasound might be assumed to be virtually endless—including any condition that might occur during fetal development.

If the assumption of an autism-ultrasound link becomes widespread, I could only imagine the family tragedies, health expenses, and lawsuits against physicians that would occur as a result of pregnant women refusing prenatal ultrasonographic assessments.

I do not know whether ultrasonographic examinations are related to the onset of autism. I do know, however,

that healthcare providers need to exercise great caution when making any public statement regarding any disorder. As an osteopathic physician, I am proud to see my colleagues publish studies that provide evidence to support osteopathic principles and practice. It is our responsibility to help the public understand our profession through scientific research. It is also our responsibility to avoid misleading the public by making unsubstantiated assumptions without putting forth any effort to provide supporting evidence.

As osteopathic physicians, we are given a certain power by our patients. How we exercise this power defines who we are as professionals. My mentors have often told me that with great power comes great responsibility. We walk a fine line between providing good care and potentially causing great harm. Most medical, diagnostic, and treatment procedures include guidelines to help physicians avoid harming their patients. Any deviation from these guidelines carries potential risks, requiring deep consideration and supporting evidence to justify a changed course of action. If we fail to gather and present such evidence, we will slip in our professional standards, and the resulting chaos can be lethal to our patients and our profession.

The *JAOA* has a power over the public welfare that we, as individual community physicians, can never have. Without wielding such power carefully, the consequences can be unimaginable. Thus, I strongly urge the *JAOA*'s editors to be more critical before publishing any assumption that does not have substantiated evidentiary support.

KinKee Chung, DO

Pediatric Associates of Cincinnati, Inc, Cincinnati, Ohio

References

1. Olson CD. Does prenatal ultrasound increase risk of autism [letter]? *J Am Osteopath Assoc*. 2009;109:71-72. Available at: <http://www.jaoa.org/cgi/content/full/109/2/71>. Accessed July 6, 2009.
2. Wakefield AJ, Murch SH, Anthony A, Linnell J, Casson DM, Malik M, et al. Ileal-lymphoid-nodular hyperplasia,

non-specific colitis, and pervasive developmental disorder in children. *Lancet*. 1998;351:637-641.

3. 2009 immunization schedule. January 4, 2009. American Academy of Pediatrics Web site. Available at: <http://www.cispimmunize.org/>. Accessed May 8, 2009.

4. Gorski D. Jenny McCarthy, Jim Carrey, and "green our vaccines": anti-vaccine, not "pro-safe vaccine;" June 9, 2008. Science-Based Medicine Web site. Available at: <http://www.sciencebasedmedicine.org/?p=139>. Accessed May 8, 2009.

5. Brady TM. Court decisions should allay parents' vaccine fears. *AAP News*. April 2009;30:1.

6. Bocchini JA Jr. Hib cases underscore need for immunization with primary series. *AAP News*. March 2009;30:1.

Response

I appreciate the opportunity to respond to the comments by Drs Magen and Chung. Their opinions in regard to my letter (*J Am Osteopath Assoc*. 2009;109:71-72) were well presented, and I understand their concerns.

I have repeatedly observed among some of my own patients the paranoia associated with false assumptions. Members of the public frequently do not evaluate health issues in a logical manner, and those of us in the medical professions constantly need to defend proven science.

In defense of my letter, however, I did not intend to "[jump] to premature conclusions," as Dr Magen suggests, nor to make "unsubstantiated assumptions," as Dr Chung suggests. The title of my letter, "Does prenatal ultrasound increase risk of autism?" clearly expressed my intent—that is, to ask a question that hopefully someone can answer. We surely need valid research on the factors involved in causing autism and related disorders, even if these disorders are not increasing in incidence, as Dr Magen maintains. Any disorder that has an incidence as high as autism and that causes as much personal and societal impact as autism begs to have answers to questions concerning etiologic factors.

I understand the concern expressed by Drs Magen and Chung that some fearful patients might adopt my letter as a reason to avoid modern obstetric care. I certainly hope that this does not occur, however, because it is true that I have no evidence of a connection between

prenatal ultrasonography and autism. That is why I would like to see someone conduct research on this matter. Although I am not in a university setting where resources are available to conduct such research, I may pursue evaluation of retroactive epidemiologic data to determine if there is reason for further investigation.

I thank Drs Magen and Chung for contributing to a serious discussion of my thoughts. While I fully support their concerns about making assumptions and jumping to conclusions, I hope their comments will not prevent further consideration of this topic. Research is the answer.

Christopher D. Olson, DO
Shamokin Dam, Pa

Where is the "Captain of the Ship"?

To the Editor:

I vividly remember a lecture during my first year of osteopathic medical school in which I was taught the concept of the "captain of the ship."

It goes like this: the attending physician is the captain of the ship. He or she is ultimately responsible for everything that happens to his or her patient. If the attending physician misdiagnoses the patient's condition, resulting in a bad outcome, that attending physician is responsible. If the wrong medication is given to a patient by a nurse, the attending physician is responsible. If the patient falls out of bed and breaks his or her hip, the attending physician is responsible.

In March, my father, a retired osteopathic family physician, received a routine outpatient injection of methylprednisolone in his right knee for arthritic pain. He said he felt better the next day. By the following day, however, the knee was so swollen and painful that he could not stand.

Emergency medical services personnel brought my father to the closest emergency department, which is part of

a large, financially successful community hospital with several university affiliations in suburban Philadelphia, Pa. Aspiration of the knee produced frank pus. Two arthroscopic lavage procedures were performed. After 3 days, *Streptococcus viridans* grew from culture of the joint aspirate.

Conforming to current standard hospital procedures, my father was informed of his discharge by the hospital's "discharge planner" the following morning, and he found himself placed in a nursing home by the afternoon. He needed this placement because he was still unable to stand. While in the nursing home, he became progressively lethargic. Although the staff was instructed not to dispense additional opiate analgesics to him, his level of consciousness failed to improve.

My father was readmitted to the intensive care unit of the hospital—again through the emergency department—in a condition of acute renal failure with urinary retention. His level of consciousness improved with a single dialysis treatment and Foley catheterization. He soon improved enough to be transferred to the in-house rehabilitation unit.

While my father was in the rehabilitation unit, I became upset that I could not determine the identity of his attending physician. My father's nurse of the day informed me that my dad was on the "rehab service" and that a different attending physician was in charge each day. I took a deep breath because I knew what this meant. It meant that no one human being was in charge of my father's care—a prescription for disaster.

My dad's knee pain was relentless. The pain interfered with his ability to participate in his physical therapy sessions, and he became increasingly depressed at his lack of progress. Two trials to remove his Foley catheter failed. His blood urea nitrogen levels continued to fluctuate.

I was finally able to speak with one of his "attending physicians of the day"

by telephone. While we spoke, she had my father's electronic medical record in front of her. She knew his laboratory "numbers," but she did not know him. She offered resistance when I requested follow-up visits from the hospital's urology and orthopedics services. I had to ask specifically for magnetic resonance imaging for the problematic knee. The physician I spoke with indicated that no one in my father's treatment team believed that anything was out of the ordinary in his course of rehabilitation.

Growing more and more frustrated, I placed a call to the chief executive officer of the hospital. I wanted him to know that in his hospital, reasonable follow-up procedures were not occurring, and patient problems were not being adequately addressed. His secretary told me that she would see that he got my message. However, I received a call back from a "patient advocacy specialist" (another non-physician disseminator of information). I thanked her for her involvement but reminded her that I, as a physician, had asked to speak with the CEO. She replied, "Oh, he won't call you back."

She was right. He did not call me back. Perhaps he would have returned my call if I had been an attorney.

This is a travesty of modern medicine! To accommodate the desires of current-day physicians—who are seemingly more interested in their own lifestyles than in caring for their patients—we have created a world of "treatment teams." Today's physicians

want jobs, not careers. They want to get to work at 9:00 AM and leave by 5:00 PM. They do not want to work nights or weekends. They want the patient's electronic medical record to serve as a surrogate for actual personal knowledge of the patient and his or her health status.

There was no physician's name on my father's patient identification bracelet in the hospital. There was no one physician in charge. This type of patient management system has come about, no doubt, as a response to market forces. It is based on the assembly line, in which any one worker is interchangeable with any other worker.

The "physician employees" of today's healthcare system are able to maximize their personal compensation while minimizing their involvement with patients as human beings who have their own needs and concerns. Although some physicians may think this is a "win-win situation," it is not. Patient care is suffering profoundly. But perhaps the general public does not yet realize the extent of this problem. Fortunately, many patients can heal without the "benefit" of learned medical intervention.

I have listened to many sad stories from my own patients who have been admitted to other hospitals at which they—or perhaps their family members—have received shoddy care. I commiserate with them as best I can.

In 1988, Nancy M.P. King, Larry R. Churchill, and Alan W. Cross recorded similar observations in their book, *The Physician as Captain of the Ship: A Critical Reappraisal* (Boston, Mass: D Reidel Publishing Co). I believe that, since the publication of that book, this matter has become an even more serious problem.

Where was my father's "captain of the ship"? If he—as a retired physician with a son and daughter-in-law who are also physicians—received medical care that was this paltry, what happens to the poorly educated patient who does not have a knowledgeable personal advocate?

Physicians have earned the public's distrust—and they should be ashamed of themselves. The medical system is broken. We need to return the ultimate responsibility for the patient's welfare to the individual physician.

I know that "treatment teams" will not go away. Nonetheless, we must make the extra effort to ensure that the transfer of critical information from one team to the next is seamless. This effort will require an additional investment of time by physicians. A single dedicated and passionate human being has to be at the helm of a patient's care at all times.

We all need our "captains of the ship."

David Stuart Tabby, DO
Associate Professor, Drexel University College of Medicine, Philadelphia, Pa ♦

Wherever the art of medicine is loved, there is also a love of humanity.

Hippocrates

Corrections

The *JAOA* deeply regrets that several editing errors were made in the following book review:

DiLullo C, reviewer. *J Am Osteopath Assoc.* 2009;109:75,101-102. Review of: Rohen JW. *Functional Morphology: The Dynamic Wholeness of the Human Organism.* Available at: <http://www.jaoa.org/cgi/content/full/109/2/75>. Accessed July 2, 2009.

These edits were not approved by the book reviewer prior to publication. The changes detailed below, which restore the reviewer's original intent, were made to the full text (<http://www.jaoa.org/cgi/content/full/109/2/75>) and Adobe Portable Document Format (<http://www.jaoa.org/cgi/reprint/109/2/75>) versions of this piece online.

□ *Page 101*—The fourth sentence of the fourth paragraph in the second column mistakenly read as follows: "In the endocrine system, the pituitary gland is designated as the thinking element based on its anatomic location. However, this gland is also characterized as a major metabolic regulator, suggesting instead that it should perhaps be depicted as the willing element." The passage should have been printed as follows: "In the endocrine system, the pituitary gland is designated as the thinking element based on its anatomic location, but this gland is also characterized as a major metabolic regulator, indicating it could alternatively be depicted as the willing element."

In addition, the first sentence of the next paragraph was erroneously published as follows: "The threefoldness dogma extends through the book's concluding section where the author addresses the process of evolution and our place in the cosmos." Instead, this sentence should have been published as originally approved by the book reviewer: "The threefoldness dogma is extended in the book's concluding section to correlate with the process of evolution and our place in the cosmos."

Finally, in the next column, the seventh paragraph appeared as shown: "Although Dr Rohen makes it clear that the human species is still evolving, he promotes a view of human evolution in which the endpoint is the achievement of a state of selflessness and pure form. In this pure form, the ultimate evolutionary possibility of the human being is resurrection." Instead, the paragraph should have read as follows: "Although Dr Rohen makes it clear that the human species is still considered to be evolving, he offers for consideration a view in which the endpoint of human evolution may be the achievement of a state of selflessness and pure form. In this pure form, the ultimate evolutionary possibility is resurrection."

□ *Page 102*—The concluding paragraph originally appeared as follows: "To gain an appreciation of the full scope of Dr Rohen's philosophical treatise, it must be read to the very last page. And yet, though *Functional Morphology* is an intriguing text, it is but a single interpretation of a very broad and complex set of data." The book reviewer's approved concluding paragraph should instead have been published: "*Functional Morphology* is certainly an intriguing text. However, it must be read to the very last page to appreciate the full scope of Dr Rohen's philosophical treatise and to understand that this work is but a single interpretation of a very broad and complex set of data."

In addition, the *JAOA* and the lead author regret an error that appeared in the following article:

Mason DC, Ciervo CA. Brachial plexus injuries in neonates: an osteopathic approach. *J Am Osteopath Assoc.* 2009;109:87-91. Available at: <http://www.jaoa.org/cgi/content/full/109/2/87>. Accessed July 2, 2009.

On page 90, the work and recommendations of Benjamin M. Sucher, DO, were presented inaccurately. Likewise, the authors' opinions were inappropriately attributed to Dr Sucher:

Sucher²⁵⁻²⁷ recommends that manual treatment for patients with thoracic outlet syndrome focus on the use of myofascial techniques. If an osteopathic physician finds decreased range of motion or hypertonicity in the myofascial structures around the thoracic inlet unilaterally, Sucher²⁵⁻²⁷ suggests the use of gentle myofascial stretching. We believe that the same principle applies to infants with brachial plexus injuries.

Instead, the first paragraph under "Osteopathic Manipulative Treatment" should have appeared as follows:

Dr Sucher's articles²⁵⁻²⁷ on treating adult patients with thoracic outlet syndrome focus on myofascial restrictions and aggressive myofascial stretching techniques. We believe that if the osteopathic physician determines that there is decreased range of motion or hypertonicity in the myofascial structures around the thoracic inlet unilaterally in neonates, then gentle myofascial stretching may be used, along with careful patient monitoring, to remove the somatic dysfunctions and restore normal anatomic relationships around the thoracic inlet area.

These changes were made to the full text (<http://www.jaoa.org/cgi/content/full/109/2/87>) and Adobe Portable Document format (<http://www.jaoa.org/cgi/reprint/109/2/87>) versions of this article online. ♦