In defense of publishing research findings in *JAOA*

To the Editor:

The value of publishing original osteopathic research findings in *JAOA* has now been questioned in two letters, one in the July 1999 issue, the other, in the November 1999 issue. Others have urged me not to publish my findings in *JAOA*. The few individuals I know with osteopathic research to report are all considering nonosteopathic journals. This is not good for the profession.

Dr Juhn, in his letter, "The dilemma of publishing OMT studies in *JAOA*" (*JAOA* 1999;93:339), advances the idea that publication in *JAOA* does little to convince those outside the profession of the value of osteopathic manipulative treatment (OMT) and that those of us who read *JAOA* need the least convincing about osteopathic practices. In his letter, "The importance of referencing osteopathic medical literature" (*JAOA* 1999;99:558-560), Dr DeBias states that Dr Juhn is absolutely correct and recalls his own research experiences in the days when *JAOA* was not listed in the *Index Medicus*. These statements raise the question: Why should we even have *JAOA*?

Having a scholarly journal with a focus on osteopathic research and issues relevant to the profession brings respect and distinctiveness. The best and largest concentration of clinical osteopathic research in the world is published in *JAOA*. Dr Juhn is incorrect when he asserts that publication in *JAOA* will not convince those outside our profession. Granted, biased people will discount a study on the basis of its publisher, but quality research published in any scholarly journal holds the respect of the scientific community. It is true that publication in a larger journal brings more exposure, and this is a valid reason to seek publication outside *JAOA*, but *JAOA* reaches the main audience for osteopathic research—the osteopathic profession.

It is also not true that those who read *JAOA* need less convincing about the value of osteopathic practices. Many in our profession openly doubt the value of OMT, particularly for systemic or visceral disorders.[1] The purpose of research is not to prove what we already know. I am convinced that research will show that some of our dogma on OMT is incorrect, but it will also bring to light new applications for OMT. We need a scholarly journal to inform our profession about what we are learning.

I agree with Dr DeBias, that research should be published in journals listed in *Index Medicus* so that the work is attainable by anyone around the world interested in the same topic, but *JAOA* is in *Index Medicus*, so this is no longer a reason to seek publication elsewhere.

Our main problem in osteopathic research today is not which journal to publish in, but the small number of reports of original research produced each year. We have so few committed to ongoing clinical research in this field. Our greatest need is for individuals like Dr Juhn to become active in osteopathic research.

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Response

To the Editor:

I appreciate Dr Noll's input on this controversial issue. I would like to reiterate the statement I made in my original correspondence, that "we need to continue to make *JAOA* the respectable publication and voice of the AOA that it has been for so many years." My opinion is simply that we should not publish osteopathic research findings in *JAOA* exclusively.

I respectfully disagree with Dr Noll's opinion that quality research will hold the respect of the scientific community regardless of its publisher. The frustrating experience of Dr DeBias[1] is not uncommon today. Undoubtedly, the recent osteopathic article published in the *New England Journal of Medicine*[2] has done far more to increase awareness of osteopathic principles than any article in *JAOA*. As I stated in my original letter, this is neither fair nor justified, but it is true.

I appreciate Dr Noll's suggestion that people like myself become more involved in osteopathic research. Unfortunately, my other responsibilities preclude this possibility. However, each family medicine resident spends time with me as part of a required rotation, and I can assure Dr Noll that these residents are introduced to the values of osteopathic medicine. My optimism tells me that most, if not all, of them are very accepting as well.

I still maintain that those who actually read *JAOA* need the least convincing of the value of osteopathic practices. Dr Noll disagrees, stating (correctly) that there are some in our profession who doubt the value of osteopathic manipulative treatment. However, these are the same people who do not read *JAOA* to begin with. If 4 years of osteopathic medical education results in a graduate questioning the value of osteopathic practices, then I doubt that an article in *JAOA* would change that line of thinking. It always disappointed me when I saw my osteopathic colleagues toss *JAOA* into the nearest trash can. I sometimes wonder: Did we fail in teaching these prospective DOs properly, or did we fail by admitting them in the first place?

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2. Andersson GBJ, Lucente T, Davis AM, Kappler RE, Lipton JA, Leurgans S. A comparison of osteopathic spinal manipulation with standard care for patients with low back pain. *N Engl J Med* 1999;341:1426-1431.

Paradox answered

To the Editor:

I read with interest the article by Andersson and associates[1] and the editorial by Howell[2] comparing allopathic and osteopathic management of low back pain. This and similar studies will enhance integration of allopathic and osteopathic medicine toward the primary goal of both disciplines: prevention and treatment of disease and suffering with the most efficacious and least toxic means available. I see this more as allopathic and osteopathic medicine moving toward each other than Dr Howell's concept of osteopathic practice moving closer to allopathic practice. He recognizes that the allopathic majority has devoted increasing recognition to the importance of preventive care.[2] Osteopathic manipulative techniques have been integrated into the repertoire of some allopathic specialties such as physiatry. Other considerations include recognition of the fact that there exists suboptimal learning about identification of musculoskeletal pathologic disorders in traditional allopathic training regimens[3] as well as the tremendous pharmacologic advancements since Andrew Taylor Still's day. There is no reason why allopathic physicians interested in the treatment of patients with musculoskeletal disorders cannot learn and become proficient in these techniques.

As more quantitative and qualitative investigations into the cost and benefits of osteopathic manipulation become available, there is likely to be continued integration by allopathic physicians of manipulative techniques. To that end, the continued existence and evolution of osteopathic physicians is justified for at least as long as there is need to (1) define and quantify the optimal osteopathic techniques and (2) teach these techniques to allopathic physicians and other healthcare providers who care for patients with musculoskeletal disorders in and outside of other medical disorders.

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The opinions and assertions contained herein are those of the author and not to be construed as official policy of the Department of the Army or Department of Defense.

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2. Howell JD. The paradox of osteopathy. *N Engl J Med* 1999;341:1465-1467.

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Physician recalls unforgettable evening in her early career

To the Editor:

One spring evening in 1927, I was on duty as an intern at Lakeside Hospital in Kansas City, Missouri, when a situation developed that would engage my services for the next 12 hours. This was not only unique in my professional training; it was a valuable experience that I have never forgotten throughout my years as an osteopathic physician.

Postoperative pneumonia developed in a young man who had been operated on that morning; the surgeon and the radiologist confirmed the diagnosis by fluoroscopic observation. Dr George J. Conley (DO, MD, FACOS), chief surgeon and the patient's physician, ordered me to perform osteopathic treatment every half hour until he saw the patient the following morning. After I resolved to carry out the order myself (the evening shift usually ended at 11:30 PM), I decided to be available all night by resting on the davenport in the patient's waiting room when my duties were done. The nurse could summon me for this service as needed. Now I needed a treatment plan.

As the order to administer treatment every half hour differed from my customary, 30-minute procedure for treating a patient in a hospital bed, I began to evaluate the situation. With pneumonia, the alveolar spaces contain exudates and fibrin, and the process may involve a lobe or lobule. Obviously the lungs do not get oxygen exchanged for carbon dioxide when clogged this way—what could I do in a 5-minute osteopathic treatment that would support the capacity of the body to resolve the problem and restore healthy physiology in the lungs?

I thought of Dr Still's advice, to turn on the blood supply.

I was used to treating patients in a hospital bed. The nurse had the upper part of this patient's bed somewhat elevated. I decided to leave it that way. Then after considering that the sympathetic ganglia of the thorax are positioned in front of the heads of the ribs and that the whole relationship of the ribs and the vertebrae is available to manual operations, I devised my plan of treatment. For each 5-minute treatment, I grasped the angles of the ribs, carried them laterally, and waited for action with the breathing until it became free. Then I balanced the patient's neck in my hands until it relaxed. This procedure was aimed at the sympathetic ganglia of the neck and the physiologic control over the circulation of the blood.

During the night, it became obvious that the patient was sleeping more deeply and that he was more relaxed, but I had never considered the ultimate result: Dr Conley informed me the next morning that the patient's pneumonia had resolved. Fluoroscopic examination revealed that the patient's lungs were working normally and that he was breathing comfortably.

I do not remember anything more about that patient—only the night I administered one 5-minute treatment every half hour to resolve his pneumonia. Since then I have used the principle of short treatments administered frequently for some occasions.

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Importance of empiricism in medicine

To the Editor:

In a recent editorial (*JAOA* 1999;99:565), Coughlin, Kriebel, and Fogel asserted: "The

desire of people to seek out manipulative treatment from practitioners of all kinds, and to pay for it, although not scientifically valid, *is evidence of some type of effectiveness*" (my italics). This is a widely held and entirely fallacious belief; it is a mistake to assume that such behavior on the part of patients is in any way related to clinical efficacy.

Without careful controls, practitioners and patients both can be fooled into believing that a treatment is effective when it is not (eg, Beyerstein[1] and numerous references cited therein). Important contributions to such misperceptions include the following:

- Because of the natural course of most diseases, patients usually show symptom improvement even if no treatment is given.
- Symptoms often improve for specific reasons (eg, increased exercise, improved diet, reduced stress) unrelated to a particular treatment.
- Symptoms are often perceived by a patient/practitioner as improved consequent to one or another variety of placebo effect.
- Patients often report symptom improvement because they *want* to feel better, *want* to please their practitioner, and *want* to believe that their time and money have been well spent.[1-3(pp117-137)]
- If practitioners evaluate symptom improvement subjectively, they see often what they want or expect to see because so much of their ego and the patient's well-being are invested.[4]

For example, the impact on a patient of caring, human touch from a sensitive, knowledgeable, and confident practitioner is such that, once a process that includes these elements (eg, any form of osteopathic manipulative treatment) is completed, patients often will indicate that they feel better *regardless of whether any disease-related physical or physiologic changes have been manifested*.[3(pp77-92)] This positive response can reinforce the practitioner's conviction that a treatment was well chosen, which may augment the practitioner's confidence, which may make it easier for the practitioner to inspire a placebo response from subsequent patients, which may reinforce further the practitioner's confidence...until numerous patients are seeking out, paying for, *and settling for* a treatment that actually may not be improving relevant pathophysiologic processes.

I am not suggesting that the "magic" in such an encounter is unimportant. It is always important and for many patients is all a practitioner can offer. However, to assure that biological reality will be the primary guide to further medical development, I am suggesting that we strive for accuracy in clinical judgments of cause and effect. Unfortunately, the popularity of a treatment provides little evidence for clinical efficacy. I summarize and close with a reminder: "...[I]t was the realization that shortcomings of perception, reasoning, and memory often will lead us to comforting, rather than true, conclusions that led the pioneers of modern science to substitute controlled, interpersonal observations and formal logic for the anecdotes and surmise that can so easily lead us astray."[1]

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3. Harrington A, ed. *The Placebo Effect: An Interdisciplinary Exploration*. Cambridge, Mass: Harvard University Press; 1997.

4. Gilovich T. *How We Know What Isn't So: The Fallibility of Human Reason in Everyday Life*. New York, NY: The Free Press; 1991:49-87.

Response

To the Editor:

We appreciate Dr. Hartman's pointing out the difficulty presented by the placebo factor in research. His description is apt and informative. While being a recognized concern in all clinical science, the placebo effect is a particularly thorny issue in experimentation involving human touch. It is difficult to construct appropriate placebo (or "sham") treatments in manipulation research, especially because it is not presently clear what the exact nature and (potential) therapeutic effect of simple touch actually is.

Efficacy is not and should not be judged on the use of a therapeutic modality by the general population. We would like to point out, however, that as he takes issue with the statement "is evidence of some type of effectiveness...," he apparently ignored the previous clause in the sentence, "although not scientifically valid...." We should like to also point out that although many drugs are approved by the Food and Drug Administration, approvals are based primarily on the safety of those drugs as demonstrated by double-blind, placebo-controlled clinical trials (the gold standard of clinical research), rather than peer-reviewed basic science. That is to say that many drugs are distributed based on their "efficacy" relative to placebo, but without complete experimental demonstration of biochemical or physiologic action. While there is "magic" in the physical encounter between clinician and patient, the use of that word in particular suggests that all touch is placebo.

In reference to the descriptive title of his letter, we would remind Dr. Hartman that the American College Dictionary defines *empirical* as: "2. Depending on experience or observation alone, without using science or theory, especially in medicine." ◆

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