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 Word for Windows (.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.

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**Short-Term Hematologic and Hemodynamic Effects of Osteopathic Lymphatic Techniques**

*To the Editor:*  
I wish to congratulate William Evan Rivers, DO, and his coauthors for their original contribution in the November 2008 issue of JAOA—The Journal of the American Osteopathic Association.

Although a small pilot study, the results of the research conducted by these individuals represent an enormous amount of time and dedication. The osteopathic medical profession needs more original research to be published on questions that relate to our profession’s unique contributions to healthcare. Specifically, efficacy and mechanistic studies are needed, both of which are currently far too few in number.

Despite the value of the study by Rivers et al, I have several modest criticisms of the article. In the introduction, the authors state that “comprehensive lymphatic treatments were reported to decrease mortality rates during the influenza pandemic of 1918,” and this statement is referenced with a 1920 JAOA article by R. Kendrick Smith, MD, DO.

This reference citation is misleading, however. First, Dr Smith does not refer to osteopathic medical treatments during the 1918 pandemic as being comprehensive lymphatic treatments. In fact, he does not provide any details about the treatments or techniques that were used at the time. Rather, he presents an analysis based on 2445 case reports that were collected during the pandemic, he describes how the mortality rates were estimated, and he discusses the significance of the findings. Dr Smith also emphasizes the importance of mobilizing the spinal column—a procedure that was not performed in the lymphatic treatment protocol used by Dr Rivers and his coinvestigators.

Another problem with citing the Smith article is that the treatment protocol used by Dr Rivers and colleagues relied heavily on lymphatic pump techniques. However, it is unlikely that lymphatic pump techniques were used commonly—if at all—during the 1918 influenza pandemic. It was not until the 1920s that C. Earl Miller, DO, developed techniques intended to circulate lymphatic fluids and first coined the phrase “lymphatic pump.”

I have a number of other questions regarding the article by Rivers et al. Results of oxygen saturation measurements are not reported. Can we assume that these data were unremarkable?

It also appears from the article by Rivers et al that complete blood counts were collected. If that is true, it would be interesting to learn the results of the red blood cell and differential cell counts, because these data would likely complement work conducted by previous researchers (myself included).

For example, in experiments with direct splenic pump stimulation, Castillo and Ferris-Shift measured pre- and posttreatment red blood cell counts and leukocyte differential cell counts in healthy individuals and in patients with infectious disease. In addition, Mesina
and colleagues measured complete blood cell parameters after application of lymphatic pump techniques, reporting that all results of these measurements were negative, except for basophil measures.

As Dr Rivers and his co-investigators noted, comparisons between studies must be made with caution because the treatment protocols vary from one study to another. Nevertheless, such comparisons would surely be of some value.

These minor criticisms notwithstanding, I again congratulate Dr Rivers and his co-authors on their diligent efforts.

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References

Response
In his historically significant and intriguing document, R. Kendrick Smith, MD, DO, quotes a passage from Carl McConnell, MD, DO, to explain his approach to osteopathic medical care. In the quotation, Dr McConnell emphasizes the assessment of the “tension of muscles, the status of glands, the flexibility of the vertebral joints, the mobility of the chest wall, and the vital response of viscera” in osteopathic diagnosis. Dr McConnell also asserts the importance of addressing the “edematous barrier of the involved lymphatic tissues” to “enhance drainage and leukocytic [sic] activity.” Thus, in our November 2008 article, we used the term “comprehensive lymphatic treatment”—that is, a treatment that addresses the multiple factors that contribute to lymphatic flow.

To develop our protocol, we used the most recent edition of Foundations for Osteopathic Medicine for current and standardized descriptions of osteopathic manipulative treatment techniques.

We caution the reader against making direct comparisons between Dr Smith’s 1920 report and our recent study because of differences in purpose, research design, methods, and study populations—as well as different historical contexts.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Baseline Treatment Received</th>
<th>Measure, Mean (SD)</th>
<th>Postbaseline Measures, Mean (SD)</th>
<th>Condition Effect</th>
<th>Time Effect</th>
<th>Condition × Time Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Red Blood Cell</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Count, million/µL</td>
<td>N</td>
<td>4.90 (0.23)</td>
<td>4.94 (0.22)</td>
<td>4.98 (0.25)</td>
<td>4.98 (0.22)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>4.87 (0.19)</td>
<td>4.91 (0.21)</td>
<td>4.92 (0.21)</td>
<td>4.97 (0.22)</td>
<td></td>
</tr>
<tr>
<td><strong>Basophils, %</strong></td>
<td>N</td>
<td>0.50 (0.52)</td>
<td>0.25 (0.45)</td>
<td>0.25 (0.45)</td>
<td>0.25 (0.45)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>0.17 (0.38)</td>
<td>0.25 (0.45)</td>
<td>0.17 (0.38)</td>
<td>0.25 (0.45)</td>
<td></td>
</tr>
<tr>
<td><strong>Eosinophils, %</strong></td>
<td>N</td>
<td>1.92 (1.44)</td>
<td>2.00 (1.12)</td>
<td>1.75 (1.05)</td>
<td>1.75 (1.05)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>2.08 (1.50)</td>
<td>2.00 (1.53)</td>
<td>2.25 (1.54)</td>
<td>1.83 (1.33)</td>
<td></td>
</tr>
<tr>
<td><strong>Lymphocytes, %</strong></td>
<td>N</td>
<td>32.42 (8.82)</td>
<td>32.42 (8.71)</td>
<td>33.08 (9.94)</td>
<td>33.83 (8.30)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>33.42 (8.36)</td>
<td>33.50 (7.62)</td>
<td>33.33 (8.63)</td>
<td>34.33 (8.53)</td>
<td></td>
</tr>
<tr>
<td><strong>Macrophages, %</strong></td>
<td>N</td>
<td>7.42 (0.99)</td>
<td>7.83 (1.40)</td>
<td>8.50 (1.97)</td>
<td>7.92 (1.50)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>7.67 (1.30)</td>
<td>7.58 (1.16)</td>
<td>7.58 (1.31)</td>
<td>8.00 (1.53)</td>
<td></td>
</tr>
<tr>
<td><strong>Neutrophils, %</strong></td>
<td>N</td>
<td>57.75 (9.45)</td>
<td>57.50 (8.80)</td>
<td>56.58 (10.06)</td>
<td>56.08 (9.20)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Y</td>
<td>56.75 (8.67)</td>
<td>56.92 (8.79)</td>
<td>56.83 (9.52)</td>
<td>55.50 (9.70)</td>
<td></td>
</tr>
</tbody>
</table>

* Ellipses (...) indicate that the comparison is not statistically significant.
We collected subjects' heart rate measurements using a pulse oximeter, as stated in the “Methods” section of our article, but we did not record subjects' oxygen saturation levels. An analysis of heart rate measurements is noted in the final paragraph of our “Results” section.

We found no condition x time interaction effects for red blood cell count or differential leukocyte counts. Please refer to the Table for the requested data on these variables.

Finally, the “Comment” section of our article notes that we did not confirm results of previous studies of hematologic effects of lymphatic techniques in healthy subjects.

William Evan Rivers, DO
Rehabilitation Institute of Chicago

References

Self-Reported Cardiac Risks and Interest in Risk Modification Among Volunteer Firefighters

To the Editor:
I read with great interest the December 2008 special communication article regarding the evaluation of volunteer firefighters for cardiac risk by Patrick Scanlon, PA-C, DO, and Elizabeth Ablah, PhD, MPH.

As an osteopathic physician who provides care full time for firefighters, I am well aware of the health hazards that these individuals face on a daily basis. The survey results reported by Drs Scanlon and Ablah provide good insight into a population that represents more than 70% of the firefighting community—the volunteers.

I must point out, however, that the National Fire Protection Association (NFPA) 1582 standard that was referenced by Drs Scanlon and Ablah is quite dated. There have been three updates to this NFPA standard since the 1998 publication of that document. The most current standard, from 2007, provides clear healthcare guidance to fire departments, as well as to physicians involved in providing care for firefighters.

According to the current NFPA standard, medical examinations are required annually for all fire department members—not on a graduated basis depending on age, as indicated in the article by Drs Scanlon and Ablah. In addition, these annual examinations are required to be at the expense of the employer.

In addition, I would recommend that body mass index (BMI) be used with caution when evaluating the health and fitness of the firefighter population. We have found that BMI does not fairly measure our fire department members, mainly as a result of the consistently high muscle weight that these individuals often maintain in order to effectively perform the functions of their profession. We find that performing actual body fat measurement is more predictive of health risk in firefighters than is BMI.

A strong support mechanism exists for fire department members to obtain information and assistance in health maintenance. The International Association of Fire Fighters and the International Association of Fire Chiefs have collaborated to develop a comprehensive Wellness-Fitness Initiative, which is available to all fire departments and department members. In addition, fire departments may also be able to obtain financial assistance from the Federal Emergency Management Agency in developing and managing their own wellness-fitness programs.

James L. Fleming, DO, MPH
Medical Director
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References

Response
Dr Fleming is absolutely correct to note that the National Fire Protection Association standard on comprehensive occupational medical programs has been updated. We express our apologies for citing an outdated standard in our December 2008 article.

Regarding our use of body mass index (BMI), we agree about the limitations of this measurement, especially among firefighters. Although the World
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Health Organization allows for BMI (≥30 kg/m²), waist-hip ratio (>0.90 in men, >0.85 in women), or both to be used in obesity assessments, the National Cholesterol Education Program Expert Panel on Detection, Evaluation, and Treatment of High Blood Cholesterol in Adults and the International Diabetes Federation Epidemiology Task Force Consensus Group suggest the use of waist circumference to measure obesity—as one component of metabolic syndrome.

Our use of BMI arose from inherent limitations of the self-report instrument used in our survey-based study. With a larger, funded study, waist circumference would likely be used in lieu of BMI, resulting in improved evaluation of health behaviors that modify cardiac risk factors (eg, physical activity, healthful diet).

We agree with Dr Fleming that wonderful mechanisms have been developed to support firefighters’ health. Regardless of the support mechanisms in place, however, volunteer firefighters may not have the time or inclination to be involved in these initiatives because they are not paid fire department employees and most often have other jobs.

In addition, many volunteer fire departments may not have the funding necessary to support such health-related amenities as weight-loss programs, gym memberships, or gyms in the firehouse.

Of course, these assumptions are based on anecdotal experiences, and this topic certainly deserves more serious attention in the medical literature.

Because respondents to our survey reported that they wanted to know more about cardiac risk factors—and they wanted their fire departments to play a more active role in providing members with opportunities to improve their health—perhaps needs assessments and evaluations of current health and wellness initiatives could also be conducted among the volunteer firefighting community.

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Lahey Clinic Medical Center
Tufts University School of Medicine
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Elizabeth Ablah, PhD, MPH
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References

Prevention, Diagnosis, and Management of Osteoporosis-Related Fracture

To the Editor:
Quality of life is an important consideration when evaluating treatment options to prevent osteoporosis-related fractures in postmenopausal women. The benefit-risk ratio of treatment varies for women, depending on age and severity of menopause-related symptoms.

The October 2008 review article by M. Jill Gronholz, DO,1 provides valuable information on the use of a multifactorial approach in the management of osteoporosis-related fractures. Unfortunately, Dr Gronholz seems to dismiss the role of hormone replacement therapy (HRT) as a viable mode of treatment for this population. The following single reference was made in regard to HRT: “Although hormone replacement therapy has long been an option for osteoporosis prevention, recent findings from the Women’s Health Initiative [WHI],2 resulted in new recommendations to limit its prophylactic use.”1

It is true that the WHI results indicated significantly increased risks of cardiovascular events and breast cancer for women taking estrogen and progesterone therapy. However, level 1a evidence continues to support the use of HRT for risk reduction of osteoporosis-related fractures based on an individual’s history, risk factors, and need for treatment of vasomotor symptoms.4

Results of the WHI showed that the use of conjugated equine estrogen (CEE, 0.625 mg daily) together with medroxyprogesterone acetate (MPA, 2.5 mg daily) reduced the risk of hip and vertebral fractures by 34%, and the overall risk of fractures by 24%, compared with placebo.5 (These percentages are calculated from the associated hazard ratios reported in the study.)5 This risk reduction amounted to five fewer hip fractures per 10,000 women per year.5

In regard to cardiovascular risk, recent data have revealed an important therapeutic window of opportunity for HRT implementation. In the WHI estrogen-plus-progesteron arm,6 coronary heart disease (CHD) relative risk results—in terms of hazard ratio (HR) and according to time since menopause—were the following (P for interaction =.33):

- <10 years since menopause, HR=0.89
- 10-19 years since menopause, HR=1.22
- <20 years since menopause, HR=1.71

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In the WHI estrogen-only arm, the relative risks of CHD death—according to age at randomization—were the following:

- age 50–59 years, HR=0.63
- age 60–69 years, HR=0.94
- age 70–79 years, HR=1.11

Thus, recent WHI results suggest that HRT may decrease coronary disease in younger women (i.e., those near the age of menopause), but it may increase the risk of a coronary event in older women.

Differences in age at HRT initiation and duration of therapy are relevant factors in treatment outcomes. These WHI results should modify the benefit-risk perception of HRT and support an individualized approach to management.

In another recent update, WHI investigators reported outcomes in patients 3 years after study medication (i.e., estrogen-plus-progestin or placebo) was halted. No increased risk for thrombosis, CHD, or stroke was observed during these subsequent 3 years in women who had received HRT. Moreover, a statistically significant increased risk for invasive breast cancer, associated with HRT, did not persist at 3-year follow-up. It is also important to note that in the estrogen-only arm of the WHI study, no increased absolute risk of breast cancer was observed during 6.8-year follow-up.

The effectiveness of low-dose HRT (CEE, 0.3 or 0.45 mg daily, combined with MPA, 1.5 or 2.5 mg daily) has been demonstrated for the treatment of patients with vasomotor symptoms or genital atrophy, as well as for the prevention of bone loss, with fewer adverse effects than standard-dose HRT. These initial results were confirmed by the HOPE (Women’s Health, Osteoporosis, Progestin, Estrogen) study. Further research, however, is needed to determine the clinical effects of low-dose HRT on fracture, cardiovascular disease, and breast cancer.

The review article by Dr Gronholz mentions only briefly the use of selective estrogen receptor modulators as a treatment or prevention option. Yet, when compared with placebo, raloxifene hydrochloride—approved by the US Food and Drug Administration for the prevention of osteoporosis—has been shown to decrease bone resorption, increase bone mineral density, and reduce fracture risk by 35% to 50% in women with osteoporosis (P<.001).

The North American Menopause Society’s position statement concludes that recent data support the initiation of HRT at menopause to manage vasomotor symptoms and to reduce the risk of osteoporosis in select postmenopausal women. As noted in the WHI results, the benefit-risk ratio for HRT is favorable when treatment begins close to menopause, but this benefit-risk ratio decreases with age and with time since menopause.

These findings should have been emphasized in the review article by Dr Gronholz. Furthermore, we, as osteopathic physicians, should consider these findings when making informed treatment recommendations to our patients—especially because our profession emphasizes a holistic approach to patient care.

In summary, recent data argue for the need to balance the benefits of HRT against its potential risks. Recommendations for individualized regimens and dosing of HRT need to be based on severity of symptoms—choosing the lowest appropriate dose for the shortest duration of time—as well as time since menopause.

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References


Dr Gronholz was shown this letter and declined to comment at this time.

Don’t Neglect the “Art” of Medicine

To the Editor:

As an “old timer” in the osteopathic medical profession, I was deeply moved by the thoughts expressed in “Pathos,” the poem written by Mai Kim Ho, OMS V, that appeared in the November 2008 issue of JAOA—The Journal of the American Osteopathic Association (2008;108:669). In my opinion, the future of our profession lies in the words of Student Doctor Ho and in her stated hypothesis: “Burning hearts united in their efforts to practice osteopathy can reignite the revolution.”

In our quest for improvement in patient care, too often the concentration of studies at colleges of osteopathic medicine has become strictly science-oriented—with far too little offered in the “art” of medicine.

It is my hope that more young people entering our profession discover, as Student Doctor Ho has expressed so eloquently, that there is much more to modern medicine than memorizing test answers and writing prescriptions.

Paul G. Kleman, DO
Professor Emeritus
West Virginia School of Osteopathic Medicine
Lewisburg

Corrections

The JAOA and the lead author regret a terminology error that appeared throughout the following November 2008 article:


The term “hemoglobin platelet count” was used incorrectly in the following locations when the terms “hemoglobin” and “platelet count” should have been used separately:

- page 646—“Methods” section of abstract
- page 647—fourth paragraph of right column
- page 648—fifth paragraph of “Comment”

In addition, “hemoglobin platelet count” was used in the following locations when only “platelet count” should have appeared:

- page 646—“Results” section of abstract
- page 648—second and fourth paragraphs of “Comment”
- page 650—second paragraph of “Conclusion”

Also, the first sentence in the “Results” section of the abstract originally read as follows:

“Significant condition X time interaction effects were observed for all subjects, indicating a decrease in hemoglobin platelet counts and an increase in diastolic blood pressure during the lymphatic treatment protocol.” Instead this sentence should have read as:

“Significant condition X time interaction effects were observed, indicating a decrease in platelet counts and an increase in diastolic blood pressure after the lymphatic treatment protocol.”

Finally, the biographical information at the bottom of page 646 should have stated that “Dr Rivers was a predoctoral Osteopathic Principles and Practices Fellow at Kansas City (Mo) University of Medicine and Biosciences College of Osteopathic Medicine at the time of the study.” It incorrectly stated that he was a fellow at Henry Ford Macomb-Clinton Hospital in Clinton Township, Mich. Likewise, Dr Rivers should have been identified as an “investigator” rather than an “osteopathic physician” at the bottom of the right-hand column on page 647.

These changes were made to the full text (http://www.jaoa.org/cgi/content/full/108/11/646) and Adobe Portable Document Format (http://www.jaoa.org/cgi/reprint/108/11/646) versions of this article online.

In addition, the authors of the following contribution regret an error that appeared in their May 2002 article:


The first reference on page 292 was submitted incorrectly and should have been given as follows: Hooper J, Cox CC, Cambre K, Willburn D, Webster M, Wolf T. Comparison of the scope of allopathic and osteopathic medical school health promotion programs for students. Am J Health Promot. 1999;13:171-179. This change was made to the Adobe Portable Document Format version (http://www.jaoa.org/cgi/reprint/102/5/289) of this article online.

Readers are encouraged to contact the JAOA’s managing editor, Rebecca J. Fiala, MA, by phone at (800) 621-1773, extension 8161, or by e-mail at rfiala@osteopathic.org regarding corrections (ie, errata or corrigenda), retractions, and notices of duplicate publication for materials published in THE JOURNAL.

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