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.

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.

**Another Case of Extended-Release Bupropion-Induced Seizure**

To the Editor:

I read with great interest the report on grand mal seizures being induced by extended-release bupropion hydrochloride (bupropion ER), written by David J. Rissmiller, DO, and Thomas Campo, DO,1 in the October 2007 issue of *JAOA—The Journal of the American Osteopathic Association*. I too have observed this association between bupropion ER and grand mal seizures.

In treating a 42-year-old woman who had been diagnosed with bipolar depression, I prescribed the anticonvulsants lamotrigine (titrated to a final dosage of 200 mg/d) and clonazepam (2 mg/d) over a period of 2 months. Bupropion ER (150 mg/d) was initiated at week 6 and continued for 2 weeks. Upon titration of bupropion ER to 300 mg per day at week 6, the patient experienced a grand mal seizure after 2 days of therapy at the increased dose. All use of bupropion ER was then immediately discontinued. Results of a computed tomographic scan and a lumbar puncture test both proved negative.

The patient was then rechallenged within a week with bupropion ER (150 mg/d initially, titrated to 300 mg/d at start of second week). She experienced a second grand mal seizure after 2 days of therapy at the higher dose.

The patient had reported no personal history of anorexia nervosa, bulimia, or epilepsy. She had previously been prescribed tramadol hydrochloride (100 mg/d) to relieve chronic shoulder pain, but tramadol had been discontinued 6 weeks prior to her first office visit in the present case.

In light of previous reports2–7 that there is a lower incidence of seizures with bupropion ER than with the immediate-release formulation, I feel that the case of my patient and that reported by Drs Rissmiller and Campo1 are clinically important. It is particularly meaningful that the seizures associated with bupropion ER in my patient occurred in conjunction with the use of two anticonvulsants, lamotrigine and clonazepam.

I hope that the report by Drs Rissmiller and Campo1 will generate renewed interest in the association between bupropion ER and grand mal seizures.

Douglas Martin Rosoff, DO
Ukiah, Calif

**References**

Embrace Evidence...With Both Eyes Open

To the Editor:
The August 2007 issue of *JAOA—The Journal of the American Osteopathic Association*, featuring six articles on evidence-based medicine (EBM), provides osteopathic physicians with a collection of tools for practicing EBM (2007;107:289-371). I am an evidence enthusiast, having published in the *JAOA* a systematic review revealing the inadequacy of the National Library of Medicine’s PubMed database for collecting evidence regarding complementary and alternative medicine.1 In addition, my recent meta-analysis2 adapted methods used by the Cochrane Collaboration to present an innovative study of nonclinical data. Despite my enthusiasm for evidence, I would like to point out that there are a number of valid criticisms and concerns about EBM that the *JAOA* theme issue failed to raise.

According to EBM, empirical evidence—especially that derived from randomized controlled clinical trials (RCTs)—is ranked as the best evidence on which to base a clinical decision.3-5 As a result, clinical experience and pathophysiologic rationales are relegated to subordinate positions. Yet, these “other ways of knowing” actually differ in kind—not in degree—from empirical evidence and do not belong on a graded hierarchy.3 Furthermore, a variety of hierarchies have been proposed by David L. Sackett, MD,4 Gordon H. Guyatt, MD,5 and other developers of EBM. Which one of these hierarchies is “best”? Dr Sackett4 has stated that EBM does not disregard the “...compassionate use of individual patients’ predicaments, rights, and preferences in making clinical decisions about their care.” Nevertheless, EBM guidelines have been hijacked by managed-care corporations as a rhetorical artifact for denying insurance coverage to patients when treatments are not yet fully supported by RCT evidence. My experience suggests that this misuse of EBM has been directed at osteopathic physicians who use osteopathic manipulative treatment (OMT).

Just this month, I was thrice denied reimbursement as a result of misuses of EBM. In these instances, the health maintenance organizations based their decisions on meta-analyses of chiropractic and physical therapy studies. The denials were signed by three osteopathic physicians—as if by review of RCTs somehow justifies the misapplication of these meta-analyses to OMT.

Clinical decisions based on RCTs may not always be applicable to individual patients because RCTs are based on patient populations. The standard RCT protocol that excludes from study all subjects with comorbidities makes EBM least applicable to patients who are most in need of clear evidence—those with chronic, complex illnesses.6 Physicians who rely on EBM but lack Dr Sackett’s aforementioned patient-centered approach risk becoming regimented and reductionist—and certainly not holistic. Indeed, RCT-based decisions counteract the emerging paradigm of individualized “molecular medicine.”7

All six special communication articles in the August 2007 *JAOA* repeat the claim that the purpose of EBM is to enable physicians to practice the best medicine possible. If that is indeed its purpose, then EBM fails to meet its own imperative. There is no evidence, as defined by EBM (ie, RCTs), demonstrating that EBM actually improves patient care.8 Advocates of EBM label it “objective” and “unbiased,” but EBM’s reliance on scientific literature is inherently skewed by “publication bias”—that is, meta-analyses with “negative results” (ie, inconclusive findings that do not support particular agendas) are less likely to be published.9-10 Yet, systematic reviews with dramatic titles tend to be weaker methodologically.9 Evidence-based medicine may also be biased by money and power. A number of peer reviewers for the Cochrane Database of Systematic Reviews were recently found to have undisclosed financial ties to pharmaceutical corporations that led to ethical lapses in their reviews of RCTs.11

Reliance on EBM canalsize clinical reasoning by structuring one’s approach to finding answers, as well as one’s approach to asking questions (eg, the PICO [patient population, intervention, comparison, outcomes] approach). Thus, there exists the danger of EBM becoming an institutionalized and authoritative “regime of truth.”12 Such a development runs contrary to the traditional outlook of osteopathic physicians, who have long opposed allopathic hegemony and long supported physician autonomy and medical pluralism.

Evidence-based medicine can be interpreted as a medical philosophy—perhaps the first philosophical foundation to be adopted in allopathic medicine. However, osteopathic medicine already has its own longterm and well-known philosophical underpinnings.13

Some medical professionals have described EBM as “outrageously exclusive” and even “fascist.”12 Bernadine Healy, MD,14 former director of the National Institutes of Health, recently wrote, “By anointing only a small sliver of research as best evidence and discarding or devaluing physician judgment and more than 90 percent of the medical literature, patients are forced into a one-size-fits-all straitjacket.”

In conclusion, osteopathic physicians should embrace EBM, but with common sense and with both eyes open—and without sacrificing our souls in the process.

John M. McPartland, DO
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References

A.T. Still Would Not Be Proud
To the Editor:

In the abstract, Dr Mueller mentioned that OMT “...may reduce pain input into the trigeminal nucleus caudalis, favorably altering neuromuscular autonomic regulatory mechanisms to reduce discomfort from headache.” I had never before heard of the use of OMT to reduce pain input into the trigeminal nucleus caudalis.

Unfortunately, the grandiloquent excitement I felt after reading this abstract was surpassed only by my horrific disappointment upon reading the rest of the article.

The article’s observations of diagnostic criteria and pharmacologic options (the latter likely sponsored by Purdue Pharma LP, the providers of the “educational grant” supporting the publication), as well as the case presentation, were thorough and informative. However, other than a generic blurb noting that “...OMT for paravertebral cervical spasm associated with headaches may be beneficial,” Dr Mueller made absolutely no mention of osteopathic medical considerations within the body of the article. The article did not even provide an explanation of the application of OMT to the trigeminal nucleus caudalis, despite mentioning this concept in the abstract. Isn’t the abstract supposed to be a summation of information contained in the body of the article?

During a time in which most osteopathic physicians are working diligently to refine and showcase the differences between ourselves and our allopathic colleagues, it is very disheartening to read an osteopathically written, edited, and published piece on an osteopathically manageable disease process that makes no mention of somatic dysfunction, muscle energy, high velocity/low amplitude (HVLA) technique, or even cranial-sacral technique. Andrew Taylor Still, MD, DO would not be proud. Although I realize that these techniques may not yet be “evidence based,” does that mean we shouldn’t even speak of them in our own journal?

Thank you, JAOA, for reaffirming the logic of my preference of turning to American Family Physician for my literature searches and continuing medical education. I look forward to 6 months from now, when I may have forgotten about this incident and again test the waters of the JAOA’s Web site.

Cory M. Fisher, DO
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Response
Letters to the editor noting “horridic disappointment” about a lack of osteopathic content in an article—especially vis-à-vis the use of osteopathic manipulative treatment (OMT) in migraine headache management—are nothing new.2 Unfortunately, this lack reflects a clear deficiency of research in this area.

It is disappointing, as suggested by Dr Fisher, that a search of the PubMed database for published studies on OMT and migraine yields only five articles—none of which contain original research. Systematic literature reviews have concluded that the few published studies on spinal manipulation and headache have had overall poor methodology, namely in the form of small sample sizes, lack of control groups and binding protocols, and inadequate methodologic descriptions of manipulative procedures.3,4 Thus, the data that Dr Fisher requests regarding OMT mechanisms is simply not available.

(continued on page 214)
In my article in the November 2007 supplement to JAOA—The Journal of the American Osteopathic Association (2007;107[Suppl 6]:ES10-ES16), I attempted to whet the appetite of researchers by proposing several mechanisms of action for OMT in migraine management, including reducing nociceptive input into the trigeminal nucleus caudalis. An understanding of basic neuroanatomy was inferred.

In regard to Dr Fisher’s comment about the grant supporting the publication of the JAOA supplement, none of the pharmaceutical products mentioned in my article are produced by Purdue Pharma LP. Nor have I had any relationship with Purdue Pharma LP in my headache practice or national lectureships.

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References

Moving Beyond Myths of Substance Use Disorders

To the Editor:
I was very pleased to read “Improving Physician and Medical Student Educa-

tion in Substance Use Disorders” by Stephen A. Wyatt, DO, and Michael A. Dekker, OMS III, in the September 2007 supplement to JAOA—The Journal of the American Osteopathic Association. Only briefly mentioned in this article, however, are the troubling attitudes of physicians regarding substance use disorders (SUDs).

Physicians commonly respond to these patients with distain and frustration, accepting the view common in our society that substance abuse is “…attributable to morally compromised or pathological individuals who were not properly inculcated in childhood with normal American values such as self-control and respect for the law. These individuals must be disciplined and punished by authorities.” Therefore, addressing SUDs is seen as a waste of clinical effort that has been referred to as “therapeutic nihilism.”

In a study published in 2002, physician satisfaction in treating patients with alcohol- or drug-abuse disorders was found to be only 49% and 31%, respectively. Furthermore, according to a study published in 2006, beliefs persist among physicians that patients with SUDs are guilty of overusing healthcare, detracting from the care needed by other patients. When physicians have the opinion that SUDs are voluntary disorders by addicts who “do it to themselves” and who refuse to change their behaviors, then our progress in education, screening, intervention, and treatment for patients with these disorders is seriously impaired. Fortunately, current understanding of the scientific basis of SUDs refutes such misconceptions.

The belief that addicts cause their own problems ignores the fact that genetic predisposition accounts for 40% to 60% of the etiologic basis of addiction. An individual may initially use alcohol or a drug for its euphoric effect—with genetic predisposition playing a part in an abnormal need for this euphoria. However, substance abuse becomes progressively less voluntary as the brain’s learning processes are altered by the biochemical effects of the substance of choice. Thus, current scientific evidence has begun to reveal that individuals with SUDs have been genetically “primed” to try alcohol or drugs and to subsequently become addicted to these substances.

Certainly, many of us in the healthcare professions have been frustrated by patients with SUDs who seemingly refuse to change their behaviors. Scientific evidence reveals, however, that the brain’s mesolimbic dopamine pathway is altered in individuals suffering from addiction. An abnormally high stimulation of this pathway results in an extremely strong drive for drugs. Learned drug-associated cues then become essentially irresistible because of the drug-induced impairment of planning and decision-making pathways in the prefrontal cortex. Simply put, sensitized reward “go pathways” become unchecked by impaired executive “stop pathways.”

Physicians need to be aware of effective treatments that are available to help addicts change their behaviors. These treatments include pharmacotherapeutics-based management, brief intervention, motivational interviewing, and cognitive-behavioral approaches. In fact, the treatment of patients with SUDs is just as successful as treatments for patients with other chronic illness, such as asthma, hypertension, and type 1 diabetes mellitus.

A scientific understanding of the chronic disease of addiction demonstrates both the fallacy of common myths surrounding SUDs and the efficacy of treatments available for patients thus afflicted. The challenge to the healthcare community regarding SUDs is to fulfill our role in patient screening, intervention, and treatment—and to move beyond myths.

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Editor’s Note: Dr Clay serves on the JAOA’s Editorial Board as the representative of Ohio University College of Osteopathic Medicine.

References

Corrections
The JAOA deeply regrets that prepublication edits requested by Jane C. Johnson, MA, were not incorporated in the following article:


The changes detailed below were made to the full text (http://www.jaoa.org/cgi/content/full/108/2/71) and Adobe Portable Document Format (http://www.jaoa.org/cgi/reprint/108/2/71) versions of this piece online as soon as these omissions were discovered:

Page 71—In the author notes at the bottom of the page, Ms Johnson’s affiliation was incorrectly shown as the Department of Family Medicine, Preventive Medicine, and Community Health at A.T. Still University-Kirksville (Mo) College of Osteopathic Medicine. Her professional affiliation is with the A.T. Still Research Institute at that institution.

Page 73—In the third paragraph of the first column, the last sentence read as follows in the original print publication: “The effect of varying the number of compressions was also analyzed using Kruskal-Wallis tests to compare the groups on any change from baseline levels.” The authors’ hypothesis is more accurately described by replacing “any” with “the” in the sentence shown.

In addition, the fourth paragraph of the “Results” section originally indicated incorrectly that there was a “small but statistically significant decrease in the mean percentage of endothelials, eosinophils, and basophils at 45 minutes posttreatment.” In fact, the decreases in these laboratory values did not reach statistical significance at that posttreatment interval.

Page 75—The original print publication included the word “Mean” at the beginning of the title for Table 3. That redundancy has been eliminated in the electronic versions of the article posted online.

In addition, the first paragraph of the left column of text inappropriately reported that “Group 4 erythrocyte counts showed a trend toward statistical significance.” That statement should have been modified to read as follows: “Group 4 erythrocyte counts were suggestive of a change over time.”

Finally, the next sentence in that paragraph incorrectly stated, “The Kruskal-Wallis test, also used to test for between-group differences...” The statistical test is more accurately described by removing the “also” from the phrase shown.

Page 76—In the original print publication, Table 4 incorrectly showed that the pooled observation time for 5+10+15 was statistically significant at P=.05. Because statistical significance was set at P<.05 for this investigation, these data should not have been identified with a footnote symbol as statistically significant.

Readers should also be aware of the following corrections to previous editions of the osteopathic medical education theme issue published by JAOA—The Journal of the American Osteopathic Association:


The American Association of Colleges of Osteopathic Medicine researches and updates its data regularly, pro-
viding corrected numbers on an annual basis in its Annual Statistical Report on Osteopathic Medical Education. Therefore, data from the American Association of Colleges of Osteopathic Medicine Application Service are to be considered incomplete until confirmed by multiple editions of that publication.

Consequently, several updates were made to Table 1 for subsequent editions of this annual JAOA contribution. The 2004-2005 data corrections noted below are for Table 1 and originally appeared in the same table on page 111 (J Am Osteopath Assoc. Mar 2007;107[3]:109-116):

- Previously reported data on the number of applications received by Touro University College of Osteopathic Medicine–California in Vallejo was updated from 2157 to 2156.
- As a result, the total number of applications received has been corrected from 46,750 to 46,749.
- The number of students enrolled at Philadelphia (Pa) College of Osteopathic Medicine has been corrected from the 1125 previously reported to 1025. (The total number of students enrolled at the nation’s COMs during that academic year remains unchanged at 12,525.)

The 2001-2003 and 2005 data corrections noted below are for Table 2 and originally appeared in the same table on page 120:

- The total number of applications was reported as 7259, but has been corrected to 6898.
- Though the number of applications reported by race/ethnicity remains unchanged, the percentages reported have been corrected as follows: Asian/Pacific Islander, 16.9% corrected to 17.8%; white (non-Hispanic), 65.2%, 68.6%; black/African American (non-Hispanic), 4.6%, 4.9%; Hispanic/Latino, 4.0%, 4.2%; Native American/Alaskan Native, 0.9%, 1.0%; and other or unknown, 3.4%, 3.6%.

- 2002
  - Though the total number of applications reported by race/ethnicity remains unchanged, the percentages reported have been corrected as follows: Asian/Pacific Islander, 16.9% corrected to 17.8%; white (non-Hispanic), 65.2%, 68.6%; black/African American (non-Hispanic), 4.6%, 4.9%; Hispanic/Latino, 4.0%, 4.2%; Native American/Alaskan Native, 0.9%, 1.0%; and other or unknown, 3.4%, 3.6%.

- 2003
  - The total number of applications was previously reported as 6813, but has been corrected to 6814.  
  - Though the number of applicants who self-identified as Asian/Pacific Islander remains unchanged at 1201, the percentage reported has been corrected to 17.6% from 7.6%.

- 2005
  - Most data and some percentages previously reported for this year have been corrected: (1) The total number of applications was originally reported as 8255, but has subsequently been corrected to 8258. (2) Asian/Pacific Islander was reported as 1668 and has been corrected to 1669, but the percentage reported remains unchanged. (3) White (non-Hispanic) was reported as 5229 and has been corrected to 5930, but the percentage reported remains unchanged. (4) Underrepresented minorities was reported as 969 (11.7%) and has been corrected to 971 (11.8%). (5) Black/African American (non-Hispanic) was reported as 491 (5.9%) and has been corrected to 492 (6.0%). (6) Hispanic/Latino was reported as 419 and has been corrected to 420, but the percentage reported remains unchanged. (7) Other or Unknown was reported as 389 and has been corrected to 388, but the percentage reported remains unchanged.


The changes described were the result of errors in reporting. The corrections noted below are for Table 3 and originally appeared in the same table on page 123:

- In 2004, 2 (1%) physicians were recertified in Neuroradiology. Previously, no recertifications were reported in 2004 for this specialty.
- Consequently, the total number of physician recertifications awarded by osteopathic medical specialty boards for 2004 was previously reported as 286. This number has been corrected to 288.
In addition, Preventive Medicine and Occupational Medicine physician recertification data from 2004 and 2005 were accidentally transposed with data for Preventive Medicine and Public Health. Although it was originally reported that there were no physician recertifications in Preventive Medicine and Occupational Medicine in either 2004 or 2005, there were actually 4 (1%) in 2004 and 1 (<1%) in 2005. Conversely, there were no physician recertifications in Preventive Medicine and Public Health for either year, instead of the 4 and 1, respectively, previously reported.

Finally, the JAOA regrets that the following typographic errors appeared in the April 2006 issue and the May 2006 issue, respectively.

  In the Table on page 195, the No and Yes column headings under “EPDS Score, >13” were accidentally reversed. The Yes column heading should have appeared first, at the top of the second column of data.

  The quoted paragraph in column two on page 291 should end after “...prior to graduation,” and the following paragraph should begin, “The validity of this examination will have an impact on graduation and licensure of all future osteopathic physicians.”

The April and May 2006 issues of the JAOA have been corrected online to reflect both changes, as noted elsewhere (J Am Osteopath Assoc. June 2006;106[6]:363).

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