Follow-up on “Symptoms associated with anthrax exposure: Suspected ‘aborted’ anthrax”

To the Editor:

Case reports are the front line of evidence in the evolution of medical practice. Despite the obvious weakness of case reports (ie, they deal with a small number of patients, even as low as a single person), they have great value as the starting point of discovery.1

Although we cannot predict whether the ideas presented in our January 2002 case report (JAOA 2002;102:41-43) will be sustained after further study—or after additional evidence is revealed—the clinical observations included in that case report continue to be of interest.

The patient we presented, a 37-year-old man with a high level of exposure to the spores of Bacillus anthracis, has improved since publication of the report. On presentation, he had a low PO2 level, pleural effusions, and abnormal lesions on imaging of the chest. Blood culture results and antibody tests for exposure to anthrax were negative. After 3 months of triple antibiotic treatment, the patient’s PO2 level is now 108; his fevers have subsided, but his strength has not returned. The patient was noted to be anergic to measles and Candida albicans, and the results of histiocyte complement test have also come back negative.

We have received many questions about our decision to relate the patient’s illness to anthrax exposure. Our intention was to present a single case for primary care clinicians that would begin to explore the experiences of clinicians who have patients with similar findings. None of the clinicians at our facility has seen a case of anthrax disease.

This patient was cultured for anthrax and tested for antispore antibody and antitoxin antibody by the Centers for Disease Control and Prevention, and all results were negative. Therefore, the patient presented in the case report does not have anthrax disease.

But there should be an explanation as to why a healthy 37-year-old male with no medical problems suddenly has fevers, shortness of breath with a PO2 of 67, pleural effusions, and abnormal lymph nodes. The fact is, thus far, we have failed to produce an explanation for the distress occurring in this patient. The temporal relationship between his exposure to B anthracis and his symptoms—and the fact that the symptoms were relieved with antibiotic treatment—make an infectious etiology high on our list of possible causes.

If anthrax exposure does prove to be the cause of this patient’s distress, our patient would not be the first to have symptoms from an anthrax exposure without being culture- or antibody-positive. A fact sheet on three laboratory occupational deaths included in a government report, “Report of Demilitarization of Fort Detrick,”2 includes the 1951 case of William Allen Boyles, whose death was classified as caused by anthrax, despite the fact that “the microorganism could not be (and never was) cultivated from blood, sputum, nose and throat, or skin taken at any time during the illness, nor from tissue and fluids taken at autopsy.” The diagnosis was made postmortem because of a small number of gram-positive bacilli resembling B anthracis found only in the paccoon granulations of the brain.

Arthur M. Friedlander et al3 reported the findings of a study of 60 rhesus monkeys exposed to B anthracis to test the effectiveness of antibiotics. In that study, one animal given ciprofloxacin died 5 days after exposure, presumably from an aspiration pneumonia diagnosed because of negative blood cultures for anthrax. Another monkey died 73 days after antibiotics were stopped; this time the death was attributed to urethral obstruction—again because blood samples were culture-negative for anthrax. A third monkey died 5 days after antibiotics were discontinued. Similarly, no cause could be found for that animal’s death.3

The anthrax bacterium causes damage through extensive replication in the blood. Its ability to expand so successfully is helped by proteins it secretes that “toxicitate” the cells it is invading. The proteins are benign until they bind to the surface of the cell where an enzyme trims off its outermost tip. Protective antigen, endema factor, and lethal factor are the proteins involved in cellular damage, allowing the spores to enter the cytosol of the cells.4

Without knowing what caused our patient’s illness, we have to take care of him. Granted, the temporal relationship to the exposure does not automatically mean there is a causal relationship. The patient’s condition has improved while on triple intravenous antibiotic therapy and became worse when intravenous antibiotics were discontinued.

This report gives clinicians an indication as to treatment options. Limited experience with anthrax disease makes all clinical information, including our patient’s presentation, important to share.

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References
Unity between osteopathic and allopathic medicine only answer to preserving osteopathic uniqueness

To the Editor:
This letter is an addition to the ongoing dialog regarding the uniqueness of osteopathic medicine and addresses, in particular, the comments of Brent Hutson, MSII, “DOs must preserve their uniqueness” (The DO 2001;42:15-16). I will discuss the effects of disturbing trends in the current health-care environment that certainly challenge the DO “uniqueness.”

Mr. Hutson, while passionate and well-meaning, emphasizes the differences between allopathic and osteopathic medicine, rather than finding their commonalities. He implies that it is impossible to unite the two professions under a single representative body or to test the two professions with a unified testing structure. His arguments on both accounts are neither convincing nor unique.

Let us first examine the elements that create the need for unity. Medicine today is under assault on many fronts. Frivolous lawsuits led by lawyers wishing to cash in on the current litigious trend in American culture are driving the cost of malpractice insurance upward, making it nearly impossible for small groups of physicians to afford group insurance at reasonable rates. One terrible result of this trend is that a portion of students no longer wish to practice obstetrics-gynecology because of the cost of malpractice insurance. It is unimaginable that the choice of one’s profession is dictated by insurance malpractice rates, rather than sincere desire!

On another front are the problems of insurance reimbursement and Medicare and Medicaid cost cutting. This is our bread and butter, folks; this is how we survive. Reimbursement is guided by Medicare, and despite any attempts to have a unified testing structure, it is only answer to preserving osteopathic uniqueness.

To the Editor:
In his case report, “Digoxin toxicity in a 26-year-old woman taking a herbal dietary supplement” (JAOA 2001;101:444-446), Michael E. Scheinost, DO, points out some of the often-overlooked risks associated with herbal supplements and should be commended for addressing this important public health issue. The following are related issues that we believe warrant further attention.

One of the dangers associated with herbal products is their classification as dietary supplements. Since the Dietary Supplement Health and Education Act of 1994 was passed, herbal products (medications) have been exempt from the regulatory oversight and the safety and efficacy regulations established for other biologically active products used in healthcare, namely prescription and over-the-counter medications. As a result, the public, and to a lesser degree, clinicians view herbal medications as relatively harmless and may not be aware of their biologic activity. This may explain why patients frequently attribute their physicians’ concerns to anti-herbal sentiments. Herbal medications, similar to some foods and traditional medications, can trigger interactions and allergic reactions and should be taken with caution by certain populations. Unknown cardiac glycoside activity in herbal medications is only further demonstrate that osteopathic physicians have treatment options in addition to allopathic medicine, rather than care that is viewed as different.

It is my view, and certainly the view of others, that for all branches of medicine to survive in today’s threatening environment, we must unify our voices so they can be heard louder than they have ever been heard before.

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Herbal medications should undergo rigorour evaluation

To the Editor:
In his case report, “Digoxin toxicity in a 26-year-old woman taking a herbal dietary supplement” (JAOA 2001;101:444-446), Michael E. Scheinost, DO, points out some of the often-overlooked risks associated with herbal supplements and should be commended for addressing this important public health issue. The following are related issues that we believe warrant further attention.

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one example of the potential risks and biologic activity of these products.

The public may not be aware that some herbal companies do not use good manufacturing practices. As a result, contaminants and unsafe adulterants such as mercury and arsenic may be in some of these products.

Although many physicians are inadequately educated about herbal products and nutritional supplements, it still remains worrisome that patients follow the “medical” advice of a health store clerk over that of a clinician. Our poison control center held an herbal information day and invited the public to ask related questions. A random survey of a portion of the callers indicated that fewer than 20% volunteered their use of herbal medications to their clinicians, and fewer than 33% were asked by their clinicians about taking such products.

As professionals, we need to become educated about nutraceuticals and understand why patients turn to herbal medications and to alternative medicine, then provide a nonjudgmental atmosphere for patients so they feel comfortable discussing their healthcare choices. As there are still relatively few well-designed, randomized, controlled clinical trials establishing the safety, clinical benefit, or effectiveness of many herbal products, we need to promote rigorous evaluation of such products, as well as communicate the message to patients that herbal supplements are not inert, but rather biologically active chemicals that should be considered medications.

As osteopathic physicians and students, we may be tempted to automatically accept the use of herbal medicines, as our profession is often considered a complementary modality. This is, however, exactly what we should avoid. It was, in fact, concern about the safety and effectiveness of medications and medical practices that served as a major impetus for Andrew Taylor Still, MD, DO, to develop osteopathic medicine.

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References

To kill, or not to kill...

To the Editor:

When a physician considers the issue of physician-assisted suicide (PAS), the underlying question is whether to end another's life. To many lay people, the question may not seem so difficult to answer. Is the patient's prognosis “terminal”? Is this person’s quality of life unbearable? Is the patient so depressed that he or she has lost the will to live? These questions might seem straightforward, cut-and-dried, or black-and-white, but this could not be farther from the truth. Physicians, patients, and their loved ones know the agony in attempting to cope with PAS. Therefore, physicians need to take a stand and be heard as experts on the issue. Medicine is an art, not an exact science—yet lawmakers set exact criteria that enable patients to kill themselves. This demonstrates the degree to which we have allowed our society to deteriorate. Physician-assisted suicide is an emotionally charged, subjective issue that cannot be converted into an objective, exact referendum to be decided at the voting booth.

How did we get to this point? What happened to place us in such a difficult situation? The issue of PAS first appeared on the Washington ballot in 1991 and was defeated. California voters considered the issue in 1992, and it was again defeated. In 1994, Oregon’s Death with Dignity Act passed despite the fact that the Oregon Medical Association and the Oregon Osteopathic Association were strongly opposed to PAS. The American Medical Association, the American Osteopathic Association, and the American Nursing Association continue to maintain strong opposition to PAS. Is anybody listening out there? Osteopathic physicians have taken an oath to “first do no harm” and to “give no drugs for deadly purposes.” But these patients are voluntary, terminal patients, right? In 1996, Lee et al found that 50% of physicians could not predict confidently that a patient’s condition was terminal. Further, surveyed physicians were unable to definitively recognize depression 33% of the time. So much for the objective criteria to qualify for PAS. The voting public is unaware of that side of the story.

To date, only one state—Oregon—has legalized PAS. Michigan and California have had the issue on the ballot, only to be defeated. A poll of California citizens showed that most were in favor of voluntary PAS for terminally-ill patients. California is expected to again ask its voters to decide this issue, styling the proposed law after the Oregon statute. California’s law would allow the physician to provide, but not administer, the lethal drugs to the terminal patient—a hair-splitting point but critical for passage of the proposal. This is, after all, the technicality that got the suicide pathologist, [Jack] Kevorkian, convicted of murder in Michigan after he successfully assisted in the killing of nearly 130 human beings.

In 1992, D.C. Clark of the Center for Suicide Research and Prevention stated that patients with terminal illness and intractable pain are usually grateful that no one facilitated their suicide while they were temporarily depressed or having difficulties with pain. Clark also stated that 95% of suicide patients suffered from depression, yet only 2% to 4% of suicides were in terminally-ill patients.

We must concentrate on effectively treating pain and depression in terminally-ill patients. Hospice can provide this service. The suicide rate among hospice patients is generally less than one tenth of 1%. That is because hospice manages pain and provides emotional and spiritual support for the patient and family. Rather than helping to facilitate the suicides of terminally-ill patients who are desperate and afraid, we should support the peaceful completion of their journey.

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Letters

References
To the Editor:

The article by Maurizio A. Miglietta, DO, et al, “Evaluation of spine injury in blunt trauma” (JAOA 2002;102:87-91) is an excellent discussion of the difficulties physicians face trying to “clear” the spine. In the “Indications for subspecialty consultation” section of the article, however, the authors define the three-column theory as consisting of (1) the anterior half of the vertebral body and anterior longitudinal ligament; (2) the posterior half of the vertebral body, posterior longitudinal ligament and facets; and (3) the spinous processes, lamina arcus vertebrae, and interspinous ligaments. The three-column spine theory proposed by Francis Denis, MD, which expanded on the Holdsworth two-column theory, actually defines the column differently. The correct three-column spine consists of (1) the anterior two thirds of the vertebral body, the anterior longitudinal ligament, and the anterior annulus fibrosis; (2) posterior third of the vertebral body, posterior longitudinal ligament, and the posterior annulus; and (3) posterior bony complex and ligamentous structures. The facet joints are included in the posterior column of both the two- and three-column theories.

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References

Response

To the Editor:

The comments offered by Andrew D. Mullins, DO, indicate that he thoroughly reviewed our article regarding spine clearance in trauma. As Mullin states, Francis Denis, MD, first introduced the three-column theory, which is an expansion of Sir Frank Holdsworth’s initial work. Denis’ analysis of 412 thoracolumbar injuries in 1983 systematically classified the major types of spinal injuries radiographically and biomechanically. The Denis theory remains the accepted classification.

To address Mullin’s point regarding the distinction of halves versus thirds of the vertebral body in delineating columns, I have reviewed several sources. Denis describes the three columns as follows: (1) anterior column consists of “anterior longitudinal ligament, anterior annulus fibrosus, and the anterior part of the vertebral body”; (2) middle column consists of the “posterior longitudinal ligament, posterior annulus fibrosus, and the posterior wall of the vertebral body”; and (3) posterior column consists of “posterior bony complex (posterior arch) alternating with posterior ligaments complex (supraspinous, interspinous, capsule, and ligamentum flavum).” The illustration included with Denis’ article, however, seems to agree with the halves versus thirds breakdown of the vertebral body. The two interpretations of the three-column theory appear in other texts as well. 3, 4

I believe the millimeters of difference in anatomic distinction of the three-column theory is less important than the overall concept of three columns and spine instability. However, I appreciate Mullin’s attention to this discrepancy in the literature.

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References