WRONG-SITE SURGERY

To the Editor:

In the editorial, "Medical Errors and Wrong-Site Surgery," by Dr D'Ambrosia and Ms Kilpatrick in the March 2002 issue of Orthopedics (25[3]:288) it states, "it is estimated that 1 of 4 orthopedic surgeons who practice for 25 years will perform at least one wrong-site surgery." That certainly is an attention grabber, but is not supported by the data within the body of the article. It states there have been 150 total cases since 1996. Only 126 could be analyzed, and only 41% of these related to orthopedic surgery. If we assume that 41% is the average ratio of the cases in the total number of 150 cases, this equates to 62 cases in 5 years or roughly 12 cases per year. In a 20-year period, therefore, one would expect 240 total cases of orthopedic wrong-site surgery. Even considering only board-certified orthopedic surgeons, there are 15,318 surgeons currently in practice. If the estimate of 1 in 4 orthopedic surgeons performing wrong-site surgery were true, one would expect 3830 cases in a 20-year period, or 957 cases in a 5-year period. On the other hand, you report 126.

By my calculation, given your numbers, at the most 2% or 2 in 100 surgeons would experience wrong-site surgery. Any wrong-site surgery is to be condemned and avoided. I do not object to us looking at this, I only object to gross exaggerations, failure to be consistent in the statistics, and scare tactics.

Lee D. Hieb, MD
Yuma, Ariz

Reply:

Thank you for your letter. It is good to know the editorials are being read so carefully by our readers. The statistic you mention in your letter is a quote from Dr Terry Canale, past president of the American Academy of Orthopaedic Surgeons (AAOS), in conjunction with a 1998 AAOS statement on wrong-site surgery. The original quote from Dr Canale stated, "one out of every four orthopedic surgeons who practice for 35 years will perform at least one wrong-site surgery," not 25 years, as stated in the editorial. This statement should have been referenced in the editorial. The other statistics were taken from the Joint Commission on Accreditation of Healthcare Organizations 2001 report.

Robert D’Ambrosia, MD
Jennifer A. Kilpatrick, BA, ELS

"DEADMAN THEORY"

To the Editor:

In the article by Bono et al that appeared in the April 2002 issue of Orthopedics (25[4]:399-402), which was devoted to disproving the conclusions of my "Deadman Theory" of suture anchors (Arthroscopy. 1995; 11:119-123), the authors, by their own admission, did not prove anything.

There are several problems with this study. First, a uniform material such as polyurethane "foam bone" should have been used to eliminate the huge variability of bone density that occurs in cadaver specimens. Second, the testing angle of 30° abduction (to recreate the angle of largest force generation of the supraspinatus) has no basis in practice since most orthopedic surgeons place the arm in a sling at the side postoperatively, therefore, inadvertent force generation would generally occur at 0° abduction rather than 30° abduction. Third, the authors’ assertion that "there was no demonstrable trend between decreased angle and increased pull-out strength" is followed by the statement that the statistical power was 0.32 (insufficient to make the above assertion about the data "trend").

I hope your readers will not reject a readily-verifiable mechanical analysis (the original "dead-man theory" which is, in fact, intuitively obvious) on the basis of a poorly-modeled experiment that yielded inconclusive data.

Stephen S. Burkhart, MD
San Antonio, Tex

Reply:

We appreciate Dr Burkhart's comments regarding our article. Most importantly, we wish to emphasize that this work was not intended to disprove his "Deadman Theory." We agree that the theory is intuitive and by all means readily verifiable. However, in an extensive review of the literature, we (continued on page 131)