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Arthroscopic Repair Versus Open Surgery for Shoulder Instability

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LETTERS TO THE EDITOR

Orthopaedic Information on the Internet

To The Editor:

We read with interest "Evaluating the Source and Content of Orthopaedic Information on the Internet. The Case of Carpal Tunnel Syndrome" (82-A: 1540-1543, Nov. 2000), by Beredjikian et al. The design of this study is similar to that of our study¹, to which the authors made no reference in their article. In our study we used six popular search engines to search for four orthopaedic phrases. We evaluated the first fifty sites with regard to language, type of information, and its relevance to medical professionals or patients. Beredjikian et al. improved upon our study design through the introduction of a score for the informational value, which revealed a rather low mean score of 28.4 points of a maximum 100, with a high standard deviation of 28.3 points. As the mean score was calculated from all 175 investigated sites with no distinction made as to the sites' authorship, such a result is understandable. From our own experience we know that the list of URLs (universal resource locators) produced by a search engine's phrase search has a very mixed content that includes a high percentage (41% in our study) of commercially oriented sites designed with no intention to educate. We think that in order to assess the quality of information about a medical topic on a number of Internet sites, it is crucial to distinguish clearly among sites that are intended to inform medical professionals and patients (we could identify only 30% of the sites as intended for patients) and those that are designed not to provide direct information on medical topics but to point to index servers, journals, books, and congresses (almost 20% in our study). In our opinion it would be misleading to apply to such sites a score that measures the value of the clinical

informational content.

A factor not addressed by Beredjikian et al. that should be included in an evaluation of Internet sites is the availability of multiple languages. The Internet is international and multilingual. Any search engine into which a search phrase is entered that is not clearly restricted to a single language—as is true for many medical terms—will also list non-English-language sites. This seems to be an especially important consideration if we focus on the availability of information to patients on the Internet. For the majority of patients from minorities or non-English-speaking countries, only information in their own language is relevant.

Nevertheless, this article is a valuable one as it demonstrates the huge amount and variety of information on the Internet, which is increasingly important for our patients. We completely agree with the authors that we, as professionals, have the responsibility to monitor the quality of information pertaining to our medical specialties on the Internet.

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P.K. Beredjikian, D.J. Bozentka, D.R.

Steinberg, and J. Bernstein reply:

We thank Dr. Nogler and colleagues for their comments regarding our manuscript. They state that the mean informational value of the web sites

evaluated in our study is misleadingly low (28.4 out of a maximum 100 points) since web sites designed without an intention to provide medical information are included in the tally. We recognized this issue at the time of manuscript preparation, and for this reason the informational score of the web sites was broken down by author type, including the categories of commercial site, physician, and lay authorship (Table I). In addition, we calculated the mean informational value of our web-site roster with the noninformational sites excluded (mean score, 41.1 points). This datum can be found in the Results section of our article.

We agree with Dr. Nogler and colleagues with regard to the international nature of the Internet. We encourage investigators in non-English-speaking countries not only to investigate the quality of medical information on the World Wide Web that is available to the populations they serve but also to be proactive in posting quality health-care information in their respective languages.

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procuring information about orthopaedic foot and ankle problems from the World Wide Web. *Foot Ankle Int.* 1999;20:322-5.

Arthroscopic Repair Versus Open Surgery for Shoulder Instability

To The Editor:

We took great interest in the article "Comparison of Arthroscopic and Open Anterior Shoulder Stabilization. A Two to Six-Year Follow-up Study" (82-A: 1108-1114, August 2000), by Cole et al. We agree with the authors about the importance of comparing the outcomes of arthroscopic repair and open surgery for shoulder stabilization. However, we would like to comment on some of the issues that the authors have attempted to address.

The title of this article implies that the authors have compared the effectiveness of open procedures with that of arthroscopic methods in the treatment of recurrent shoulder instability. However, it is clear that patients with isolated Bankart lesions were treated arthroscopically while patients with evidence of anteroinferior capsular laxity were treated with an open procedure alone. The study groups were obviously different and were not randomized; therefore, direct comparison of the two methods cannot be made without the risk of significant systematic bias.

Furthermore, in their introduction the authors state that it was their aim to determine the effectiveness of their method of patient selection for open repair or arthroscopic stabilization surgery. To do so, they tested the null hypothesis that there is no difference between the two treatment groups with respect to the final clinical outcome. We are unable to understand how the finding of no statistically significant difference in the final outcome between two distinctly different groups of patients with diverse pathology can possibly lead to validation of a method of treatment selection. Conversely, would they have rejected their null hypothesis (and thus have concluded that their method of treatment selection was invalid) if one of the two methods had resulted in significantly superior results compared with the other?

Moreover, regarding the authors' conclusion that the two surgical methods "yield comparable results," it appears to us that this trial had little chance of detecting major differences between the two groups studied due to the small sample size (thirty-seven arthroscopic repairs and twenty-two open reconstructions). Although the article does not include the standard deviations for the American Shoulder and Elbow Surgeons' standardized assessment score, if one assumes a standard deviation of 16 points¹⁻³ and a study power of 80%, detection with use of the t test of a real difference between the two groups in the score (10 points) would necessitate inclusion of at least forty-two patients in each group (Minitab 12.1; Minitab Inc., State College, Pennsylvania). Also, although the authors did not do a power study and they did not report a beta value for their study, they nevertheless state that "there was no significant difference between the two groups"; this clearly contradicts the statistical policy of *The Journal*.

We would suggest that, in future studies, prospective, randomized methodology be used to test the relative effectiveness of open versus arthroscopic anterior stabilization for shoulder instability in comparable patient groups.

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B.J. Cole, J. L'Insalata, J. Irrgang,
and J.J.P. Warner reply:

We appreciate the interest of Mr. Karnezis and Mr. Sarangi in our article. They appropriately highlight the fact that the title implies that we presented a population of patients whose treatments were comparable simply because the underlying clinical manifestation of

their pathoanatomy was that of a singular entity, recurrent anterior shoulder instability. We agree that the title, read out of context, could be misconstrued in this fashion. It was our intention to report retrospectively the results of patients with similar clinical presentations but differing pathology, ranging from isolated Bankart lesions to frank capsular laxity, in an effort to demonstrate the efficacy of contemporary decision-making in determining the appropriate surgical procedure.

We were careful to elaborate on this issue of group comparability in the Abstract, Materials and Methods, and Discussion sections. We clearly stated that this study reports a series of consecutive patients diagnosed with recurrent traumatic anterior shoulder instability for whom the choice of surgical procedure was made according to the pathology identified at the time of the index procedure. Furthermore, we believe and we acknowledged that these study groups were comparable in terms of etiology, age, and chronicity of the instability. We admitted in the Discussion section, in which we described the limitations of this study, that "we did not perform a truly randomized prospective study with an absolutely pure patient population to compare arthroscopic and open stabilization techniques, [but rather] we sought to optimize the indications for each technique in order to improve their respective outcomes."

Mr. Karnezis and Mr. Sarangi raise an interesting issue regarding our stated null hypothesis that, using our method of treatment selection (based upon defined perioperative decision-making), there would be no difference in the final clinical outcomes between the two treatment groups. In fact, implicitly our conclusion was that our results supported the premise that the application of the described selection criteria led to comparable outcomes in terms of the recurrence of instability. We agree, however, that conceptually there is much work to be done to statistically validate our method of decision-making in order to truly define which groups are optimally treated by arthro-

scopic repair and which are optimally treated by open procedures. Admittedly, the variables that we currently believe to be critical for success may, as of yet, be only poorly defined. Furthermore, generalizations from the results of a single arthroscopic technique performed by a single surgeon are of limited value to the general orthopaedic community. Nevertheless, most of us who manage patients with shoulder instability are frequently faced with the issue of which patients are optimally treated by either an arthroscopic or an open stabilization technique. It was our goal to help the readership understand this process more clearly through an honest appraisal of a single surgeon's experience. It is important to note that the senior author (J.J.P.W.) initiated his arthroscopic practice in 1991, a time when arthroscopic techniques were heavily scrutinized.

Just as we would be reluctant to conclude that our selection process was absolute and all-inclusive had our null hypothesis been rejected, we most certainly would not have concluded that patients should have all been treated by one method or the other. In other words, we believe that refined selection criteria and an understanding of the pathology associated with this clinical entity are critical to the success of any surgical procedure selected. Appropriate decision-making with proper respect for surgical indications will always be the standard to which we will all be held.

Mr. Karnezis and Mr. Sarangi accurately recognize that no formal power study was performed for this analysis. We were careful to indicate in our Results that, "with the numbers available, there was no significant difference between the groups." Thus, we admit that, with larger numbers of patients in each group, statistically significant differences between the two groups may have occurred. Unfortunately, the lack of a power analysis is an inherent limitation of any retrospective, nonrandomized study of consecutive patients over a defined time-period.

All of the authors of this study agree that the arthroscopic technique

applied in this report has certain limitations and that contemporary techniques that address both the capsule and the labrum (e.g., suture anchors) may offer results comparable with even the best results following open stabilization. We also agree, however, that a truly prospective, randomized methodology comparing arthroscopic with open stabilization techniques is critical to validate this conclusion and to obviate the inherent limitations of our study.

Once again, we would like to thank Mr. Karnezis and Mr. Sarangi for the time and energy spent generating their observations and commentary; we sincerely hope that we have adequately addressed their concerns and would look favorably upon any future correspondence.

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Autologous Blood and Allogeneic Transfusion

To The Editor:

As one of the ten surgeons who "participated in the study" entitled "Efficacy of Postoperative Blood Salvage Following Total Hip Arthroplasty in Patients with and without Deposited Autologous Units" (82-A: 951-954, July 2000),

by Grosvenor et al., I have several comments regarding this manuscript.

I have serious reservations regarding the validity of the authors' results. The authors used the end point of the transfusion of allogeneic blood as the determinant of the efficacy of postoperative blood salvage. However, they studied a consecutive series of patients who had been treated by ten different surgeons who ordered allogeneic blood for their individual patients. In the Materials and Methods section of the manuscript, the authors state: "The decision to perform a transfusion was made by each surgeon and patient without a formal protocol." Therefore, the authors' statement in the Abstract that patients who had postoperative salvage "required" allogeneic blood one-tenth as often as those patients who did not have postoperative salvage is inappropriate. These patients merely "received" allogeneic blood less often. The only way the authors could have made a valid conclusion in this study would have been to have had all of the participating surgeons follow strict criteria for ordering allogeneic blood.

At the institution from which this study emanated and during this study period, I performed fifty-four consecutive primary total hip arthroplasties on patients in whom the CBC ConstaVac cell-salvage system was employed (these patients were a subset of the ninety consecutive patients included in the authors' study). I had prospectively recorded and analyzed complete data on all fifty-four of my patients. Of the fifty-one patients who predonated autologous blood, eight did not have postoperative blood salvage because of insufficient volume or other problems and forty-three received an average of 401 mL of postoperative salvaged blood. One patient in each of these subsets received an allogeneic transfusion; however, the difference in the prevalence of allogeneic transfusion was not significant ($p = 0.17$, chi-square test). This analysis would indicate that postoperative blood salvage does not reduce the risk of allogeneic transfusion for patients who predonate autologous units, contrary to what the

authors conclude. Since my patients were treated by only one surgeon who ordered transfusions according to a consistent protocol, it would appear that the conclusion that I reached from a subset of the authors' series is valid and their conclusions are not.

I believe that the use of predeposited autologous blood, not postoperative blood salvage, is the main determinant in the prevention of allogeneic transfusion. The efficacy of blood salvage following total hip arthroplasty can only be determined in a study in which patients who do not predeposit autologous blood are randomized to the use or non-use of postoperative blood salvage and in which there are strict criteria for the use of allogeneic transfusion.

Finally, there may have been a bias that affected the results for the two groups of patients in the study. Surgeons who used the postoperative blood-salvage system may have been more likely to avoid the use of allogeneic blood than surgeons who did not use the salvage system. In other words, the surgeons who used postoperative blood salvage may have had a higher threshold for ordering allogeneic transfusion than the surgeons who did not use postoperative blood salvage, thus skewing the results in favor of the study group.

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D. Grosvenor and S. Goodman reply:

We thank Dr. Woolson for his comments concerning our manuscript. The purpose of our study was to examine blood utilization in total hip replacement patients in an effort to optimize patient care. The study was initiated as part of a comprehensive examination of total hip replacement at our institution. Optimal blood utilization in total hip replacement was identified as an area for improvement by the University Healthsystem Consortium and by Stanford Medical Center.

Regarding Dr. Woolson's reservations about the outcome measure of our study, allogeneic transfusion has

been an accepted end point in several previously published studies on postoperative and intraoperative blood-salvage techniques. Dr. Woolson cites our Materials and Methods section as stating: "The decision to perform a transfusion was made by each surgeon and patient without a formal protocol." While his concern about the variability in choosing when to transfuse patients is valid, our Materials and Methods section states that "autologous and allogeneic units were transfused on the basis of a variety of criteria . . . such as dizziness; a hematocrit less than 0.30; and a history of angina, myocardial infarction, tachycardia, or postural hypotension." Importantly, the criteria for blood transfusion following total hip replacement are multifactorial, colinear, and often highly confounded. The surgeons were surveyed during data collection about their transfusion practices. The responses demonstrated that the primary indication for transfusion was the surgeon's judgment based on one or more of the above-mentioned criteria. Whereas we agree with Dr. Woolson that the patients "received" allogeneic blood, in the surgeons' minds this blood was "required." We disagree with Dr. Woolson's statement that "the only way the authors could have made a valid conclusion in this study would have been to have all of the participating surgeons follow strict criteria for ordering allogeneic blood" because there is no consensus on a transfusion trigger after total joint replacement. A truly blinded, randomized, prospective study is both impractical and implausible due to difficulties associated with surgeon blinding and patient care.

Concerning Dr. Woolson's analysis of a subset of study patients, the patients in our study were selected for inclusion according to the criteria cited in our paper. We are unaware of the specific inclusion criteria used for Dr. Woolson's fifty-four patients. The strength of our analysis was the logistic regression model that controlled for key perioperative variables while containing enough subjects to demonstrate sufficient power. A chi-square analysis of a subset of patients and a logistic regres-

sion analysis of an entire study population most probably will yield different results and different conclusions. It should be noted that our statistical model was reviewed in detail by a biostatistician selected by *The Journal*. Furthermore, the numbers in each of the cells in Dr. Woolson's chi-square analysis (one allogeneic transfusion each among the cases and controls) have inadequate power for validity.

We agree with Dr. Woolson regarding the value of autologous predeposited blood in preventing allogeneic transfusion. This is so stated on pages 953-954 of the article: "Depositing units of autologous blood before operations resulted in the most substantial reductions in allogeneic transfusions in our study and others. . . . The results of our study showed postoperative blood salvage to significantly reduce the risk of allogeneic transfusion after total hip replacement in patients who had deposited autologous blood and in those who had not ($p < 0.0001$)."

Dr. Woolson's final concern was potential bias between cases and controls. Because our study was retrospective, surgeons were without any incentive or inherent bias to alter their transfusion practices based on their use of postoperative blood salvage. Therefore, we disagree with Dr. Woolson's opinion that surgeons using postoperative blood salvage may have had a higher threshold for ordering allogeneic transfusion.

We thank Dr. Woolson for his comments and *The Journal* for the opportunity to respond.

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Attracting Female Candidates to the Field of Orthopaedic Surgery To The Editor:

I am writing with regard to the article "Initial Review of Electronic Residency

Application Service Charts by Orthopaedic Residency Faculty Members. Does Applicant Gender Matter?" (83-A: 65-70, Jan. 2001), by Scherl et al.

I feel that the authors' efforts are misdirected in this study. The issue is not that female applicants are not being accepted into orthopaedic residencies. The issue is that women are not choosing to be orthopaedic surgeons.

One of the most obvious reasons why women do not choose to specialize in orthopaedic surgery is that it requires a physically demanding, call-intensive, five-year residency. For women, the years between age twenty-five and thirty-five are prime childbearing years. There is very little leeway in either the schedules or the policies of most orthopaedic residency programs to accommodate pregnancy, maternity leave, or the needs of a working mother.

Instead of studying the application process, the authors should try to attract more women to orthopaedics by working to provide more role models, to change prevailing attitudes about female orthopaedic surgeons, or to arrange better child-care options.

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*S.A. Scherl, N. Lively, and
M.A. Simon reply:*

We completely agree that women will never choose orthopaedic surgery until the specialty addresses the "upstream" challenges involving perception and

support that deter potential women candidates. In fact, the introduction to our article acknowledged this issue, and its conclusion stated that "additional efforts should be made to familiarize female students with the profession early." To combat misperceptions and to attract female candidates, the field of orthopaedic surgery needs to have mentors and peers in place. We won't have them until we attract and match a sizable number of candidates with orthopaedic residency programs nationwide. In order to match those qualified women who do apply, we need to have a fair and equitable application process. Our study was an attempt to find out whether bias against women occurs if they do decide to make orthopaedics a career. We'd also like to point out that this project was initiated, in part, in response to anecdotal complaints (posted on an Internet bulletin board for female medical students interested in orthopaedics) from women who felt that, despite exhibiting obvious interest and dedication and having the proper academic credentials, they were not treated fairly during the application process. In other words, there is a perception among potential female candidates that the playing field isn't level, and that perception in itself could deter a woman from applying for a residency in orthopaedic surgery.

Interestingly, Dr. Stickles seems to have subscribed to some of the misperceptions that educators in the field of orthopaedic surgery should be combating. Is a residency in orthopaedic surgery really any more "physically demanding" or "call-intensive" than the average residency in obstetrics and gynecology or general surgery? Certainly,

the post-residency practice of most obstetricians entails an enormous amount of unpredictability, late-night calls, and physical exertion, possibly more so than that of the average practicing orthopaedist. No one would argue that women are unsuited to that field, or that the field itself is "inhospitable" to women of childbearing age or to working mothers. Moreover, though faculty at residency programs with higher percentages of female residents (such as pediatrics) may be more accustomed to negotiating and managing issues of maternity leave and child care, we are not convinced that those at residency programs in orthopaedic surgery would necessarily be less accommodating.

In summary, we agree with Dr. Stickles. Programs in orthopaedic surgery should make efforts to attract women by providing female mentors in college pre-medical programs and in the first two years of medical school. Once women decide to become orthopaedic surgeons, our study shows that there is no bias in the application process through the Electronic Residency Application Service. Any bias that may occur during the interview process is unknown.

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