Fluoroscopic Imaging of Acetabular Cup Position During THA Through a Direct Anterior Approach

To the Editor:

The recent article “Fluoroscopic Imaging of Acetabular Cup Position During THA Through a Direct Anterior Approach,” by Alvarez et al., was well written, and the content should be beneficial to surgeons performing total hip arthroplasty (THA) using this approach. However, the article contains an error that could potentially mislead surgeons using a direct anterior approach toward achieving the desired cup anteversion.

According to Figure 3’s caption, when the fluoroscopic beam is moved in the cephalic direction, the cup position appears more anteverted, and when it is moved in the caudal direction, the cup position appears less anteverted. This should actually be the other way around—when the beam is moved in the cephalic direction, the cup should look less anteverted, and when the beam is moved in the caudal direction, the cup should look more anteverted. This is evident when looking at Figures 3A (showing less anteversion) and 3B (showing more anteversion).

Ajit J. Deshmukh, MD
Parthiv A. Rathod, MD
Jose A. Rodriguez, MD
New York, New York

REFERENCE


Reply:

We thank Drs Deshmukh, Rathod, and Rodriguez and Dr D’Ambrosia for pointing out this oversight. Figure 3’s caption mentioned that when the fluoroscopic beam is moved in the cephalic direction, the cup position appears more anteverted, and when it is moved in the caudal direction, the cup position appears less anteverted. However, this should have read that when the beam is moved in the cephalic direction, the cup should appear less anteverted, and when the beam is moved in the caudal position, the cup should appear more anteverted. The Figure and corrected caption are printed here.

Andres M. Alvarez, MD, MSc
Weston, Florida

Long Bone Osteomyelitis in Adults

To the Editor:

The article “Long Bone Osteomyelitis in Adults: Fundamental Concepts and Current Techniques,” by Sanders and Mauffrey, is irresponsible and self-serving: the figures are inaccurate, nonsensical, and misleading; the case study is mis-staged and overtreated; and the conclusions are incredulous and so, therefore, is their treatment algorithm. The hypothesis of the article is that:

1. Clinical trials to support and standardize treatment are currently lacking;

2. Treatment algorithms based on a classification system with low interobserver variability somehow contribute to the “problem”; and

3. Surgical methods aimed at local excision are bound to failure.

At the conclusion of the article, the authors present a “universal treatment algorithm for adult osteomyelitis” based on a clear misinterpretation and misrepresentation of the current scientific literature, an apparent lack of surgical experience, and high disregard for outcomes and patient well-being.
STANDARDIZATION OF TREATMENT

Osteomyelitis treatment protocols provide guidelines to the diagnosis and treatment of the infection. Reconstruction, after surgical treatment of the disease, presents treatment options with different subsets of guidelines designed to restore form/function at the hands of caretakers with varying skill sets. Yes, antibiotic protocols are still evolving. However, the surgical treatment of this disease has been vetted and established for decades. I refer the authors to Table 2: A Protocol for the Treatment of Adult Osteomyelitis on page 144 of AAOS Orthopaedic Knowledge Update: Musculoskeletal Infection.

Obviously, Figure 1 (Cierny/Mader anatomic classification) was never vetted by the editors: it is incorrect, and the legend is inept. The superimposed, green-shaded areas do not designate the correct extent of cortical and medullary necrosis assigned to the anatomic types in the Cierny/Mader system. In Figure 1B (superficial osteomyelitis; type II), there is no violation of the skin; in Figure 1C (localized type III), there is no violation of skin or medullary extension to designate a localized lesion; and in Figure 1D (diffuse osteomyelitis; type IV), there is skin violation but no cortical permeation or medullary extension.

LOW INTEROBSERVER VARIABILITY

Low interobserver variability in a classification system is a positive, not a negative, feature. The Cierny/Mader Clinical Staging System for Adult Chronic Osteomyelitis would not have withstood the test of time and currently be considered the global standard if it were not user-friendly, intuitive, and reproducible. However, one must understand the pathophysiology and natural history of the disease to correctly interpret diagnostic studies and scientific communications.

For example, it is not unusual for a medullary lesion (type 1) to be associated with considerable, extracompartmental soft tissue edema on magnetic resonance imaging; however, this does not necessarily mean that the intervening cortex is dead, overclassifying the lesion to type III or IV osteomyelitis. Similarly, marrow edema on the endosteal side of a superficial osteomyelitis (type II) does not mean it is localized or diffuse.

Figure 2 does not show a Codman’s triangle!

In Figures 3A and 3B, the authors classify this lesion as a diffuse osteomyelitis (type IV), but clinical staging studies suggest that it is a hematogenous osteomyelitis (type I) with a biopsy window. The magnetic resonance image shows no cortical extension or satellites to corroborate the findings suggested in Figure 6A. At our center, this lesion would be managed with a low-risk surgical protocol that preserves bone stock and weight bearing: first stage=medial unroofing, antibiotic cementation+rebar and a cast; second stage=bone grafts/limited-contact plate. According to the information provided, block resection and a true, wide margin exposed this patient to excessive morbidity, risk, and potential disability.

SURGICAL METHODS AIMED AT LOCAL EXCISION

The author’s premise that a true, wide surgical margin (5 mm outside the inflammatory nidus) is necessary to cure osteomyelitis was flawed at conception. Even their “margin gurus” state, in their conclusion, “As a result of this study we have altered our practice and are now more likely to carry out marginal excision procedures, especially in type-A hosts.” Apparently unrecognized by the authors, Simpson et al do not use “wide excision” and “marginal excision” as intended by our tumor colleagues. Instead, they reference surgical margins to the zone of necrosis, irrespective of the lesion’s true, inflammatory margin (and involucrum). In tumor-speak, they clearly advocate an intralesional, marginal excision.

Uniform bleeding (the paprika sign) is the endpoint of a thorough debridement; the secret to a successful limb salvage program is to preserve healthy bone and limit surgical risk in the process. Fortunately, this endpoint is reliably determined visually and is not dependent on frozen section analysis. In their closing comments, Simpson et al realized they could accomplish more by resecting less.

Figure 3D is too small and ambiguous to have been instructional. It is also incorrectly staged: the presence of bright signaling (T2) on both the superficial, medial cortex and the endosteal surface of the posterior cortex suggests a full-thickness, cortical ischemia, thereby upstaging this lesion to either a type III or IV osteomyelitis.
I have some questions for the authors:

What are the indications/contraindications for using the Denver algorithm for the management of osteomyelitis?

Why have the authors chosen the Masquelet technique over bone transport, compression/distraction, or free-fibular grafts?\textsuperscript{4,7-10}

What is their experience, and how does it compare with other methods used in the past?

How does the algorithm accommodate composite-tissue defects and limb-length discrepancies?

Are there contraindications for prolonged external fixation strategies?

Does this mean the authors recommend a wide tumor margin for all types of osteomyelitis? If so, how do they propose we use and apply the debridement guidelines suggested in their Surgical Intervention section?

How do you get a frozen section biopsy in cortical bone?

Because the Masquelet technique works through a polymethylmethacrylate-induced membrane, why doesn’t their algorithm specify the method of local antibiotic delivery?

**SUMMARY**

A true, wide surgical margin is rarely indicated in the treatment of adult osteomyelitis, save for when treating stage IV B associated with an implant (ie, a periprosthetic infection). For all other clinical stages of disease, an intrallesional, marginal excision is associated with an approximately 96% success rate, regardless of the method used to reconstruct the debridement defect.\textsuperscript{4,7-10}

George Cierny III, MD (Deceased)
San Diego, California

**REFERENCES**


**Reply:**

We would have liked to have been able to thank Dr Cierny for his letter to the editor and comments. Dr Cierny dedicated a lifelong career to teaching and improving the diagnosis and management of osteomyelitis, setting standards that are still used across the globe. During his final days, he took the time and made the effort to write his letter to the editor. It contains crucial messages that he has successfully taught to hundreds of disciples. It is with humility, sadness, and prayers for his family and friends that we attempt to respond to his comments and questions.

The goal of our article was to review the available evidence on the management of long bone osteomyelitis. Our article had the weaknesses inherent in a review article, such as the selection bias of our reference list. We attempted to cover the general principles taught by Dr Cierny himself: “Stage I infections limited to the medullary canal may be treated with intramedullary reaming and occasionally localized unroofing and curettage… Stage II infections can be addressed with soft tissue debridement and decortication of the bone adjacent to the infection…. Stage IV infections are the most challenging to treat surgically because they must often require multiple staged procedures and necessitate osseous stabilization."\textsuperscript{1} The algorithm that we presented is by no means a universal algorithm but rather a guide for the management of high-grade osteomyelitis. As pointed out by Dr Cierny, Figure 1 is inaccurate, and the highlights in green were misplaced.

Regarding Dr Cierny’s questions for us:

What are the indications/contraindications for using this method?

Indications for the use of this method would include high-grade osteomyelitis with segmental bone loss following debridement. Contraindications would include low-grade infection or patients unwilling to undergo extensive surgical reconstruction.

Why have the authors chosen the Masquelet technique over bone transport, compression/distraction, or free-fibular grafts?

Our favored methodology to reconstruct bony defect is the Masquelet technique. For osteomyelitis, it has the theoretical advantage of eluting antibiotics locally during the first stage of reconstruction. We are not aware of any Level I evidence to support one technique over the other but agree that other options are perfectly valid.

What is their experience, and how does it compare with other methods used in the past?

We have not prospectively collected data looking specifically into outcomes. Unfortunately, like most centers across the country, the numbers of high-grade osteomyelitis are too low to perform a randomized, controlled trial powered to reveal a difference in outcomes between 2 treatment mo-
dalities. We use the general principles and algorithms described by Dr Cierny himself and have had results comparable with those published in the literature.

How does the algorithm accommodate composite-tissue defects and limb-length discrepancies? For composite tissue defects, the microvascular team intervenes early, ideally at the same time as the first stage of the Masquelet technique to ensure adequate soft tissue coverage. Moderate limb-length discrepancies can also be adjusted during this first stage (up to 25-cm bony defects can be reconstructed using this technique). Our preference is to use a Monotube (Stryker, Kalamazoo, Michigan) external fixator for defects of the diaphysis, allowing compression or distraction.

Does this mean the authors recommend a wide tumor margin for all types of osteomyelitis? If so, how do they propose we use and apply the debridement guidelines suggested in their Surgical Intervention section? We do not suggest wide tumor margins for all types of osteomyelitis. Our algorithm is used in high-grade infections. We agree with Dr Cierny that our algorithm lacks clarity and that the legend should state that it applies to high-grade infections. Our article states, “Stage IV infections are the most challenging to treat surgically because they most often require multiple staged procedures and necessitate osseous stabilization. Staged procedures address the need for eradication of infection followed by osseous reconstruction aiming for union and stability once the tissue is healthy.” Our message is that there is a correlation between the extent of debridement and success rates, as shown by Simpson et al.2

How do you get a frozen section biopsy in cortical bone? Frozen section is taken when we resect segments of bone containing cancellous bone. For low-grade lesions, biopsy is our standard.

Because the Masquelet technique works through a polymethylmethacrylate-induced membrane, why doesn’t their algorithm specify the method of local antibiotic delivery? Through the Masquelet technique, the method of local antibiotic delivery is adding antibiotics to the cement spacer.

Once again, we would like to extend our condolences to Dr Cierny’s family and friends.

Cyril Mauffrey, MD
Denver, Colorado

REFERENCES

doi: 10.3928/01477447-20131219-02