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Letters to the Editor

Letter to the Editor

First, we want to congratulate Dr Elmarsafi and his colleagues on their article (“Concordance Between Bone Pathology and Bone Culture for the Diagnosis of Osteomyelitis in the Presence of Charcot Neuro-Osteoarthropathy. *J Foot Ankle Surg* 2018;57:919–923). We all struggle with getting the diagnosis of osteomyelitis (OM) right in complex patients. Their report adds to that discussion.

We have several observations and questions about the authors’ article. One of their pivotal factors is the presence of Charcot neuroarthropathy (CN), yet there is no definition provided for CN. We cannot find any pathologic definition of CN in the medical literature, and no pathology criteria or references were provided in the text, although it is also not clear that the CN diagnosis was histologic. The histology criteria to define OM also were not specified. We realize that pathology reports for OM and CN are often terse and provide very little description of specific criteria the pathologist used for the histologic assessment, so in a retrospective study, consistent criteria may simply not have been available.

The authors included only subjects with CN and OM. They confirmed the diagnosis of OM by using several criteria, including radiography, advanced imaging, erythrocyte sedimentation rate, C-reactive protein, positive probe to bone, exposed bone, and intraoperative assessment of bone. We realize that some studies use just 1 of these factors to define OM. However, it was unclear how all of these factors were incorporated together to “confirm the diagnosis of OM.” The reference standard for the study to define OM seems to be histology.

When a diagnostic tool is being assessed, subjects with the disease and subjects without the disease are usually included in the analysis. Perhaps by including both, Dr Elmarsafi and his colleagues would have reached a different conclusion. CN may not be the factor that muddied the diagnostic waters. Those waters are already a bit murky, as the authors point out when they referenced publications by Weiner et al (1) and Meyr et al (2). Agreement by pathologists is often poor, and the criteria they use to define osteomyelitis are variable. To determine if CN is a factor, future research should include a broader spectrum of pathology that includes patients with CN with no bone infection, bone infection with no CN, and no CN and no bone infection, as well as more complete operational definitions of disease entities. The report by Weiner et al., (1) which the authors cite, has a very different protocol: histologic and microbiologic assessments were made on specimens suspected, rather than confirmed, of being OM. This changes the underlying prevalence of OM, suspected OM, and confirmed OM, and this in turn changes the sensitivity, specificity, etc.

Further muddying the waters is the fact that clean margins (before and after debridement) were taken from patients with confirmed OM. The supposition appears to be that the clean margins should be diagnosed as OM. The fact that neither the histologic nor microbiologic assessments declared OM in these cases is to be

expected, rather than viewed as a failure. Perhaps these samples need to be more clearly classified and diagnosed. Finally, the fact that the 2 methods do not agree does not indicate, necessarily that one or the other is correct, or that one or the other is faulty. It may simply indicate that both, especially in the setting of CN, must be used to diagnose OM.

We would interpret the results of the study differently than did the authors. The disease process is in the bone. The gold standard still needs to be a direct measure from the bone biopsy. We need to develop systematic criteria to report histology and improve the information given to our pathologists and the reports that pathologists provide surgeons and infectious disease physicians. In the future, we might also be able to improve histology interpretation with computer-aided, pattern-recognition software. We may be able to improve our bone culture results with better intraoperative handling of specimens and by using DNA sequencing rather than traditional culture techniques to identify pathogens. We used 16s rRNA in a series of patients with DFO and identified many more pathogens, especially anaerobes and gram-positive bacilli, compared with traditional culture. Traditional culture techniques may simply not be suitable to identify all of the bacterial pathogens, so some cases would be misclassified as not having OM if culture was the sole criterion.

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References

- Weiner RD, Viselli SJ, Fulkert KA, Aceetta P. Histologic versus microbiology for accuracy in identification of osteomyelitis in the diabetic foot. *J Foot Ankle Surg* 2011;50:197–200.
- Meyr AF, Singh S, Zhang X, Khilko N, Mukherjee A, Sheridan MJ, Khurana JS. Statistical reliability of bone biopsy for the diagnosis of diabetic foot osteomyelitis. *J Foot Ankle Surg* 2011;50:663–667.

Reply to Letter to the Editor

We are equally frustrated with the lack of general consensus regarding the clinical, histologic, serologic, and genomic means to define

