Reply to Letter to the Editor

Treatment of Early Postoperative Infections After THA: A Decision Analysis

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We appreciate the opportunity to reply to the comments of Drs Wolf, Leithner, and Clar. Their comments regarding our study [1] are centered on the concept of applying risk stratification for patients being treated for an infected THA during the early postoperative period. They state that our study does not account for patient-related risk factors in its analysis and that we overlooked two studies [2, 3] that might have contributed insight into this element of the analysis.

As is known, the success or failure of any medical intervention in general, and the treatment of periprosthetic joint infections in particular, are predicated on many factors. The concept of Bayes' theorem and a priori risk are highlighted in this clinical scenario and are mentioned in our article. We believe strongly that risk stratification has an extremely important role in deciding a treatment regimen. What Wolf et al. fail to mention is that these are just concepts. Even for common orthopaedic procedures such as THA, the elements that lead to meaningful risk stratification are not fully understood or implemented, and moreover, in complex clinical scenarios such as with a periprosthetic joint infection, these elements and their application become even more challenging. The two

studies referenced by Wolf et al. [2, 3] serve to highlight the lack of information on this concept in the literature.

The study by McPherson et al. [3] reports on a staging system for patients with chronic infection who have had a resection arthroplasty as the first stage of a two-stage procedure. McPherson et al. report that the staging system they used correlated with the ultimate ability to successfully reimplant a prosthesis. Although this study provides insight into the specific clinical scenario of patients with a resection arthroplasty after a chronic infection awaiting a second-stage reimplantation, this does not in apply to the hypothetical clinical scenario of our patient 3 weeks after THA. In addition, although elements of this staging system might be useful for acute postoperative infections, the study by McPherson et al. [3] cannot be applied to fundamentally different clinical scenarios.

In the study by DeMan et al. [2], no elements of risk stratification are provided, but rather this is a study reporting on the results of a specific algorithmic approach to infection. In addition and similar to the study by McPherson et al., DeMan et al. do not report on the outcomes of acute postoperative infection, but rather a combination of acute and chronic infections they treated. They also report the use of antibiotic cement for fixation of the femoral and acetabular components in approximately half of their cohort. The use of antibiotic cement for the single-stage approaches is mentioned specifically in our study with reported infection control rates. In addition, we discussed the concept of single- and two-stage exchanges being performed using cementless components. The study by De Man et al. does not apply to the clinical scenario presented by an acute postoperative infection.

Although we fundamentally agree with Wolf et al. in their desire to use risk stratification in the analysis of different treatment algorithms, there are no published data that establish these parameters. We agree that prospective studies are warranted to better answer this question.

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