the inevitable resection arthroplasty that is seen in this model (i.e., if a patient undergoes debridement, the years until the 2-stage procedure are years that are not spent with a resection arthroplasty).

Although the authors acknowledge some of these failings in their discussion—and, granted, prosthetic joint infection is a difficult area to model—we think that this model is unrealistic. Most patients present with chronic, hard-to-diagnose late infections in which loosening of the prosthesis may have occurred. Of course retention will appear to be more appealing with this selection bias. Clinicians do not struggle with the decision making involved when these “proposed criteria” are met. Unfortunately, these variables have not been incorporated into the study, which makes certain assumptions and derives conclusions that we fear have little applicability in practice. Each of these decisions is a delicate balance of risk versus benefit to the individual patient, best understood by the experienced clinician, not by a health maintenance organization accountant with an algorithm.

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References


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Criteria for Treatment of Prosthetic Joint Infection

Sir—In a recent article, Fisman et al. [1] addressed the difficulties associated with treatment of prosthetic joint infection. Treatment of such infections leads to increases in the number of hospital admissions and in length of stay, at an estimated cost of $250 million per year in the United States [2]. As a consequence of these high costs and the resulting partial reimbursements, some hospitals reject patients who require such therapy [3]. We are grateful to Fisman and colleagues for approaching this problem in a rigorous epidemiologic fashion. However, we would like to make 2 points with regard to their study.

First, it cannot be overemphasized that, as the authors point out, the results of the study are potentially valid only for the selected population of patients who present with staphylococcal or streptococcal infection of <1 month’s duration that is associated with total hip arthroplasty and who have a well-fixed prosthesis and a good soft-tissue envelope. Unfortunately, we find that most patients in our practices who have an infection associated with total hip arthroplasty do not meet these strict criteria. We urge clinicians not to apply the results of the study by Fisman et al.

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Reply
Sir—We appreciate the opportunity to respond to some of the concerns raised by Saleh et al. [1] and Hanssen et al. [2] in their letters to Clinical Infectious Diseases. We created a mathematical model simulating competing management strategies for infection associated with hip prostheses in older people [3]. Models, including this one, can serve as a means to synthesize available data, help facilitate medical decision making, and identify important areas of uncertainty, which may in turn highlight important research questions [4]. Models are also easily modified, so that emerging data can be incorporated and novel strategies can be evaluated. In modeling, there is always a tension between an overly simplistic representation of reality, which provides little insight into real-world problems, and an overly realistic representation, which may be as chaotic and difficult to understand as the real world itself [5]. Nonetheless, we suggest that our model does provide helpful insights to clinicians dealing with this challenging issue.

We agree with Saleh et al. [1] that rehabilitation with initial debridement and retention cannot be used to treat patients who present with loosened, nonfunctional prostheses, which is why we explicitly stated that our model should only be applied to instances in which loosening of the prosthesis has not occurred, as Hanssen et al. [2] point out. In this context, we identified the best available data for the probability estimates used in our model, including data from studies by coauthors of Saleh and Hanssen [6–11]. However, most data on prosthetic hip infection have been published in the form of case series, which are difficult to interpret because of likely publication bias (i.e., the preferential publication of series that demonstrate a method that appears effective) [12] and the lack of control patients. Even observational studies that present the outcomes of multiple surgical modalities (so that “control” interventions can be identified) are difficult to interpret because of confounding by indication [13]. With reference to prosthetic hip joint infection, such confounding is a result of the more aggressive surgical treatment of people with more robust health status (e.g., younger people, people without comorbid illnesses), who are expected to have a better outcome regardless of the management strategy used.

Because of the limitations of available data, we explored the implications of the resulting uncertainty in a large number of “sensitivity analyses” [14]. This process involves varying the input data (e.g., rate of relapse after debridement) over plausible ranges to examine whether this changes the conclusions. We performed extensive sensitivity analyses on all data inputs used in the model, and a number of the concerns raised by Saleh et al. [1] can be addressed by simply examining the results of these analyses as presented in our article [3]. For example, for reasons outlined in our article, we restricted our analysis to data derived from studies of gram-positive aerobic organisms and estimated the annual rate of relapse after debridement and retention to be 30%. If the risk of relapse were to increase, as might occur with a gram-negative organism, the benefits of initial debridement and retention with regard to life expectancy would progressively decrease and costs would increase, making initial debridement a less attractive strategy. As we noted in our article, annual rates of relapse >60% after initial debridement would result in a decrease in life expectancy relative to initial exchange arthroplasty, because patients would undergo debridement only as a prelude to almost-immediate arthroplasty.

Similarly, we examined the impact of decreasing relapse rates after 2-stage exchange arthroplasty in our article [3]. As we state in our discussion, rates of relapse after exchange arthroplasty as low as 0.6% annually would make initial debridement and retention an unattractive option for 65-year-old people, although it would remain cost-effective for frail 80-year-old individuals. We are certain that Clinical Infectious Diseases readers understand why we did not preferentially use data on infected knee arthroplasty to model prosthetic hip joint infections.

We do agree with Saleh et al. [1] that average health utility values may not represent the health preferences of an individual patient. Individual health preferences may be influenced by a variety of factors, including baseline health status and risk preferences (e.g., risk-averse patients may wish to avoid major surgery even though the potential health benefits are great) [15, 16]. Again, we tested the