Reviewer's report

Title: Secondary purulent infections of the elbow joint: a retrospective, single-center study.

Version: 0 Date: 17 Dec 2019

Reviewer: Baptiste Boukebous

Reviewer's report:

Hello

Thank you for your submission. This is an interesting topic because there is little data available.

The subject therefore deserves to be published, especially since your series is important. However, I think it is necessary to apply major revisions in order to clarify the method and results.

1) The first question is the definition of the elbow: which interventions have you taken into account? (elbow prosthesis, supra-condyle fractures, olecranon fracture...)

2) There is a major lack in your publication: the analysis of the implanted material. Indeed, there is a difference between an infection after a double humeral plate for supra-condylial joint fracture, screwing of a diacondylar fracture, broaching....

All this is extremely important because we know that the material generates the development of a biofilm that precludes the action of antibiotics. I did not find in the results or in the method any paragraph concerning the delay of occurrence of infections since the first intervention. An early infection, with a simple wash, is not the same as a later infection.

Similarly, we know that fractures still consolidate in a large number of cases, despite a treated infection. There are sometimes several interventions for successive washings before a "final" intervention, aimed at removing the material when the fracture is solid and to finally cure the infection.

It is therefore necessary to completely review the description of patients and integrate the notion of delays +/- consolidation or pseudoarthrosis.

3) in methods, there is a contradiction: line 83/84: "Patients suffering from immunodeficiency (acquired or due to an underlying disease such as rheumatoid arthritis or HIV) were excluded" whereas line 112 and 113 states: "In patients with an immunodeficiency, we consider a broader antibiotic treatment aiming against methicillin-resistant Staphylococcus aureus (MRSA) or gram-negative bacteria".

Did you or did you not exclude immonocompromised patients?
if so, for what reason?

if so, can you give us a flow chart?

4) the analysis is not clear. It is not clear what the two subgroups "seropositive" and "seronegative" mean. Your Mann-Whitney test is also strange and has no clinical interest in my opinion; what is the point of knowing if the age is different according to the seropositive dans seronegative groups? Not to much in practice.

On the other hand, the number of interventions/ procedures (meaning the number of interventions before healing) IS THE parameter to be analyzed because it is the one that has a real clinical impact.

5) practical questions: have you used the Masquelet technique?

Have you immobilized the patients? if so, with extended casts?

6) on the form, I don't personally like « (ie ...) ». Because often when you used it, there were some informations that seemed to me necessary to detail.

To conclude, I think that the description needs to be revised, including the delays and the type of material. Afterwards, all of this must be integrated into the analysis, taking as the main evaluation criterion the number of reoperations (or the number of days necessary for complete and lasting recovery), and not the notion of seropositive/ seronegative.

You have to use a multiple linear model for example like this:

\[
\text{number of procedures} = \text{coefficient1} \times \text{age} + \text{coefficient2} \times \text{sex} + \text{coefficient3} \times \text{immunosuppression} + \text{coefficient4} \times \text{time of infection} + \text{coefficient5} \times \text{type of material used} + \text{coefficient6} \times \text{type of initial pathology} + \text{coefficient8} \times \text{type of germs found} + \text{coefficient9} \times \text{seropositive or seronegative} + \ldots + \ldots
\]

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

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I recommend additional statistical review

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