Reviewer’s report

Title: Can hybrid hyaluronic acid represent a valid approach to treat rizoarthrosis? A retrospective comparative study

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Reviewer: Thomas Hügle

Reviewer’s report:

This is a concise and well written manuscript investigating the use of hybrid hyaluronic acid (HA) vs. steroids in TMJ OA. Despite HA has been studied in hand OA in the past, the authors claim that this is the first survey analysing the effects of a new hybrid HA formulation.

In the abstract, the authors conclude that this new hybrid formulation of HA is superior to triamcinolone in terms of pain relief and joint function improvement. They further describe a rapid and persistent effect of HA.

However, the study design is retrospective, not controlled and notably without placebo arm. End points are VAS and functional scores. There is no radiographic assessment.

I'm afraid that based on this purely observational and retrospective study, the statement that HA is superior to triamcinolone should be made more carefully.

There further limitations of this study: 55 patients in the HA and 45 in the triamcinolone arm seem to be underpowered. There is no power analysis in the methods section. Kellgren & Lawrence grade II and III as an inclusion criterion is very heterogenic so the groups might differ in OA severity. To this end, cholesterol levels (which are associated with hand OA) are significantly higher in the HA arm which also point out a potential difference of the groups.

Another limitation is the lack of ultrasound-guided infiltration. Joint infiltration by palpation as performed in this study has significant rates of extra-articular drug application which might bias the study.

As a minor point, 6 smokers are found in the Triamcinolone group whereas there is only 1 smoker in the HA group. Smoking is a potential negative confounder of treatment in OA. This should be discussed. Also, the SF36-mental component summary is significantly lower in the triamcinolone group.

Taken together, this relatively small and uncontrolled retrospective survey potentially is biased by a heterogenic patient cohort. The conclusion made by the authors in their abstract is completely supported by the data and should be drawn more carefully.

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?**
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No

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I am able to assess the statistics

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