Review of '*What determines the predictability of a Mediterranean cyclone?*' by Benjamin Doiteau, Florian Pantillon, Matthieu Plu, Laurent Descamps, and Thomas Rieutord

In the revised version of their manuscript, the authors have made great efforts to take into account the general comments made in the first review. They rephrased the title to more accurately reflect the specific aspects addressed in their predictability study. As for the comments on storm nature, they emphasize that their focus is on the extratropical storms and argue that medicanes would only contribute with a negligible fraction. I would like to clarify that my comment was rather meant not to simply distinguish into extratropical storms and medicanes, but to analyse a bit more how the full spectrum looks like, i.e., including the hybrid stages. It would have been interesting to also condition on the storm nature, because regional differences may be related to it. Even though the authors have refrained from taking up this aspect, I think that at least the clarification made throughout the paper that the majority of the storms are 'extratropical' is helpful for the reader.

The main methodological concern, raised by both reviewers, namely the inconsistency between tracking algorithms, has been seriously addressed. The authors answered many questions related to it and clarified it in the manuscript as well. An extra analysis has been done, applying the VDG algorithm to the ERA5 dataset, resulting in the additional section 2.5, which presents the results of the comparison of the two tracking techniques.

Since the revision has improved the quality of the paper in a good way, and the authors have replied to the review comments in a satisfactory manner, I now recommend the paper for publication after consideration of the minor specific comments below.

For future revisions, when you prepare a response to a reviewer, please give line number(s) of the text you cite from your revised manuscript in the response to reviewers. Otherwise, one has do the search for the position in the revised document oneself. Thanks!

Specific comments

l. 104: Add "(125-km horizontal resolution)" after "model data".

l. 143: Please add again the word "reforecasts" after "ensemble".

l. 163: Please check my comment to line 158 in the previous version of your manuscript. In the tracked changes document, I can see that you (probably accidentally) removed the sentence you indicated as corrected in your reply to my comments.

l. 188: Replace "non-identical" by "different".

l. 189-190: To make the reader aware of it, please add a sentence on the fact that your comparison on the ERA5 dataset between VDG and AYRAULT has a limitation in terms of storm occurrence. VDG can only identify occurrence if it was found by AYRAULT before.

l. 312-313: I'm not convinced by the statements you make in response to my comment (l. 296-297 in the original version of the manuscript). I don't see why it would be "very unlikely" that extratropical cyclones enter mutliple of the regions you defined throughout their lifetime. If they are embedded in Rossby waves, they can easily get steered over long distances. And, again, I recommend not using the phrase "suggesting two different processes of cyclogenesis" as you are not distinguishing between different stages of a cyclone's lifetime in your study. Keep it on the "occurrence"-level, instead of speculating about stages!

l. 327: Replace "increases of" by "increases by".

l. 329-332: I know what you want to say, but I still find it hard to read and digest this sentence. Consider checking and simplyfing it further to help the reader. To me, the explanations in brackets (i.e., …) are more confusing then helping as they address a totally different question (namely track divergence, while the main text discusses cyclone numbers changing with lead times).

l. 368: On your response to my comment on l. 349 in the previous version of the manuscript: The word "calibration" is often degraded to a statistical post-processing method (i.e., a methodolocial term), but it is actually much more than that. In statistics, it means a joint property of the predictions and the events that materialize, and is thus equivalent to the word "reliable".

l. 452: Given that CDFEs yields absolute values, using 'over-predicted' seems not appropriate then. Please reword.