We are pleased with the generally positive reviewer remarks and thank the reviewer for the invested time and the very helpful comments provided, which will help us to improve the manuscript. A pointwise reply to the reviewer's comment is given below.

Specific Comments:

1.) *Table 1 shows that after the calibration procedure, the weight w3 (related to the jet latitude goes from 0.92 to 0.95. If I am correct, this implies that the cost function essentially accounts for the local latitude, while the terms related to wind speed and direction are almost negligible. Why it is necessary to retain the terms X and Y in the cost function with such small weights? Would the resulting jet path be different if those terms are simply not considered?*

It is correct that w3 (latitudinal steering parameter) is important but it is still necessary to retain the terms X and Y. With w3=1 (and without term X, which accounts for the strength of the wind field the jet stream core) would give just a straight line at ϕ_{clim}, since this would be the minimal cost. Without the term Y the jet stream curve would be not smooth and locally spiky. For those reasons, we never tried to consider only term Z, but we did even test lower weights for X and Y, which showed us this behaviour.

2.) *Along the text, the authors describe some "constrains" they needed to close their algorithm without clear justification. For example (see page 4, line 5) they limit PFJs to be between 30◦N-90◦N latitudes and state that this is "something which does not affect the results". How sensitive is the method to changes in the 30◦N threshold? Has this limit been explicitly tested? I know that is not frequent at all, but It seems to me that locally, the polar front jet (or some of its branches) could be occasionally close to the 30◦N limit. Other example appears in page 6, line 9. The authors establish that they set the weight w2 "manually". How was this done? (Need clarification). Same for page 7, line 12 (and table 1).*

We used this latitude constraint only to speed up the code, but this is easily be changed. In fact we rerun our analyses using the full latitudinal range and get the same results with and without this constraint. For that reason, we will rewrite this part as we use the boundaries *0◦N-90◦N latitudes*.
Regarding w2: We set manually different values for w2 and looked at different plots to decide, which values gives the best results. Again this parameter is not important for the circumglobal path of the detected jet core but only for local smoothing.

We will rephrase this part in the manuscript to make it clearer.

3.) *In view of the examples shown in figures 5 and 6 and in the climatology (figures 11 to 14) it seems clear that the algorithm is doing quite a good work locating both jets. In this point I really miss a comparison with other similar schemes like those of Archer and Caldeira. (2008), Pena-Ortiz et al (2013) or Rikus (2015). In particular it would be very interesting a comparison related to the averages and trends of the jets (strength, mean latitude or even prevalent wave-number). Such an addition would largely improve the scientific value of this work. (I know that the new climatology represent 15-day periods, but anyway, for long term trends and averages, the new method should provide results comparable to those obtained with daily approaches)*

We agree with the author and will add an additional discussion section in the manuscript, where we compare strength and mean-latitudinal trends of our analysis, compare them with the literature and will provide a table of the trend analysis.
We already did such analyse and could conclude that the results are very consistent with the existing literature mentioned by reviewer.

Formal issues

We agree with the referee and rephrase as suggested.