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Earth System Dynamics
Discussions

Interactive comment on "A network-based detection scheme of the jet stream core" *by* Sonja Molnos et al.

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First, I would like to emphasize that I really like the methodology developed by Molnos et al. to characterize the jet stream. I think that the authors have developed an interesting new method able to cope with the extraordinarily complex structure of these systems. The presented results suggests that the method is able to "link" maxima in the hemispherical wind field in a way consistent with the jet stream structure, and in this sense, I think that this manuscript can be considered for publication in Earth System Dynamics. However, I have some concerns I detail in the next paragraphs:

Scientific issues:

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?

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).

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).

Formal issues:

1. When the detection scheme is developed (sections 3 and 4), the authors first show the results of their first attempt (section 3), only to conclude that it did not worked (see Figure 2). Then, they start a new section (section 4), which is devoted to explain how the authors calibrated their first scheme in order to properly separate polar and

subtropical jets. Moreover, section 4 is not completely clear. In occasions the text goes "back and forth", anticipating concepts that are only developed in a following section (see for example lines 5 or 26 in page 6). Of course I know that this comment reflects mostly my personal taste, but in my opinion, section 3 and 4 could be rewritten and merged, avoiding the mention of the first not-working attempt (and thus Figure 2) and describing at once, and in a "linear" and concise way, the final working scheme.

2. Equation 1 is not consistent with the text (page 3, line 13).

3. The explanation of the Rikus' algorithm (section 4.1) is not clear. In particular the text between lines 12 and 18 could be rewritten in order to better explain the basis of this algorithm and the concepts involved (as for example how are defined the maximum (minimum) filters and the maximum (minimum) stencils).

4. Figures 11 to 14 (seasonal climatology) are very interesting because they give simple and very visual information about the new jet climatology. On the other hand, Figure 10 (annual climatology) is a little bit redundant.

5. I do not see the point in considering a single figure as a supplementary material (Figure S1). If the authors think this figure is necessary, they should include it in the main text and add some more discussion. If not, it would be better to remove it.

6. Figure 7: The caption should indicate only what is displayed by the figure, leaving the discussion for the main text.

7. Finally, the text has a number of errata (capitals, latitudes of 140°N, typos in equations, missing years in references, etc.). Please! Do a careful revision prior to publication.

Despite these comments, I would like to highlight that I consider the method quite interesting, and providing the authors perform some rewriting of the text, justify some of the thresholds they used and add some comparison with similar works, this paper is a valuable addition to the scientific literature related to the jet stream.

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