

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

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General Comments:

This paper addresses scientific questions relevant to ESD because it introduces a new methodology for identification of jet streams and then applies it reanalysis data to provide a useful probability analysis of PFJ/STJ core positions. Although numerous methods to characterise the properties of jet streams exist this represents a useful contribution to the literature by introducing a novel method. One aspect of the jet literature is that although general definitions do exist there are a number of different operational definitions essentially determined by the methods used to characterise the jets, in this case local maximum wind speeds in a mass weighted layer. I thought the context of the method was a little underdone (particularly in the abstract) and the paper would

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benefit from a little more discussion on other similar work even if only to highlight the advantages of this approach. Overall the method is well developed and reasonably well described but there are a few areas where the language is confusing (see comments and corrections below). I think a minimalist change to the title would probably be "A network-based detection scheme for the jet stream core". While I would suggest a couple of changes (see below) the abstract reflects the content of the paper. The overall structure of the paper is quite good although there are a couple of minor problems, e.g. Equation 6 is discussed on page 6 before it is actually given on page 7, which could be addressed. I would recommend publication with some changes.

#### Specific Comments:

1. I'm not so sure that algorithms to detect jet cores are lacking (as stated in the abstract). There are actually a relatively large number of previously published papers which are based on either a single level/layer or zonal/sectorial mean latitude-pressure fields. The current application of the network-based method is yet another variation of this and so needs to be put in context with other (similar) methods which use single level or mean-layer wind fields (e.g. Koch et al, 2006, Archer & Caldiera 2008, Pena-Ortiz et al 2013). Hence at the risk of expanding the paper too much I think it would be useful to acknowledge some more of the previous work and to compare with the results obtained here (even though they are based on a 15 day mean as opposed to monthly means) and discuss why this method has advantages over the previous studies.
2. I think 'time step' is a confusing choice of phrase to describe the 15 day means – maybe use 'time period' ?
3. The simulated annealing actually uses the Rikus algorithm so it is being used as more than just a comparison. (page 1, line 18) and the abstract description should reflect that.
4. Were the original runs (Fig. 2) done with with the un-optimised weights from table 1? If not what was used?

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5. The supplementary plot (S1) is only mentioned in a single sentence without sufficient context to make it worth while. Either add more discussion or remove it?

Technical Corrections:

Abstract Line 21: ‘mean longitudes of 20S and 140N’ ?????? I don’t know what this is supposed to mean!

Page 2, line 27: No year given for Gallego et al (and in reference list). Try 2005.

Page 2, line 32: there is no reference for Limbach et al (2012).

Page 3, line 2: “zonally” should be “zonal”

Page 3, line 4: “such approach” should be “such an approach”

Page 3, line 13: “all different” should be “different”

Page 3, line 18: “for 4” should be “four”

Page 4, lines 1,2: I’m not sure what this line actually means!

Page 4, lines 3,4: “To avoid noise and reduce computational costs only those grid points where the wind velocity is greater than 10% of the maximum wind velocity for the considered time step are connected.”

Page 4, lines 12-13: The description of the weights is inconsistent – is their sum 1 or less than 1?

Page 5, equation 4: there appear to be some brackets missing in the denominator.

Page 5, line 19: “near of 65” should be “near to 65”

Page 6, line 15: “First a maximum (minimum) filter” should be “First a local maximum (minimum) filter”

Page 7, line 5: “With the found zonal mean subtropical and polar jet stream latitudes by Rikus” should be “With the zonal mean subtropical and polar jet stream latitudes found

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by Rikus' algorithm”

Page 7, lines 7-8: This sentence needs to be clarified.

Page 7, line 21: “it is more undulated” should be “it undulates more”

Page 7, lines 22-23: Try - “Improvements in the jet stream core positions due to the optimization process relative to the positions found by the untuned ...”

Page 8, line 7: “polar Jet” should be “polar jet”

Page 8, line 16: “not between minimum and maximum latitude” should be “not between the minimum and maximum latitude”

Page 8, line 17: “equivalent of 6.4” should be “equivalent to 6.4”

Page 8, line 20: Change to “These differences are due to the undulations explained above.” ?

Page 9, lines 16-34: The language needs to be cleaned up – the section does not scan well with a number of missing ‘the’ and ‘a’s.

Fig. 4 caption (page 15, line 14): the points in (f) are blue not white.

Fig. 5 caption (page 16, line 4): should be “compare with Fig. 2”

Fig. 6 caption (page 16, line 7): Remove “, compare Fig. 2.” - it is not necessary.

References:

Gallego D, Ribera P, Garcia-Herrera R, et al. (2005) A new look for the Southern Hemisphere jet stream. *Clim Dyn* 24:607–621. doi: 10.1007/s00382-005-0006-7

Koch P, Wernli H, Davies HC (2006) An event-based jet-stream climatology and typology. *Int J Climatol* 26:283–301. doi: 10.1002/joc.1255

Limbach S, Schömer E, Wernli H (2012) Detection, tracking and event localization of jet stream features in 4-D atmospheric data. *Geosci Model Dev* 5:457–470. doi:

10.5194/gmd-5-457-2012

Pena-Ortiz C, Gallego D, Ribera P, et al. (2013) Observed trends in the global jet stream characteristics during the second half of the 20th century. *J Geophys Res Atmos* 118:2702–2713. doi: 10.1002/jgrd.50305

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