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Comment

***Interactive comment on* “Jet characterization in the upper troposphere/lower stratosphere (UTLS): applications to climatology and transport studies” by G. L. Manney et al.**

Anonymous Referee #1

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Review of the manuscript “Jet characterisation in the upper troposphere/lower stratosphere (UTLS): Applications to climatology and transport studies” by G. L. Manney and co-authors

General comments

This manuscript proposes a procedure for building climatologies of jets and for mapping fields (e.g. from satellite data) with respect to the jet location in the horizontal and the vertical. It shows how it can be applied to a sample of data. Overall, the research and methodology presented in this manuscript is sound and very relevant to the current research in the field of UTLS. Although the manuscript reads well and the methodology

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is carefully described, the text insists too much on promoting the advantages of the framework proposed, while discussions on its limitations are almost absent. Other methods are mentioned without proper account of their strengths and limitations. To my opinion, the manuscript's text needs to be re-balanced, including proper discussions of the limitations of the proposed method and the strengths and limitations of the existing methods it is compared to (see specific comments). Apart from this balancing issue, the manuscript represents an exciting, original work and is clearly relevant to the field.

Specific comments

1. The jet catalogue excludes longitudes of weak jets. This exclusion is fine for the mapping of jets because its implications may be anticipated. However, when used to map satellite data, this exclusion leads a biased representation of the data when a correlation exists between the longitude of the strength of the jet and the satellite data. If such a correlation is absent, then it leads to a sub-sampling of the data, which arbitrarily reduces the statistical significance of the data. The authors argue that this framework gives an advantage because it allows to focus exclusively on regions of special interest. This argument hypothesizes that longitudes with strong jets are of higher interest than longitudes with weak jets. To my opinion, we do not have the knowledge today to ascertain this. Showing that this is indeed the case, would be a result in itself. In order to show that this is the case, the authors would need to provide a mapping of the satellite data in longitudes of weak jets as well. I suggest that the authors provide the maps of data in longitudes of weak jets as well, if possible. If any, I strongly recommend to add to Fig. 7 the quantities mapped in the EqL framework in the longitudes of strong winds only, with a corresponding discussion. This could give a hint on the implications of the selection made in the jet-relative framework. In any case, the manuscript needs a discussion on this issue, not only in the conclusion area, but also in the introduction.

2. The methodology uses threshold values for the minimum jet strength and for the latitude distinguishing sub-tropical and polar jets. In studies which span a century,

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the jet stream may increase and shift in latitudes. These changes may affect the way satellite data are mapped using this jet catalogue. This issue needs to be addressed somewhere in the manuscript, such as in the conclusion section.

3. The mapping of fields with respect to the jet offers an interesting perspective, since jets are known for their capacity to organise fields such as passive tracers in particular. However, away from the jet, passive tracers will become less organised by the jet and be subject to other dynamics. Hence, the mapping of a tracer with respect to the jet is expected to provide a sharp view close to the jet and to become blurred away from the jet. This needs to be addressed in the manuscript.

4. Fields mapped with respect to the jet location, using horizontal distances as x-axes, are shown from -60 to 30 (SH) or -30 to 60 (NH), spanning 90 degrees latitude (Figs 5, 6, 11, 12). It is clear that the number of data available at these latitudes decreases towards the pole because of the variation of the jet location. This also means that statistical significance of averages decreases towards the poles. The knowledge of statistical significance (or at least of the number of available data in each grid box) is essential to the interpretation of the averages presented in this manuscript. This aspect is omitted in the current manuscript and needs to be included. I strongly recommend to either overlay statistical significance on the appropriate figures, or to add graphs showing the number of available data.

5. The manuscript does not provide a balanced account of existing methods for mapping fields (EqL, PV, TH,...). It tries to convince the reader that the proposed methodology is better, but fails to discuss within what limits it is better, and for what type of focus. The manuscript seems to promote the proposed method as a generally better way of mapping fields. The above items suggest several limitations of this method that need to be discussed in the manuscript for a more balanced account of the strengths and limitations of the method itself. In addition, the current manuscript needs to be improved (at least in the introduction) to provide a more balanced account of other methods. In particular, the EqL framework is criticized because it fails at showing the strong peak

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in the PV gradient around the jet. This is inherent to the EqL framework, of course, and is not surprising. However, the EqL provides an interesting theoretical framework for understanding non-conservative transport, since PV has conservative properties. In my opinion, the framework proposed here shows a greater interest when compared with the EqL framework, such as in Fig. 5.

6. In Fig. 7, center panel, the standard deviation of Hor PV Gradient in EqL is higher than the one in the jet-relative coordinate system. I am surprised. I wonder whether this is not just an artefact due to the number of data used in these standard deviations. Indeed, since the EqL loses the longitudinal dimension, the number of data used to calculate the standard deviation must be simply the number of times. On the other hand, the numbers of values used in the standard deviations using the jet-coordinate include the longitudes and the time, ie a much larger number of values. Comparing standard deviations with such a difference in the number of samples is misleading in this context.

7. Fig. 8, top panel: Is this the correct figure? The x-axis says “windspeed difference from max”, and the axis ranges from -60 to +60. How can you have winds 60m/s larger than the max wind?

8. It is argued through the paper that effort is made to automate the search for the jet center(s) using what the human eye would see as a reference. Why would the human eye be such a good reference?

Technical corrections

- I. 41: remove first bracket
- I. 206: decrease between them: do you mean in straight line? clarify.
- I. 281: remove last bracket
- I. 379: last “2005” should be “2009”

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I. 406: define EqL

I. 412-413: Undulating flows, ie with conservative waves, should be well described with conservative quantities, such as PV or TH. Complex jets may make the jet-relative framework less relevant. Clarify.

I. 456-457: This suggests that the jet-relative coordinate system is less relevant far away from the jet.

I. 469: Is this really an advantage?

I. 524: weakly positive equatorward, not poleward

I. 534: why would transport be more interesting near strong jet cores than elsewhere?

I. 545: with a plot

I. 562: The strongest

I. 565: I do not understand this statement.

I. 636: latitudinal

I. 657-658: I am surprised that MLS can see below the tropopause

I. 763: “undiluted”: what do you mean?

Fig. 9: Is this figure really useful?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1835, 2011.

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