

Interactive comment on “How well do stratospheric reanalyses reproduce high-resolution satellite temperature measurements?” by Corwin J. Wright and Neil P. Hindley

Corwin J. Wright and Neil P. Hindley

c.wright@bath.ac.uk

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1. *On page 1, line 5, COSMIC is described as an instrument. It is not. See page 3, lines 6 and 7.*

We've rephrased the sentence to avoid this implication.

2. *On page 1, line 7, I suggest “use cases” be deleted, in favor of the word “usage.”*

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Done.

3. *On page 1, reference is made several times to “full input reanalyses” without definition.*

This has been clarified by adding “(those which assimilate the full suite of observations, i.e. excluding JRA-55C)” after the first mention.

4. *On page 3, on line 2 the acronym COSMIC is defined incorrectly. This is done correctly lower down on line 28.*

Fixed.

5. *On page 3, line 20, “they” should be “this.”*

Fixed.

6. *Page 4, line 1. Aren't stratospheric temperatures always “dry?”*

True! We've removed the word 'dry'.

7. *I found the description of figure (4b) to be confusing. Perhaps, the authors might revise the text in this regard.*

The description has been rephrased to be clearer.

8. *I think the paper would benefit from a more “broad-brush” description of the methodology in the main text with the details being in either an appendix or a supplement. While it is important to describe their procedures, I tended to get bogged down in the*

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detailed procedure descriptions. This detailed description should not detract from the paper's important conclusions.

We agree with this comment, which both reviewers made in some form. The current ordering arose due to the evolution of the paper: as originally planned it did not include the material after section 7 and as such was more focused on the method. We have now moved the technical details of the OIF, MIF and Core to a new Appendix, and replaced this section with a brief overview of the three components.

9. *Page 23, lines 11-15. I find this statement confusing. The authors are attributing summer pole problems to the cold-pole problem. Isn't this a winter pole problem? Aren't the authors referring to the need for more gravity wave drag around 60S? Certainly, this was the concern of the cited papers.*

You're absolutely correct - the sentence has been removed.

10. *Would the authors say a few words describing the stand-alone black dots in figures 16 and 17?*

The captions of Figures 16 and 17 have been significantly extended for clarity - including explaining the dots!

11. *Page 17, line 26, satellite should be singular.*

We believe this refers to Page 26 Line 26, which we have fixed.

12. *Page 29, lines 12-14, is this due to the large vertical extent of the AIRS weighting function, its fine horizontal resolution, or both?*

This is an important question, and one that should have been considered at time of

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writing in more depth. We have now assessed this in detail, as described in our response to the other comment on this article by Simon Chabrillat.

Specifically, we find that while a 1D (i.e. considering the vertical form only) approach does achieve most of the gains of the 3D approach, more than 50% of samples retain an above-instrument-precision difference, of up to 5 K. This has been added to the text both in the main body and as an additional section of the relevant appendix.

13. *Page 29, line 20. The zonal mean results are very interesting. I think it might also be interesting to compare results for various wavenumbers since planetary wave diagnostics are quite common in stratospheric research.*

We also agree this would be interesting. We are currently in the data-analysis stage of a follow-up paper focusing on gravity waves as-resolved by these reanalyses, and now plan to include planetary wave diagnostics in response to this comment.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-515>, 2018.

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