Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-515-RC2, 2018
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Atmospheric Chemistry and Physics Discussions
Open Access

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

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Received and published: 20 August 2018

1 General comments

This intercomparison of stratospheric temperatures in reanalyses is broad, detailed and innovative thanks to an in-depth comparison with four different satellite instruments and an interesting interpretation using the cluster analysis technique (section 10). The manuscript is very well written and the figures are excellent as long as they are seen

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on a large screen: many require improvements to be readable on a print-out.

I found that Section 4 is too long and technical. It could be summarized, moving some content to the annexes (e.g. Figure 5 and most of section 4.2). The really significant information in section 4.2 is about the specification of the sensing volumes of the instruments but this is not sufficiently detailed (see specific comment below).

In my opinion this study has only one weakness: the vertical and horizontal dimensions should be separated in the comparison between full sampling and single-point sampling of AIRS observations (section 5 and appendix B). It is found that the added cost of full sampling is not justified when comparing reanalysis temperatures with HIRDLS data and seldom justified when comparing them with COSMIC and SABER data. This is an interesting (and comforting) outcome. But considering the viewing geometry of AIRS, the finding that it requires full sampling is quite trivial in the vertical dimension. For example in the case of constituent measurements, any comparison with nadir-looking instruments requires preliminary convolution of the model output by the vertical averaging kernels of the observations. Hence we are left wondering if full-sampling of AIRS in the horizontal dimension has any impact on the comparison. Of course there is no point in re-processing the whole dataset, but this question deserves either some discussion or (better) a sensitivity test. Here I would recommend to pick one year from one of the higher-resolution reanalyses and compare its fully (3D) AIRS-sampled dataset with another one where a more "usual" sampling is applied, i.e. 1D sampling in the vertical and bilinear interpolation in the horizontal.

2 Specific comments and minor corrections

- The abstract should explain in a few words the "full-3D sampling approach" to contrast it with the single point approximation.
- P.2, lines 1-4: You state that the biases between the reanalysis and observed

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states are due not only to the multiplicity of assimilated datasets but also to the need to favour the model state for reasons of numerical stability and dynamical balance. This is a very interesting insight but it requires supporting references.

- P.2, line 29: please give a few words about the different diagnostics used in sections 6–9.
- P.3, line 20: "they preserve"
- P.3, line 21: re-phrase the sentence, e.g. "... while they are suppressed by the methods used to optimise the standard AIRS Level 2 product..."
- P.3, lines 25-26: it makes no sense to describe values derived from perturbations to synthetic data as "measurements" - please use better wording.
- P.4, line 7: Please provide specific references for the across-line-of-sight and along-line-of-sight resolutions of AIRS.
- P.4, lines 29–31 and Figures 1–2: these are very helpful and informative figures but they require some details about the methods used to approximate the sensitivity of the instruments. Note also similar question below (p.10, lines 28–32).
- Figure 1d: it is not possible to distinguish between solid and dotted lines (except looking on a screen with very high zooming)
- P.5, lines 13–14: "Each of them is widely used in the scientific community for a variety of purposes" - not yet for ERA-5 which was released very recently.
- P.5 lines 15 and 16: please define "upper-atmospheric data" in this context. Consider using the word "upper" between quotes.
- P.5 lines 20-21: it is easy to be more specific. Consider: "COSMIC is assimilated by all reanalyses except for JRA-55 and JRA-55C, AIRS by most..."

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- P.5 lines 21-22: The words "Beyond these details" and "extremely" are not necessary, and Fujiwara et al. (2017) is an introductory paper - not a special issue. Consider: "The S-RIP introductory paper (Fujiwara et al., 2017) provides a detailed summary of the key features of each reanalysis".

- P.5 line 26: I think that there is a description of ERA-5 either in the ECMWF newsletter or (better) in a dedicated ECMWF technical report. Please check.

- P.10, line 24: I am not an export in satellite viewing geometries, but this really puzzles me: when vertical viewing angles are defined from instrument nadir, limb-scanning instruments should be defined as 90° - not zero !?

- P.10, lines 28–32: sensing volume parameters are an important input for this study, yet no sufficient details are given about this. Are the standard deviations in each dimension a constant for each instrument? If so, this should be written in a table. If not, what do these standard deviations depend upon? Latitude, longitude, date? Or do they differ for each observation depending on its context (e.g. surface albedo)? Please provide appropriate references for each instrument. Note also similar question above (p.4, lines 29–31).

- P. 12, line 23: please take this opportunity to define the SPA acronym (and capitalize the first letters).

- P.13, lines 10–11 and also p.32 lines 4–5: see general comment above – is this difference between SPA and full sampling due only to the vertical distribution of sensitivities or also to their horizontal distribution?

- P.13, line 12: footnote is not necessary

- P.13, line 17: delete extraneous words "and Appendix A)"

- P.13, line 18: "for COSMIC and SABER data, in particular..."

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- Figure 6: it is not possible to distinguish between solid and dotted lines (except looking on a screen with very high zooming).
- Figure 6 and 7: Despite the explanation in the caption of Figure 6, the last line of text annotation (e.g. "CO=0.99C+3" or "SA=1.01E+-2") is unclear (especially for ERA-I and ERA-5 where the "E" looks like scientific notation). It would be simpler to directly write the values of gradient and the intercept separated by a comma, e.g. "(0.99,3)" or "(1.01,-2)".
- P. 14, line 6: while discussing figure 6, please remind the reader that ERA-5 is not compared with HIRDLS because you study only the post-2010 subset of ERA-5 while HIRDLS ended in 2008.
- P.15, lines 3–7: this is easy and interesting to check: are most outlying COSMIC profiles located close to the poles?
- P.18, lines 5–9: this attempt to qualitatively discuss SSW interannual variability is inadequate. Since this topic is largely out of scope, it should be sufficient to simply list the largest SSWs while dropping lines 6–8: "The SSWs of January 2006, January 2009, March 2010 and January 2012 (Butler et al., 2017) are clearly visible at both altitudes, and all datasets show a near-identical response at the 30km level. However... "
- P.19, end of section 7: Figure 10 is not discussed at all. This should be done (e.g. there is a large spread in annual cycles at 50 km) or else this figure should be dropped.
- P.19, line 14: "...acts as a 'true' estimate which the *reanalyses* are attempting to approximate."
- P.20, line 19: It may be worth mentioning that this "ability of the reanalyses to reproduce the observational record" is relatively low at 70km.

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- Figures 12–15: quite difficult to visualize (especially on paper) due to the monochrome colormaps.

- Captions of Figures 12–13: re-phrase "...indicate boundary full and partial SABER coverage".

- P.23, line 14: "... resolving..."

- P.23, end of 23: on Figure 15 one also notes significantly larger RMSD in the winter polar latitudes. This should be highlighted and may be shortly discussed.

- Title of sub-section 10.2: this should not be identical to the title of section 10. For 10.2 I suggest "Co-located Cluster Analysis".

- P.29 line 19 This is still part of section 10. Replace "Section 10 suggests..." by "The previous section suggests..."

- P.26 line 31: remove extraneous ")"

- P.28 caption of Figure 18: this is an unusual graph in our field and it plays an important role in the paper, so it is important to provide a clear and complete caption (i.e. "see text for details" is insufficient). Please repeat that the co-located measurement pairs are all at 30km, horizontal distance does not imply any information, and the ordering is chosen purely to produce a simple tree.

- P.29 line 13: "... always required when comparing to AIRS,...": this may be true only w.r.t. the vertical dimension whcih would be trivial (see above)

- P.29 line 14: "... and required in equatorial regions and regions of high gravity wave activity ..." . This is only a conjecture i.e. it has not been demonstrated in this study. I suggest to tone down the conclusion: "... and may be required in ..."

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- P.29 lines 27–28: as I understand them your results are not about the variability between pairs of datasets but rather about the agreement between these pairs. If this is correct, consider replacing "...variability...significantly less..." with "...agreement... significantly better..."
- P.31 lines 25–27: this is an interesting point. It should be mentioned in the body of the paper.

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