

Interactive comment on “Representation of the Equatorial Stratopause Semiannual Oscillation in Global Atmospheric Reanalyses” by Yoshio Kawatani et al.

Martin Dameris (Editor)

martin.dameris@dlr.de

Received and published: 29 May 2020

Dear authors,

sorry for the delay!! One referee gave already a full review to your paper in the round of the quick-reviews (technical review); here again are the statements of the reviewer. The referee rated the manuscript in all categories with "excellent". The reviewer ask for minor revisions. Please consider the points in your revised manuscript.

Here is the referees report to your manuscript:

“Representation of the Equatorial Stratopause Semiannual Oscillation in Global Atmo-

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spheric Reanalyses” by Yoshio Kawatani, Toshihiko Hirooka, Kevin Hamilton, Anne K. Smith, Masatomo Fujiwara

This paper represents the state of the art in depicting the equatorial stratopause (and mesopause) semiannual oscillation in global reanalyses. Its focus is not on dynamical cause or dynamical diagnosis, but on differences among the reanalyses, in an effort to increase our understanding of “what is the best description of the lower mesosphere and upper stratosphere”? The authors are international experts who have been working intensively on this inter-comparison for several years. The exposition is logical and the technique of comparing standard deviations is helpful for concisely describing differences among data sets. They present helpful detail, both in assessing differences, and in attempting to diagnose the cause of the differences among analyses. I would like to suggest to try adding even more commentary regarding the likely causes of the differences among analyses. In particular, it would be helpful to add a few sentences in the introduction or data and analysis section which describes the altitude range of reliable data for MLS and SABER, and the degree to which they are used in reanalyses, since these are the primary contributors toward improved representation of winds and temperatures in the USLM.

It might be helpful (but certainly not required!) if a summary graph could be included showing an idealized version of the altitude range of reliability for each of the analyses, with specific labelling and footnotes which offer likely reasons for diminished quality. If the authors have access to the following information, it would also be interesting to clarify the degree to which analyses are more strongly regressed toward Singapore radiosondes compared to other tropical stations with comparable accuracy and frequency of launch. I recommend publication with minor revision.

1. I17-18: This basic difference in structure is probably just the latitudinal profile of the Coriolis parameter. It's too bad that there are very few rocket wind profiles to include. That means that differences in low latitudes among analyses may have a lot to do with differences in the manner of treatment of building up heights from temperature

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soundings. The fact that SD for temperature is larger in the polar regions may reflect modest differences in sampling of actual large-amplitude Rossby and gravity waves.

2. l19-21: This, and other things that I have read, strongly suggests that there is an embedded preference in algorithms underlying most global analyses which favors Singapore, simply by using a higher weighting factor, compared with other stations. This seems to be a legacy of respect, but should perhaps be relaxed, particularly if there are other reliable radiosonde stations in the tropics (please state them, perhaps near p2. L15-16).

3. l24-27: This is a key theme in the paper. You mention sponge layer differences and the interesting JRA-55 and 55C difference. Do you know of any specific algorithm-based differences in how different satellite data streams are dealt with in different re-analyses?

4. p2, l20-25: Can the influence of HRDI be pointed out in the figures? Are there any lidar temperature profiles that are ever included in global analyses? 5. p3, l1-5: When I was doing this for LIMS data, I tried several smoothing techniques for building up Z in the tropics to obtain a good match for zonal winds with rocketsondes. I found that a 1-2-1 smoother in latitude for temperature applied at each level before integrating thicknesses upward yielded better agreement than smoothing at each level independently. I also tried smoothing more at each level, which degraded the comparison. I also tried smoothing across different ranges of latitude, and decided that within 8s-8n is about right, so as not to include thickness information from the subtropics, spreading inward and upward. I didn't like the results from using a cubic spline, which can yield larger amplitudes at higher altitudes. I only mention this because the growth with altitude of SD among analyses for zonal wind is largest at the equator, making it likely that such simple differences in how smoothing is done may explain quite a bit.

6. p3, l14: Could point out the lack of raobs in the central and eastern pacific.

7. section 2: Several kinds of sources for differences are mentioned, including sponge

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layer treatment, and non-orographic gravity waves in MERRA-2. Is it possible to give provide more information about how different satellite data streams are treated in different analyses? On p3, l28 it was suggested that such is not possible, but to whatever extent the authors are aware of helpful information in this regard, please do describe further. Otherwise, it may be time to send in investigative reporters to find out what is in those black boxes, anyway.

8. section 2: If any reanalyses include SABER or MLS data, please describe. If so, can their effect be seen in the figures?

9. p6, description of Figure 1: Are there any other features of interest to point out besides the lack of information above 10 hPa in JRA-55C and increasing disagreement at higher levels?

10. Figure 2: Hitchman and Leovy (1986) summarized what was known from rocketsondes about the time mean vertical mean profile, which includes time mean westerlies in the lowest stratosphere, easterlies in the middle, and westerlies in the lower mesosphere, as shown here. The differences in time mean profiles in the MS shown here could be more strongly emphasized as a theme (cf. Fig. 3b). The figure 2 caption and figure need to be reconciled (there are two dashed profiles). It is very hard for this reviewer to distinguish the differences among the colors chosen for profiles. Please try to distinguish profiles more clearly, perhaps with dash-dot or thickness variation.

11. p7, discussion of Fig. 4: I don't see a special change in 1999. Please discuss why you include the MERRA panels.

12. p7, l26: "geographical" suggest variation in (x, y) to me. Perhaps "variation in the meridional plane"?

13. p8, l2: The midlatitude maximum in SD for zonal wind may be related to the climatological mean maximum location for the polar night jet (cf. Fig. 6b).

14. Fig. 7: Again, there are two dashed lines but only one is described in the legend.

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15. p11, l22: Which other analyses include MLS? Do any include SABER? Please clarify early in the paper.

16. p12, l1: Please weigh in with an editorial decision. My understanding is that an apostrophe takes the place of missing letters, such as in the word “doesn’t”, or indicates possession, but that plural never has an apostrophe, so it makes reasonable sense to write “1900s” instead of “1900’s”. Yet “1900’s” is quite commonly used.

17. p13, l11-12: This also seems to indicate a very strong regression coefficient for Singapore winds in many algorithms.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-73>, 2020.