

Review of “Representation of the Equatorial Stratopause Semiannual Oscillation in Global Atmospheric Reanalyses” by Kawatani et al.

Recommendation: Accept with minor revisions

General Assessment: This paper does a thorough analysis of how upper stratospheric to lower mesospheric winds and temperatures in the tropics from reanalyses compare with each other and with observational data derived from satellite measurements, focusing on the zonal mean flow and the semi-annual oscillation. Observational data are sparse in this region and our ability to model it is limited, so a paper such as this that quantifies how the reanalyses represent this region and discusses possible reasons for the differences is valuable. The authors do a good job of relating their results to previous literature and our current state of knowledge of this region of the atmosphere. This paper should be suitable for publication in ACP after minor revisions.

A few small overall comments:

- a. For many of the “climatological” analyses (that is, those for which averages and standard deviations are calculated over a period of years) there are several different time periods used. Especially, the reanalyses are generally used for 1980--2010, but the SABER and MLS derived fields are used for about 2002--2016 and 2005--2016, respectively. Can you say something about how real atmospheric differences between the two periods may affect the results? Have you looked at reanalysis / satellite comparisons including only the years that are available in both (if that comparison showed significantly different agreement between the reanalyses and the satellite data, it would raise questions about the reasons for disagreements seen in the current comparisons)? (As a minor clarification related to this, since several time periods were used, it would be helpful to list the periods used in each of the figure captions that show climatological fields -- Figures 2, 5--12 -- which is currently done in some, but not all, of these.)
- b. The discussion of the sponge layers in the reanalysis description and ensuing text could be made clearer if you were a little more specific in the former about comparing the vertical regions over which the sponge layers are applied, and differences between the (to use imprecise language) “severity of the damping” at the altitudes you later focus on in the paper. All the information is given, yes, but if you added a sentence or two in the initial discussion of the reanalyses about which ones are more likely to be adversely affected by this at the levels you focus on, it would help the reader follow the thread through the rest of the paper.
- c. I would like to see a little more said about the possible differences related to MERRA-2 assimilation of MLS temperatures at altitudes about the 5hPa pressure level starting in Oct 2005. Figure 13 is useful, as far as it goes, but to what degree (probably a function of latitude given the weak constraint near the equator) would assimilating these temperatures be expected to indirectly (via the underlying model) affect the winds? What about for the other comparisons shown, e.g., the climatological winds and

temperatures? Is agreement between the reanalyses and MERRA-2 significantly improved (or changed in any way) for those fields in the period when MLS data are assimilated?

- d. I am a little concerned still about using “balance” winds near the equator, given that I’ve seen previous results comparing winds calculated thus (using the same balance used by Smith et al, that originally published by Randel, 1987) from analysis/reanalysis GPH with the winds from the analysis/reanalysis system itself that showed very large differences out to 15–20 degrees from the equator at pressures below 50hPa and an expanding latitude region of differences with decreasing pressure, with large disagreement at over 30 degrees from the equator in the upper stratosphere (e.g., see Manney et al, 1996, JGR, 101, 10,311–10334; their Figure 1; recent unpublished results I have using MERRA-2 for this comparison show qualitatively similar results). I realize this is a difficult issue, since we don’t have any “truth” field to compare with, but, given that the balance wind calculation done using the GPH from a reanalysis tends to have much lower winds than those from the reanalysis, could you discuss a little more how this may affect your interpretation of reanalysis differences from winds derived from MLS and SABER?

Specific Comments (in order of appearance in the paper):

Page 1, Abstract: It would be helpful to say whether there are reanalyses (other than JRA-55C) that do particularly poorly (e.g., CFSR may fit that description for some of the diagnostics?).

Page 3, lines 27–28, could you list Tomikawa et al as “in preparation”?

Page 5, line 26, The ERA5 data on model levels (hence up to altitudes higher than analyzed here) are, and have been for quite some time, publicly available, so this statement is not accurate. However, I know from personal experience over the past year or so that acquiring those fields can be exceptionally painful and time-consuming! I would suggest simply moderating that statement to something like “were not available at the time of this writing” (particularly, leave out “publicly”)

Page 5, line 30, Not sure I have access to Shepherd et al (2018) (is there a URL you could give in that citation?), but I’m curious how they know it is spurious (what data do they have to compare to or what physics is it inconsistent with?). Since that reference may not be trivial for every reader to get, could you possibly say a tiny bit more about it?

Page 6, line 25, Related to my overall comment about the sponge layers, don’t all or most (I guess MERRA and MERRA-2’s start at a bit lower pressure) of the reanalyses have an artificial sponge layer at this pressure? Is JRA-55’s more severe?

Page 7, lines 14--15, 18, and Figure 3, It is quite difficult to see these differences in Figure 3, perhaps there is a way to improve this? Might showing differences from a reanalysis mean help?

Page 7, Line 23, This seems to me to be just as clear at lower pressures.

Page 8, lines 26--27, I find this sentence a bit confusing, is "South America" supposed to be "South Africa" or are we talking about two different regions with lower observation density?

Page 8, lines 16--30, Is there a relationship between the asymmetries in SD and asymmetries in the winds and temperatures themselves?

Page 9, line 18, MLS seems to me to show more differences in variability at different times of year, can you comment on this?

Page 9, line 26, could you give a reference for this?

Page 15, lines 2--4, "Next generation" is not appropriate here, especially since one of the reanalyses you list (MERRA) has been superseded (by MERRA-2) and discontinued!

Page 15, line 6, There are DOIs and/or references for the reanalysis datasets themselves, which should be given here. Saying that one can contact the authors for the post-processed data used is appropriate.

Typos / small corrections:

Page 1, line 20, add a comma after "Interestingly"

Page 2, line 2, suggest "The present paper focuses on..."

Page 2, line 11, add a comma after "waves"

Page 3, line 22, "which" should be "that"

Page 3, line 21, suggest "in" rather than "by"

Page 3, line 27, "is" should be "are"

Page 3, line 29 "were" should be "was"

Page 4, line 2, suggest "At" rather than "In"

Page 4, line 3, "treatments" should be "treatment", "top" should be "tops", and "are" should be "is"

Page 5, line 12, "which" should be "that"

Page 7, line 13, there is some punctuation (period or colon) missing before "MERRA-2"

Page 7, line 16, suggest "show much stronger" rather than "are much larger"

Page 7, line 19, add comma after "reanalyses"

Page 8, line 12, suggest "arise from" instead of "are due to"

Page 9, line 2, "similarity" should be "similarities"

Page 9, line 22, should be “easterlies extend” and “westerlies exist”

Page 10, line 2, “peak” should be “peaks”

Page 10, line 3, something is wrong here, the word “solstices” seems to be thrown in out of place?

Page 11, line 15, “which” should either be “that” or you could say “for which there is”

Page 10, lines 7--8, this is really unclear because of the use of higher / lower in a way where it is not clear whether it is higher / lower in pressure or altitude. Please re-word this to clarify.

Page 10, line 10, “westerly” should be “westerlies”

Page 12, line 5, delete comma after “field”