

Interactive comment on “Climatology and Interannual Variability of Dynamic Variables in Multiple Reanalyses Evaluated by the SPARC Reanalysis Intercomparison Project (S-RIP)” by Craig S. Long et al.

Anonymous Referee #1

Received and published: 23 May 2017

This study provides a comprehensive overview of the temperature and zonal wind biases in eight reanalysis data products, with a focus on stratospheric levels. The study identifies biases in each reanalysis from the “reanalysis mean” (defined as the mean of the MERRA, ERA-Interim, and JRA-55 reanalyses). It then examines reanalysis temperature biases with respect to HIRDLS (an independent satellite measurement) and MSU/AMSU/SSU satellite data products. The authors identify systematic biases and notable change points in the reanalyses associated with discontinuities in data sources, such as the transition from TOVS to ATOVS around 1998-1999. One of the key conclusions of the study is the pervasive uncertainty in zonal winds in the tropical

C1

stratosphere, largely because of the inability of reanalyses to resolve the waves that drive zonal wind variability in this region.

This paper is not likely one that most readers will read from beginning to end, as it contains a highly technical description of reanalysis biases. While many of the issues discussed have been discussed in previous literature, this document serves a centralized review by the SPARC S-RIP Project of these issues, providing a guidance document to reanalysis users (to understand biases) and to reanalysis data centers (to improve upon existing reanalysis products). For these reasons, I recommend publication of this manuscript. However, I think the paper would be more useful if it provided more detailed guidance and suggestions as to the improvements necessary in future reanalysis products. Comments and suggested revisions are detailed below.

Minor Revisions

1. The authors could do more to provide guidance to improve future reanalysis products, particularly focusing on what improvements were already made from ERA-40 to ERA-Interim, JRA-25 to JRA-55, and MERRA to MERRA-2 to reduce biases. This knowledge would be particularly helpful in interpreting the results in Figs. 6-9, where the authors compare the biases among these reanalysis products. For example, if ERA-Interim has smaller biases than ERA-40 in a certain region, it would be useful to more clearly emphasize what improvements might have reduced these biases.
2. I'm curious as to why the authors did not directly evaluate the reanalysis temperatures against GPSRO data. GPSRO provides high vertical resolution satellite-derived temperature measurements up to ~40 km altitude. It is clear from Fig. 15 that the inclusion of GPSRO data in some reanalysis products had a substantial impact after 2006.
3. I'm also curious about why the authors focus on the polar regions and tropics and do not discuss biases at midlatitudes. Is there a reason why midlatitudes are not discussed in this paper?

C2

4. The paper deserves a thorough and careful proofreading. I caught a number of inconsistencies between the manuscript text and the figures, which need to be corrected prior to publication. I've listed some examples below, but I'm sure there are others that I may have missed.

a. p. 8, Line 27: In Fig. 4c, the disagreement between 7 and 5 hPa appears to terminate in 2002, not in 1998 (TOVS/ATOVS transition).

b. p. 11, Line 11: persistent cool bias from August to November

c. p. 11, Line 12: upper stratosphere warm bias

d. p. 12, Line 14: In Fig. 8i, the CFSR biases near 100 hPa appear to stop at the TOVS/ATOVS transition, not continue through it as the text states.

e. p. 12, Line 16: 0.5 to 2 K

f. p. 12, Lines 19-28: Please double-check the magnitudes in this paragraph, as they seem inconsistent with Fig. 8f.

g. p. 13, Line 25: It does not appear from Fig. 9m that the westerlies are stronger during the TOVS period. They look stronger throughout the entire data record.

h. p. 15: The color ranges in Fig. 11 do not match those discussed in the text in section 5.

i. p. 17, Lines 4-6: In Fig. 14b, the MERRA warm bias only occurs in November through February during the first year (Nov. 2005-Feb. 2006). After that, the warm bias is primarily confined to the 5-10 hPa pressure range.

j. p. 20, Line 14: cool bias at 1 hPa and warm bias between 2-3 hPa

Line-by-line comments

p. 1, Line 19: among the reanalyses themselves

p. 2, Line 19: I didn't see any mention of the v and w wind fields in the text.

C3

p. 5, Line 13: The volcanic warming is primarily confined to the lower stratosphere.

p. 6, Line 27: Why do the minimum temperatures occur before the winter solstice?

p. 7, Line 12: This sentence seems out of place. The QBO and SAO are not introduced until the following paragraph.

p. 9, Line 27: How large are the 20CR biases in the stratosphere? It might be useful to warn readers against using 20CR data, as large biases in stratospheric dynamics might also have a substantial impact at tropospheric levels.

p. 14, Lines 5-11: MERRA-2 is not discussed in this paragraph, but it looks as if it also has sizeable wind biases in the tropical troposphere.

p. 14, Line 20: 1980-2014 period

p. 15, Lines 26-29: I'm not sure that I understand how a year-round temperature bias (+ for CFSR and - for JRA-55) impacts the amplitude of the annual cycle. Perhaps this could be clarified.

p. 16, Lines 3-8: Why would a sudden stratospheric warming increase the amplitude of the annual cycle in the Northern Hemisphere but decrease it in the Southern Hemisphere (2002)?

p. 18, Line 13: 0.5 K

Figs. 4-5: The authors need to more clearly describe what they are plotting in these figures. The standard deviation of 3 data sets seems somewhat of an unusual metric, as standard deviation is typically used for larger sample sizes than 3. It might be clearer to simply show the difference between the maximum value of the 3 reanalyses and minimum value of the 3 reanalyses at each month/latitude/pressure.

Fig. 9: It might be helpful to mark the QBO phases somehow on these figures. Otherwise, it is extremely difficult to see what the authors are discussing in section 4.2.3.

C4

Fig. 10: Pressure axis needs to be labeled.

Fig. 15: It would be useful to give the approximate altitude/pressure ranges for the TLS, SSU1, and SSU2 weighting functions, as some readers may not be familiar with them.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-289, 2017.