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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Differences in tropical high clouds among reanalyses: origins and radiative impacts" *by* Jonathon S. Wright et al.

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This paper compares a few reanalyses with a focus on their representation of tropical high clouds. As the reanalysis datasets surveyed here are widely used, it is useful to detect and document their biases. Besides documenting the inter-dataset differences, this paper makes especial efforts to relate the cloud biases to radiation impacts and to discuss possible physical causes (how they may have resulted from parameterization schemes) in respective reanalysis models. My assessment is that this paper is well motivated, logically organized and likely to be widely cited if published. I do have a few comments, which hopefully help improve the clarity of the paper. I recommend this paper be accepted for publication after these comments are addressed.

1. My first comments concern the presentation of the results. First, it baffles me why many of the biases (e.g., fig. 1 and 5) are presented with respect to multi-reanalyses mean (MRM) instead of observations. While it is understood that the observational datasets are subject to their own uncertainties and sampling discrepancies, it is still of the interest of most readers to see how each of the reanalyses compares to an observational ground truth. I strongly suggest the bias results be presented with respect to relevant observations wherever available.

Second, I find some results are presented in unconventional, and probably not advantageous, ways. One example is Fig 11. The authors may be seeking a concise way to present rich information from many variables: HTR, RH, CRE, CWC, etc., although the plots become difficult to interpret. I suggest the authors decouple these variables and use more straightforward plots to evidence their points, or, less preferably, identify what features are for the readers to recognize and explain how they relate to their points.

My last complaint about presentation is that I find some potentially very interesting and important results omitted. This applies to a few places: Fig 5. What about SW and net (LW+SW) results? A central radiative question about the high clouds is to what extent their LW and SW effects compensate [e.g., Kolly & Huang 2018; Wall et al. 2019] and how different datasets may bias this compensation [e.g., Zhu et al. 2019 Fig. S1 and relevant texts]. Fig. 10. Why not show the three related variables: T, q and z (components of MSE), respectively here? Fig. 13. What about the clear-sky OLR?

2. A technical comment: it should be cautioned that CRE, defined as the difference between clear- and all-skies, is subject to influence of clear-sky [e.g., Soden 2004]. I'd suggest where appropriate (e.g., Fig. 5) clear-sky biases be also examined to ensure that the CRE difference measures cloud effect, instead of being affected by the clear-sky differences between the reanalyses.

3. Additional comments

P3, ~L5. Note there are methods, such as latent heat nudging and particle filter, that make use of cloud and precipitation info in data assimilation.

P4 31. Another benefit of simulator, if properly configured, is that it also addresses sampling consistency issue.

P4, L33. I'm surprised to read that the aim is stated to be "qualitative" – despite many quantitative – why? If q

P6, L27; P13 L26. Note the latest CERES data includes a version of clear-sky values, computed using the same clear-sky definition as in GCM [Loeb 2019].

P7, L15, Why linear with ln(p) instead of p?

P9, L8. Note that cloud top temperature (CTT) is another potential cause of (compensating) errors.

P11, ∼L22 and Fig. 4. Are the CWC averaged over only cloudy profiles or over both cloudy and clear profiles? Consistently between all the reanalyses? Both averages would be of interest to compare.

P15, Fig. 6. How is the purple line drawn exactly?

P17, L30. OLR vs. CRE – this seems significant methodological difference. What's the rational to use CRE here?

P18, Fig. 7. I am surprised to see the lack of distinctions in the ERAi results here, reminiscent of fig. 10e of Zhang et al. [2017]. Some basic radiative signatures such as cloud top cooling and cloud bottom warming are totally missing. May this be related to the use of CRE as a state indicator - it may fail to identify cloud effect due to clear-sky bias difference (see comments above)? Would it be useful to simply use cloud fraction instead to identify the regimes (Q1-4)? How does cloudy heating rate profile compare to clear-sky in this reanalysis?

P19, Fig. 8. Is it LW or net radiative heating that is used to define LZRH and the relevant results?

P20, L20. Isn't it the diabatic heating (instead of radiative only) that better inferences ascent/descent? Why focused on radiative?

P21 "Possible origins". The discussions in this section don't distinguish cause and effect of previously presented cloud biases. Maybe worth some clarification and discussion here about this.

P24, L5. The way the current plot is made makes it difficult to discern the "kink".

P26, section 5.2 and Fig 11. Besides the confusing way the figure is presented as noted above, I find the discussion here doesn't show enough recognition of the cloud position with respect to the respective levels focused (100 and 150 hPa). A basic signature of clouds is cloud top cooling and bottom warming. The sign and magnitude of the cloud radiative impact is strongly dependent on where the clouds are placed.

P29, L31. "systemic" => "artificial"?

P29, L32 and Fig. 13. A striking feature is that OLR doesn't seem "jumped" despite of the cloud fraction jump! How could cloud cover change be consistent with OLR and CRE with regard to long-term trend but inconsistent with regard to this jump? Also, the long-term trends seem similar among the reanalyses – should this be taken seriously as a sign of real trend in nature? It is important to reason and caution whether we can use these reanalyses for studying climate trend in this critical region. It should be noted that the all-sky OLR trend appears to contradict the FAT hypothesis [Hartmann & Larson 2002].

P30, L23. The clear-sky OLR change described sounds very interesting and ought to be shown. Relevant to the above point, another important question the reanalyses may or may not answer is whether broadband fluxes, either clear- or all-sky, may be useful for climate change monitoring. As shown by Huang & Ramaswamy [2009, Fig. 5], there may be intrinsic compensation between greenhouse gas forcing and Planck response that results in no trend signal. This point, together with the above one, is worth noting and discussing here.

P31, Fig. 13. Some of the time series apparently don't have zero mean. How are the anomalies defined?

P34, Summary. A general suggestion for this section is to reference the respective summary points to the relevant figures.

P34, L20. It is striking to find the lack of agreement among the studies in terms of what direction cloud drives the LZRH. Can you discuss why and how would one elucidate this matter?

P36, L29. Cloud top temperature, as related to in some of the above comments, is perhaps another aspect to note.

P37, Appendix. It may be worth reviewing the difference in assimilated data in this appendix as well. This is apparently relevant to the trend discussions (section 6) and potential affects climatology as well.

P37, L25. What is liquid water temperature?

P43, L1. Sufficient info to ensure reproducibility of the results should be included. Regarding the data sources, how were the data, such as ERA5 heating rates, obtained exactly, as they are not normally available from the webpages stated here? If scripts were used, it is useful to post a sample script and explain how relevant parameters, e.g., analysis vs. forecast and, if latter, forecast times and steps, are set. Moreover, are the these parameters set consistently for all the variables: heating rate and state variables such as cloud fraction, temperature, humidity, etc. from the same time steps?

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