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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of vertical air motion among reanalyses and qualitative comparison with direct VHF radar measurements over the two tropical stations" *by* Kizhathur Narasimhan Uma et al.

Anonymous Referee #1

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This study compares and assesses vertical wind data from five global atmospheric reanalysis data sets. As independent measurements, it uses VHF radar measurements from two tropical stations, one at Gadanki, India, and the other at Kototabang, Indonesia. This is a very important trial and should be published in the end.

The main issue of the manuscript is, I think, in the apparent large discrepancy between the radar measurement results and the reanalysis data. On the other hand, I think we can say that the five reanalysis data sets show qualitatively similar seasonal and vertical distribution of w; there are differences, but considering that omega depends heavily

on forecast model of each reanalysis system, without direct observations assimilated, these differences (among the reanalyses) may be understandable.

I am afraid that we even need to start from suspecting any errors in the data processing and analysis for reanalysis omega. Did the data really come from the correct (intended) grid point? Is the conversion from omega to w really correct? I do believe that the authors did the correct procedure, but we need some more cross-check information to confirm that they really did, simply because the difference from the radar measurements is too large. One note is that the authors should remove data at some lowest levels where the reanalysis systems simply extrapolate data below the surface (the altitude of Gadanki and Kototabang is 360 m and 865 m, respectively).

Regarding the radar measurements, it would be very useful to discuss why w shows such seasonal and vertical distributions. What processes produce upper tropospheric ascending motion and lower tropospheric descending motion, the latter for Kototabang for all seasons and for Gadanki for April to October? Are there any publications that discuss this? There are several publications on the measurements of VHF wind profiler at Christmas Island (2N, 157W) (Gage et al., 1988, 1991, 1992). Their vertical wind profiles may look rather similar to what Kototabang measurements show. Thus, their discussion may be useful. Once we (rather theoretically) understand how the actual w distributions may look like, we can get more insight on why all these reanalyses show such distributions.

In summary, I think we need much more information (maybe direct or maybe indirect) that may be useful to understand why the radar measurements and reanalysis data show such different distributions.

Some specific comments (here, for this time, I only list major ones):

Page 2, lines 43-49, and page 3, line 68, and other places: The use of the terms "direct" and "indirect" may need to be reconsidered. "Direct" may be used for in situ measurements (e.g., radiosonde horizontal wind measurements), while "indirect" may be used as "indirect estimation" e.g., of w from horizontal wind measurements/data to consider their divergence/convergence. For the case of radar measurements, we may use the term "remote sensing" measurements, because these may not be "direct" measurements (they are not in situ measurements) but at the same time these may not be "indirect" which implies indirect estimation from other variables in the context of this manuscript.

Pages 5-6, Section 2.1: The full location information on the two radar sites need to be written in this section. The information on altitude, country/island, and the institutes that operate these radars is missing. Also, please explain the topography around each of these radar sites rather extensively. Gadanki is located within high land of a continent (with a horizontal distance of ** km from the oceans), while Kototabang is located within a narrow mountain range of an island (of a scale of ** km in northwest-southeast and ** km in northeast-southwest), etc. The topographic information may be very important to judge the representativeness of reanalysis data at a particular grid point (and at the same time, the representativeness of each of these radar measurements). The direct time information in UTC should be provided, because reanalysis data are in UTC. It would be useful to show the profiles of data number, i.e., of the original ones, of the quality controlled ones, of the finally used ones (after discarding data points >1 m/s and <-1 m/s), etc. The information on the quality control procedure is also needed. (The authors listed possible issues in the radar measurements, but they did not explain what they actually did to avoid such issues.)

Page 7, line 157: Which MERRA-2 data product was used, ASM or ANA?

Pages 9-, Section 3: Please see my comments above. Also, investigation of a case or two (e.g., for a week or for a month) might be useful to understand what is going on for both radar measurements and reanalysis data.

Figure 8: Results from Gadanki and Kototabang should be shown in separate panels/figures. ERA-Interim is used as a reference. But, considering that ERA-Interim

is an outlier for some cases, the Reanalysis Ensemble Mean (REM) may be a better reference. Finally, a reference should be "subtracted", e.g., ERA5 minus REF, not REF minus ERA5.

References:

Gage et al., A Comparison of Winds Observed at Christmas Island using a Wind-Profiling Doppler Radar with NMC and ECMWF Analyses, Bulletin American Meteorological Society, Vol. 69, No. 9, 1041-1046, 1988.

Gage et al., Long-Term Mean Vertical Motion over the Tropical Pacific: Wind-Profiling Doppler Radar Measurements, Science, Vol. 254, No. 5039, 1771-1773, 1991.

Gage et al., DIURNAL VARIATION IN VERTICAL MOTION OVER THE CENTRAL EQUATORIAL PACIFIC FROM VHF WIND-PROFILING DOPPLER RADAR OBSERVATIONS AT CHRISTMAS ISLAND (2 N , 157W), Geophysical Research Letters, Vol. 19, No. 18, 1827-1830, 1992.

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