

Interactive comment on “Tropospheric dry layers in the Tropical Western Pacific: Comparisons of GPS radio occultation with multiple data sets” by Therese Rieckh et al.

Anonymous Referee #1

Received and published: 22 September 2016

General comments:

The manuscript presents results from a very interesting intercomparison of observations collected during an experiment in the tropical western pacific with other in-situ and satellite observations and with model data, with a focus on humidity. The presentation is very clear, and the manuscript is written in very good English. Still, a few minor issues should be addressed or answered by the authors before publication.

Specific comments:

Page 2, lines 2-3: In my opinion there is no doubt in the literature that super-refraction conditions do exist. I therefore recommend to reformulate: "if super-refraction condi-

Printer-friendly version

Discussion paper



tions exist" -> "under super-refraction conditions"

Section 2.2 ("The CONTRAST experiment"): For the reader's sake, there should be a very brief explanation of the sensors used for the aircraft observations, in particular the derivation of water vapor pressure, as this is a central quantity in this study. The Pan et al. paper has no full reference and could not be found.

Section 2.3 ("ERA-Interim Reanalysis") and section 2.4: The ERA-Interim dataset is available on the original 60 model levels, as well as interpolated to a predefined set of (coarser) pressure levels. It appears that the limited vertical resolution of the latter is visible in some figures (e.g. fig. 2 and 3). The same may apply to GFS model versus pressure-level data.

Models do certainly suffer from too strong numerical diffusion. Nevertheless, did the authors verify that their use of pressure-level instead of model-level data does not have any impact on the comparisons in sections 3 and 4?

Page 12, figure 5: the caption should refer to the (central) wavelengths of the MTSAT-2 IR channel used here (10.8 and 6.75 μm).

Section 5: given the scatter plots in fig.7, the authors should have noticed that the models appear to simulate very dry situations with comparable frequencies, while the aircraft data tend to show very dry situations with a significantly higher frequency. The "RO relative humidity vs. CONTRAST" in fig.6 (bottom-right) shows a similar pattern. I do not assume that the authors could explain the reason for these (common) features, but they should at least mention this apparent discrepancy.

Page 13, figure 6: given the distribution of points in the scatter plots, and that refractivity primarily depends on density resp. pressure, did the authors consider to use a logarithmic scale for refractivity?

Section 7, conclusion 7: "When compared to CONTRAST, RO has a moist bias for low humidity values, and a dry bias for high humidity values". While this may be true for low

[Printer-friendly version](#)[Discussion paper](#)

humidity values, I am less convinced by the results from section 5 that this is true for high humidity values, as the correlation fit is assumed to be valid for the full (log-scale) humidity range. Restricting the fit to e.g. the range $q > 1$ g/kg, there appears to be only a small bias.

Technical corrections:

Page 4, lines 6-7: The official spelling is "Metop", not "METOP". See EUMETSAT's web site or the WMO OSCAR database. Similarly "TerraSAR-X", not "TerrSarX".

References:

Pan et al.: journal reference?

Randel et al.: more details needed (journal reference?)

[Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-258, 2016.](#)

[Printer-friendly version](#)

[Discussion paper](#)

