

[Interactive
Comment](#)

Interactive comment on “Analysis of upper-tropospheric humidity in tropical descent regions using observed and modelled radiances” by V. O. John et al.

Anonymous Referee #1

Received and published: 25 June 2013

In this paper, the authors make use of HIRS and AMSU-B observations of the upper tropospheric humidity (UTH) to confront the modeled UTH produced by the HadGEM2 GCM. The comparison is performed over the last 2 decades (1989–2008 with HIRS / 1999–2008 with AMSU-B) and in large-scale descent areas selected with the 500hPa vertical velocity extracted on one side from the ERA-Interim analysis when dealing with the satellite observations and on the other side from the model itself.

Except the fact that this paper evaluates the HadGEM2 model, this paper does not add a clear significant contribution to the water vapor feedback analysis in climate models, like the abstract seems to imply.

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



If such contribution is the goal of this study, a more deepened review of the existing work should have been presented in order to place this work into the global picture.

Thus, some important reading seem to miss in this paper such as - Allan et al 2003 (QJRM) - Huang et al 2005 (GRL) for analysis on GCM vs HIRS T12 measurements

- Allan and Soden 2007 (GRL) for discussion about trends in the ascending/descending branches of the tropical circulation

- Pierrehumbert et al 2006 - Soden et al 2005 (Science) - Larson and Hartmann 2003 (J. Clim) - Bony et al 2006 (J Clim) for discussion on the water vapor feedback and its evaluation in climate models

and more specifically the - Sherwood et al 2010 (JGR) paper that deals with CMIP3 archive and the RH changes associated to a warmer climate

This is just a short list of the existing work that deserve to be examined in a model vs observation study about upper tropospheric relative humidity.

Of course I am not expecting a dedicated review paper, such as the Sherwood et al 2010 (Rev Geophys) not even mentioned, but the present work is missing the important part of scientific context.

Finally there is no discussion on the HadGEM2 model as should be expected from a model vs observation study: what are the improvements ? the advection/convection schemes ? Could there be a link between those schemes and the "no trend" results ?

Several aspects are missing and a general improvement of this article should yield to more specific comments.

To conclude, I cannot recommend this article for publication in ACP in this current state.

Some specific comments: pp 10554 l8: The authors make statement like such as "no trend in the UTH", but the study is restricted to large-scale subsiding areas only, but there is no discussion on the global circulation. How is it possible to decompose the

water vapor feedback into ascending and subsiding parts ? Of course the restriction to the subsiding branches of the Hadley cells allows to avoid any cloud bias in the observed UTH, but the discussions go too rapidly towards a conclusion.

Figure 2: the figure represents a simple histogram of UTH in the model and the AMSU-B UTH. Why isn't the HIRS UTH not represented ? is it only because of the IR / MW differences in terms of cloud screening ? On this figure, the difference in the moist end of the histogram is only discussed in terms of return flow of the monsoon circulation. How different are the 500hPa vertical velocities between ERA-Interim and the model ? Could this be due to a too smooth lateral mixing (eg. discussion of Pierrehumbert et al 2006) ?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10547, 2013.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

