Atmos. Chem. Phys. Discuss., 12, C10998–C11003, 2012
www.atmos-chem-phys-discuss.net/12/C10998/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A discrepancy in precipitable water among reanalyses and the impact of forcing dataset on downscaling in the tropics" *by* H. G. Takahashi et al.

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Received and published: 24 December 2012

[CXX]: referee comments

(RXX): response to referee comments

[C1] General comment

Both the title and the abstract indicate the two components of the work. On one hand an inter-comparison among 7 reanalyses and on the other the analysis of the impact they have on downscaling for the tropical regions. However through the manuscript the first is presented in qualitative terms as a description while the second is poorly analysed.

From the documentation of the reanalyses and several papers it is well known that each of them have important biases (whether dry or wet). It is not surprising then to find that a drier/wetter than normal reanalysis may produce a drier/wetter than "normal" simulation. This "result" is in fact commented all over the manuscript. We know it is important as we know that the biases are there. However this is not substantially a result of pure scientific interest. What may be of interest is the reason of the biases themselves and how they do produce biases in the simulation and therefore provide a proper quantification of the biases in the simulations. Precipitable water is fundamental for precipitation, mainly in the tropics where we know modeling is quite complex and most of global water vapour is trapped in the lower troposphere. But the analysis provided required a bigger effort. The issue the authors aim to tackle is of interest because of the importance of precipitation and the difficulties of modelling in the tropics. Now regarding the impact of the biases in downscaling, in the manuscript a detailed analysis which may support the importance of the study is missing and much more work is required in order to make it publishable. A recommendation is to consider the two problems aimed to be analysed in the manuscript separately and work on a complete and detailed study of the biases of precipitable water in the reanalyses based on the features of each with its correspondent statistical analysis. Then tackle the problem of the impact of the discrepancies on downscaling, notice that you may be interested in considering SST too as it is strongly related with PW in the tropics and is a main forcing of mesoscale modelling. Spell check suggested.

(R1) We are very grateful to Reviewer #1 for his/her time and careful reading in pointing out where improvements could be made. We will appropriately address each of the points raised and have altered the text and added figure of sensitivity experiments to address the concerns as follows in the revised manuscript.

As you pointed out, quantification of biases and uncertainties of PW among reanalyses is very important. We have shown the biases and dispersion in PW (Page 23766, Line 9-11). We think that the information is not only technical problem but also scientific issue. In general, it is very difficult to clarify the causes of the discrepancy in PW of each reanalysis by a paper. Thus, we would like to point out this problem by this manuscript.

The impact of SST on precipitation is basically very important, as you pointed out. In this case, it was found that the impact of lower boundary forcing, such as SST and soil moisture, was much smaller compared with the impact of water vapor field, because additional sensitivity experiments showed the discrepancy in the simulated precipitation was explained by the water vapor fields (See also response with Fig. 1 to the general comments for Referee#1).

Specific comments

Section 2:

[C2] How were the domains defined? Why did you select July 1998 for your simulations? Notice that the 1997/1998 ENSO cycle has been found to be associated with intense precipitation variations in some regions. How may the particular conditions of the selected analysis period influence your results? The effect of ENSO for the analysis domain for Jul 1998 should be indicated.

(R2) We showed our results were common in the other cases. We have checked the tendency was basically same in June 1998. We checked SST condition over the ENSO monitoring region and found that June 1998 was normal condition and July 1998 is relatively cold condition. In addition, we have checked convective activity over the target domain and found that the anomaly of outgoing longwave radiation (OLR) from 30-year mean (1979-2008) was basically within -10 W/m2 and 10 W/m2 (not shown). In addition, we conducted additional experiments for July 1997 (DS-ERAint, DS-NCEP1). The result was similar to July 1998 (not shown the new manuscript to avoid confusion).

[C3] It should be useful to include a table with the features of the reanalyses and more importantly with an indication of how each compute PW

(R3) We cited description papers on reanalyses. PW was basically provided as standard output.

[C4] How about the uncertainties and biases of the NVAP data? How do they may influence the comparisons?

(R4) We discussed the point in Section 2 (Page 23763, line 14-21).

[C5] You neglect cumulus convective parameterization because of its "unrealistic" representation of precipitation but we know very well how important is cumulus convection in the tropics. How you assumption of neglecting it may influence the results of the simulations?

(R5) As you pointed out, cumulus convection is very important in the tropics. Because we used 3.5-km resolution model, realistic convective activity can be reproduced without cumulus parameterization. "Unrealistic" means spatial distributions of precipitation and phase of diurnal cycles of precipitation were largely different from observation. For example, nocturnal precipitation was not simulated with a cumulus parameterization.

[C6] A table with the description of the model and parameterizations may be useful too.

(R6) To explain the model setting, we would like to describe in text.

[C7] Moreover, under which assumptions you have chosen the used parameterizations?

(R7) We determined the parameterization by trials and errors. We checked the reproductivity of regional climate, particularly basic precipitation system, such as spatial distribution of precipitation, diurnal precipitation cycle, and related local atmospheric circulations.

Section 3

[C8] The results are presented basically as a description, your analysis may be improved to provide a proper quantitative analysis of biases and uncertainties. Such results may be of interest of another type of journal, perhaps a technical note report on biases in reanalyses may be worth. Notice that some papers on differences among reanalyses for other variables have been published (see references recommended at the end)

(R8) As you pointed out, quantitative analysis of biases and uncertainties are very important. We have shown the values (Page 23766, Line 9-11). Because the previous expression may not be clear, we changed as follows;

Compared with the range of inter-reanalysis dispersion in the global mean PW, the range of inter-reanalysis dispersion in the tropical mean PW is very large. The range of inter-reanalysis dispersion in the tropical mean PW is from about 2.5 mm to 4 mm, as estimated from the difference between PW of ERA40 and NCEP1. The range of inter-reanalysis dispersion can be a magnitude of uncertainty of PW among reanalyses.

Following a comment of Referee#1, the averaged period was changed from 1979–2008 to 1988-1999. Thus, the values are somewhat changed.

Thank you for providing interesting papers. Some of papers that you suggested are dealing with multiple reanalysis datasets for their analyses. However, their foci were basically different regions, such as polar and extra-tropical regions. As Referee#2 stated in the general comments, water vapor is fundamental for precipitation, particularly tropics. However, objective inter-comparisons of PW in reanalyses over the tropics are limited (such as Trenberth et al. 2005, which has been cited in the previous manuscript). Thus, description of biases and uncertainties of PW among the reanalysis in our manuscript can be valuable.

[C9] Are you comparing different time spans?

(R9) We confirmed the similar wet/dry biases from 1979 to 2008 (2001 for ERA40). The biases were found for the whole period of the reanalyses and throughout the annual cycle as shown in the manuscript.

[C10] Wet/Dry biases in the simulations are clearly linked to biases in the reanalyses. Furthemore you are not dealing with the effect of these biases in the simulations beyond indicating that the biases are reflected in the simulations.

(R10) Thank you for evaluate our results. We cannot understand the second sentence of this comments. We cannot respond to this comments.

[C11] Regarding your "suggested result" in page 23769 line 22: is it domain dependent? How do we link active precipitation with precipitable water?

(R11) As you pointed out, the threshold value may be domain dependent. The value is associated with air temperature. In general, water vapor condenses in high PW conditions, when there is a trigger, such as development of boundary layer and convergence of local winds.

[C12] Some reference that may be of interest for the authors:

(R12) Thank you for providing interesting papers. Some of papers that you suggested are dealing with multiple reanalysis datasets for their analyses. However, their foci were basically different regions, such as polar and extra-tropical regions. As Referee#2 stated in the general comments, water vapor is fundamental for precipitation, particularly tropics. However, objective inter-comparisons of PW in reanalyses over the tropics are limited (such as Trenberth et al. 2005). Thus, discretion of biases and uncertainties of PW among the reanalysis in our manuscript can be valuable.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23759, 2012.