Atmos. Chem. Phys. Discuss., 12, C10986–C10996, 2013
www.atmos-chem-phys-discuss.net/12/C10986/2013/

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.

H. G. Takahashi et al.

hiroshi3@tmu.ac.jp

Received and published: 14 January 2013

[CXX]: referee comments

[RXX]: response to referee comments

[C1] General Comments: The paper can be divided into two components. The first, the comparison of precipitable water (PW) from 7 different reanalysis data sets, could be worthy of a technical note but not publication in a scientific journal such as ACP. The discrepancies among these reanalyses are not surprising nor are they particularly noteworthy. It is well known that the hydrological cycles of these data sets are strongly influenced by modeled physics and poorly constrained by observations. Thus, these results are not scientifically interesting in themselves. If the authors identified fundamental science issues at the root of these discrepancies, then they might become scientifically interesting. Nevertheless, it would be useful to document these discrepancies in a journal less focused on scientific results. If the authors choose to publish the comparisons in a technical journal, the comparisons should be expanded to include a wider range of statistical measures.

The second component is the impact on downscaled precipitation of precipitable water in the boundary forcing data. This issue is scientifically interesting, but the analysis provided is not convincing and much more work needs to be done before that result is publishable. The authors perform four experiments – each using a completely different reanalysis forcing data set. This has two problems: it is a very small statistical sample and does not isolate PW as the cause of, or even a contributor to, the precipitation discrepancies. The authors do attempt to dismiss dynamical boundary forcing as a cause by arguing that the wind fields among the boundary forcing data sets are too similar to profoundly affect precipitation rates. Their arguments are inadequate: their analysis of dynamical boundary forcing is limited to small portion (850 mb winds) of the overall dynamical forcing, they do not show that the dynamical differences are indeed smaller than the precipitable water differences, they do not show what the impact of either the dynamical differences alone or the PW differences alone make on downscaled precipitation.

The simplest way to estimate the impact of PW boundary forcing on downscaled precipitation would be simple to perform downscaling experiments that alter only PW. For example, perform an experiment that uses ERA-interim dynamics and PW of the form PW = f*PW(ERA-interim)+(1-f)*PW(NCEP1) Where, f varies from 0-1, PW(ERA-interim) is the PW from ERA-interim and PW(NCEP1) is that from NCEP1. If, using f=0, the NCEP1 precipitation results are reproduced, then one can conclude that PW 'can' cause the discrepancy. This is not final proof that is 'does' cause the discrepancy, but it is a strong indicator. Using a few different values of f would also reveal how nonlinear the response of downscaled precipitation to boundary forcing PW is.

[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.

The discrepancy in PW among reanalyses may be not scientifically interesting, but we think quantification of discrepancy in PW over the tropics among reanalyses is useful to understand hydrological and energy cycles, which should be scientifically interesting. We provided fundamental information of biases and uncertainties of column integrated water vapor among reanalyses, which should be an interesting scientific topic. Specifically, most of reanalyses showed dry biases, which should be commonly associated with each energy budget and hydrological cycle. The range of uncertainties of PW among reanalyses over the tropics was larger than the range of seasonal change of PW in each reanalyses, which be also associated with energy budget and hydrological cycle. In this manner, this paper quantitatively showed the valuable scientific results. However, the investigation of causes of the discrepancy of PW should be the other challenging issues. In general, these many problems can't be addressed by only a paper. This should be a further study.

Objective inter-comparisons of PW over the tropics in reanalyses were limited (such as Trenberth et al. 2005), even though PW is fundamental as the source of precipitation over the tropics. Although Referee2 (not Referee1) introduced some papers on inter-comparison of multiple reanalysis datasets, their foci were basically extratropical regions. In addition, the amount of PW should be associated with the other meteorological valuables, although we cannot address much more topics in this paper. We think assessment of discrepancies in reanalyses should continue because understanding this problem can facilitate the assessment of biases and, hence, the improvement of AOGCMs. Thus, we focus on discrepancy in PW over the tropics in this paper.

We conducted two types of sensitivity experiments to estimate the impact of PW boundary forcing, following your suggestions. We found that the discrepancy of the simulated precipitation was basically caused by PW in boundary conditions (Fig. 1). The result of the sensitivity experiments, which was suggested by Referee1, can explain the impact of PW boundary forcing on the simulated precipitation was significant (This was major concern of Referee1).

One was same as DS-ERAint (DS-NCEP1) but relative humidity of boundary conditions at all pressure levels were replaced by that of NCEP1 (ERAint), which was named as ERAint-rh-NCEP1 (NCEP1-rh-ERAint). If dynamical fields has significant impact, ERAint-rh-NCEP1 (NCEP1-rh-ERAint) should be similar to DS-ERAint (DS-NCEP1). The result showed the amount of simulated precipitation of DS-ERAint (DS-NCEP1) was almost the same as that of NCEP1-rh-ERAint (ERAint-rh-NCEP1), which implies that impact of PW was significant. The result also indicated that the dynamic fields in the reanalyses were basically good.

ACPD
12, C10986–C10996, 2013

The other was same as DS-ERAint but for relative humidity of the boundary conditions. Relative humidity of the boundary conditions were reduced by 95%, 90%, and 85% of those of DS-ERAint, which were named ERAint-095, ERAint-090, and ERAint-085. The results showed 5% dry bias was significant impact on reproduction of precipitation amount. Therefore, it was found that reproductivity of the precipitation amount was very sensitive to absolute value of PW in boundary conditions, even if dynamical fields and spatial-temporal variations of water vapor of forcing data were same.

Specific Comments:

[C2] Abstract, page 23760, lines 6-7 (23760,6-7): "very small compared to observation". Very small is neither an objective description nor accurate unless a meaningful context is given. Differences with observations shown in Fig. 3 are $< 5\%$ of the observed value. In many contexts, a 5% discrepancy between reanalyses and observations would be considered a good agreement. Please use objective criteria and put errors into a meaningful context.

[R2] As you pointed out, the sentence may be subjective. We specified the amount of discrepancy in Abstract. (Results showed that the absolute amounts of PW in some reanalyses were 5 to 10% smaller compared to the observation.....)

[C3] Abstract, 23760,22-24: The statement "Downscaled models can provide realistic simulations of regional tropical climates only if the boundary conditions include realistic absolute amounts of PW" is too strong given the supporting evidence in the paper. The authors have only shown that, in 4 cases, the two with lower precipitable water have unrealistically low precipitation. Furthermore the two wet reanalyses are related (ERA40, ERA-interim) as are the two dry reanalyses (NCEP1, NCEP2). One could argue that there are only two independent data points for this statement. Furthermore, no controlled experiments were performed. Differences in the downscaled results from ECMWF reanalyses v. NCEP reanalyses could arise from many causes.

[R3] As you pointed out, the expression was too strong. This is associated with your general comments. However, following your suggestion, our added sensitivity experiments support the sentence (See also the response of general comments). We only confirmed realistic simulations of precipitation over the tropics not regional tropical climate. We changed to as follows; Downscaled models can provide realistic simulations of precipitation over the tropics ......

ACPD
12, C10986–C10996, 2013

ERA40 and ERA-interim (NCEP1 and NCEP2) are basically similar. However, the amount of PW was clearly different between NCEP1 and NCEP2, which was associated with different physical schemes (As you described general comments of Referee1). The difference was clearly found the simulated precipitation (DS-NCEP1 and DS-NCEP2). In addition, the results of the five sensitivity experiments support our statement. As we stated above (in response of general comments), the amount of the simulated precipitation of DS-ERAint was similar to that of NCEP1-rh-ERAint and the amount of the simulated precipitation of ERAint-095, ERAint-090, and ERAint-085 decreased by only reduction of relative humidity. Thus, we concluded that the discrepancy of the simulated precipitation was mainly caused by difference in PW between ERAint and NCEP1/NCEP2.

[C4] Sec. 1, 23761,8 and 12: The use of the word "precise" is ill advised in this context. It could imply that the authors think that climate models need to be able to simulate actual ocean-atmosphere states as they are observed in order to provide reliable climate forecasts. However, it is unlikely that any ocean-atmosphere model will be able to do so in the near future. Furthermore, a climate model can be very valuable by providing reliable climate state statistics, without reproducing precise states.

[R4] As you pointed out, the word "precise" was not be appropriate. We would like to the word replace by realistic. In addition, we strongly agree with your statement that a single cimate model study is very important without reproducing precise states. At the same time, it is valuable to improve reanalyses and climate models toward realistic simulation.

[C5] Sec. 2, 23763,10-11: The sentence “In general, : : :” implies that the authors used reanalyses data to evaluate couple ocean-atmospheric GCMs when, in fact, they did not.

[R5] Thank you for your comments. However, we found many papers that evaluate GCM results using a reanalysis or multiple reanalyses. We cannot understand this comment.

[C6] Sec. 2, 23764 and Fig. 1 caption: the regions D1 and D2 are not explained sufficiently.

[R6] Thank you for your comments. We added explanation of D1 and D2.

[C7] Sec. 2, 23764,14-15: The statement "The first two days of the simulations were not used as a spin-up period" is confusing. Do the authors mean to say that these days were used as spin-up and, so, were not included in the analysis?

ACPD

12, C10986–C10996, 2013

[R7] Thank you for your comment. We rewrote the sentence as follows; The first two days of each simulation were not used for analysis but as a spin-up period.

[C8] Sec. 3, 23765: I think the authors should make the comparisons during the 12 yr NVAP period (1988-1999) for all data sets since they overlap for this period. This probably won't change the results, but is a cleaner comparison than the one used.

[R8] As you pointed out, 12-yr means may be better. We reproduce the figure. The dispersions were larger, because sample number of the means was decreased. The result was unchanged (Figs. 2, 3a 3b).

[C9] Sec. 3: Observational (NVAP) data: It would be useful to know what the uncertainties of monthly/tropical, monthly/global means are.

[R9] We showed biases and dispersion in tropical PW (Page 23766, Line 9-11). We cannot understand this comment. How do we estimate uncertainties from only observational data?

[C10] Fig. 3: Why were JRA25 and NCEP2 omitted?

[R10]To facilitate to see, we omitted the two reanalysis. We reproduce the figure (Fig. 3)

[C11] Fig. 6 caption: Caption needs to be clarified. What are the dark bars? How can 12-yr mean PW be the monthly mean PW for July 1998 as implied by the last sentence in the caption (seems contradictory)?

[R11] Thank you for your comments. We rewrote the caption. Dark bars are 12-yr mean PW. Opened bars are PW in July 1998, which was used for downscaling experiments.

[C12] Conclusions: It seems to me that if errors in the PW boundary forcing are indeed responsible for large precipitation errors in downscaled models, then the downscaling process might be amplifying errors in the boundary forcing. If true, this could be a fundamental failing of the downscaling method used by the authors.

[R12] As you pointed out, our method might amplify errors by our unknown problems. However, we have used same method for many downscaling experiments for various regions including tropics (Some papers were cited by this manuscript). Thus, the possibility of fundamental failing of our downscaling method is low.

Technical Comments:

ACPD

12, C10986–C10996, 2013

[C13] Abstract, 23760,24-36: The sentence "Use of boundary conditions that include realistic absolute amounts of PW in downscaling in the tropics is imperative at the present time" repeats what is said in the previous sentence and can should be deleted.

[R13] Thank you for your comments. We deleted the sentence. The previous sentence was changed as follows; Downscaled models can provide realistic simulations of regional tropical climates only if the boundary conditions include not only realistic spatial-temporal variations in water vapor but also realistic absolute amounts of PW.

[C14] 23764,6: "boudanry" should be "boundary"

[R14] Thank you for your comment. We corrected.

[C15] 23764,25: Should "Noah" be "NOAA"?

[R15] Noah is correct.

Captions:

Fig. 1: Simulated precipitation amounts averaged over the Indochina domain (97–107.5°E, 10–18°N). The values for the precipitation are shown as a proportion of the amount of simulated precipitation of DS-ERAint. N1rhERA, ERA95, ERA90, ERA85, and ERArhN1 are NCEP1-RH-ERAint ERAint-095RH, ERAint-090RH, ERAint-085RH, and ERAint-RH-NCEP1, respectively.

Fig. 2: Scatter plot between annual and global mean PW and the annual and tropical mean PW in the reanalyses and observation. The PWs were averaged over 12 years from 1988 to 1999. The units of the x-axis and y-axis are mm.

Fig. 3: Climatological seasonal marches of (a) the tropical mean PW and (b) the global mean PW of the reanalyses and observation over 12 years from 1988 to 1999. The units are mm.

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

ACPD

12, C10986–C10996, 2013

Interactive Comment

ACPD

12, C10986–C10996, 2013

Interactive Comment

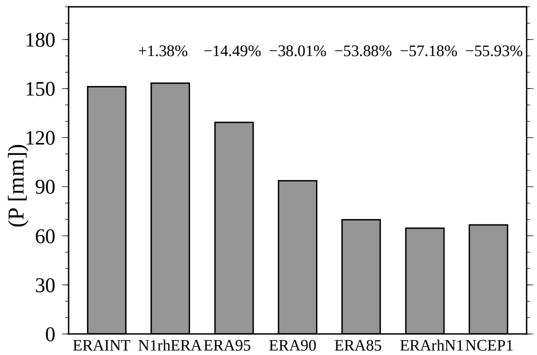

Fig. 1. Fig. 1: (See text)

ACPD
12, C10986–C10996, 2013

Interactive Comment

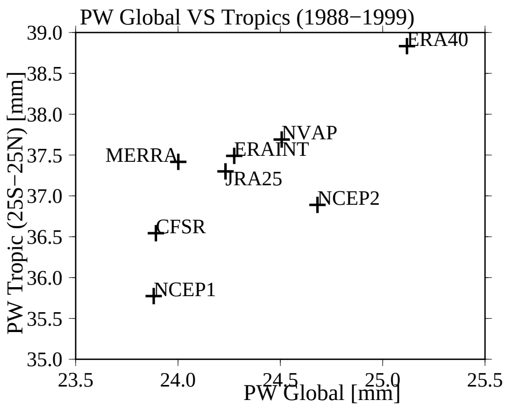

Fig. 2. Fig. 2: (See text)

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

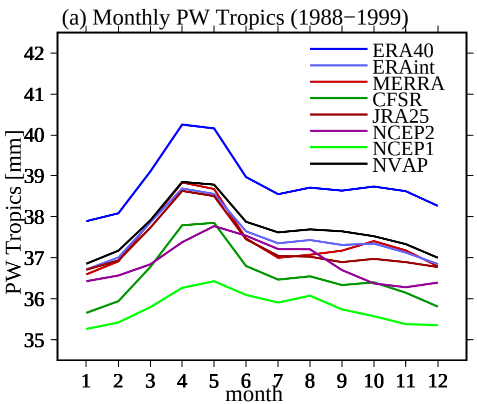

Fig. 3. Fig. 3a: (See text)

ACPD
12, C10986–C10996, 2013

Interactive Comment

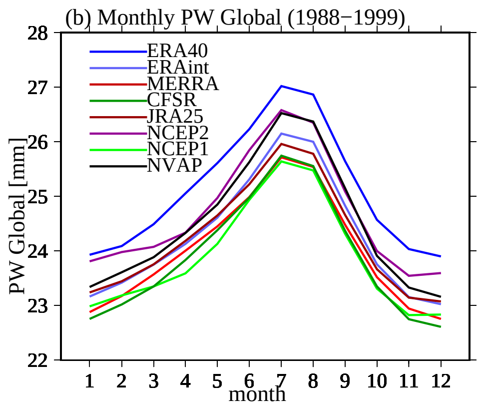

Fig. 4. Fig. 3b: (See text)

ACPD
12, C10986–C10996, 2013

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper