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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.

Anonymous Referee #1

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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 inter-

esting. 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

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precipitation to boundary forcing PW is.

Specific Comments:

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 $\sim<$ 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.

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.

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.

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.

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

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?

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.

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

Fig. 3: Why were JRA25 and NCEP2 omitted?

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)?

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.

Technical Comments:

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.

23764,6: "boudanry" should be "boundary"

23764,25: Should "Noah" be "NOAA"?

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