

Interactive comment on “How robust and (un)certain are regional climate models over the Himalayas?” by A. P. Dimri

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Interactive comment on “How robust and (un)certain are regional climate models over the Himalayas?” by A. P. Dimri Anonymous Referee #1 Received and published: 13 February 2015
GENERAL COMMENTS The evaluation of Regional Climate Model (RCM) data over remote mountainous regions is a valid and useful research endeavour. A significant hindrance to such research, however, is the lack of observed data against which to compare the models. In this paper temperature and precipitation values in three RCMs are compared against gridded observed data and three (rare) station series at a remote albeit geophysically important glacier in the Western Himalaya and its surrounding area. The station-level analysis is useful, and the paper includes an interesting analysis of the vertical atmospheric temperature profile which

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is shown to be biased in the RCMs considered. The comparison against the gridded data is a weaker component of the paper and, as per my specific comments below, needs significant re-working to take into account the uncertainty in the observed gridded data, which I suspect is substantial over this region. Reply: Author thanks the reviewer for this positive comment. In the present paper analysis and discussion is limited with model comparison with the observed station data only to establish the fact about model behavior and digression from the actual. No comparison with corresponding reanalysis is made as they are very coarser. In addition reanalysis digress much over the mountainous and have huge uncertainty in the precipitation and temperature fields. These changes are planned to be referred and mention in the revise manuscript. More reference are included in the revised manuscript.

Comment: I would also have liked to have seen more models/ensemble members being considered, including the latest CORDEX (S_ASIA) simulations. However, as a test of three RCMs the sample used is probably sufficient if the reasons for selecting these models are more adequately explained. Reply: As reviewer mead a mention of CORDEX-South Asia experiment – Author’s group has worked with 11 suite of regional climate models under CORDEX-South Asia experiment and has shown huge uncertainty in precipitation field in a separate paper which is under revision. Change made: For reference this paper is provided as a supplementary material for reference to the reviwier as this work is still under consideration in another paper. **SPECIFIC COMMENTS** Comment: p. 6252, line. 8. I suggest stating here that three RCMs are tested in this paper (and maybe naming them). Reply: Name of all the three experiment is provided at referred place in the revised manuscript.

Comment p. 6252, line. 20. “primarily as they are data void...”. Please make it clear that you are referring here to a lack of observed data. It would also be useful here to refer to the work of Wiltshire (2014), who used HadRM3 to downscale the ERA-interim data over the Western Himalaya, due to a lack of sufficient density of observed data in the region. Reply: Integration of reference of Whiltshire (2014) is made in the revised

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manuscript in reference with the usage of ERA-interim data reanalysis to strength this viewpoint of data paucity.

Comment p. 6253, lines 1-8. It is worth including information in this paragraph about the wider field of bias-correction, which I consider this paper to fit into, although a correction procedure is not actually applied. Reply: Mention of bias correction is incorporated in the revised manuscript. Details of bias correction are discussed in brief which is likely to provide correct information on valley-ridge kind of topography at a much localized scale. Such correction will provide closer resemblance with the natural environment and the work of Thayyen and Dimri (2015) is referred to strengthen this view point.

Comment p. 6253, line. 16. There is no "Methodology" section in this paper. Change to "... details of these models is provided in Section 3" or similar. Reply: Author thanks review for this highlight., Yes, there is no such Methodology adopted in the present paper and hence modification/change of the title of the corresponding sub-head is made.

Comment p. 6253 lines. 18-19. More information is required here about the body of work conducted on the mass balance of the Siachen glacier. Like Upadhyay, 2009; Ganjoo and Kaul, 2009; Dimri and Dash, 2010 etc. Reply: More information on mass balance, climate change carried out by various researches is added in the revised manuscript to show work done by various researchers over the Siachen glacier.

Comment p. 6253, line 22. Please include the reason for using these models, and state that you are using two RegCM3 models and one version of HadRCM3. This information is clearly stated in line 18 on page 6255, but should be included earlier. Reply: Clarity on model used is specifically mentioned at this place. Brief elaboration is also made. Since it is very difficult to mention why only these model are used and hence this statement is not covered in the present paper-may be in future a separate study on which model is better over this region will be taken up. Rather it is other way-

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that model outputs from only these models are available and hence synthesis is mainly drawn from these model details. This issue is very briefly touched upon in the revised manuscript.

Comment p. 5256, line 5. Which version of the CRU data was used? Reply: Correct input and corresponding observation is made in the revised manuscript.

Comment p. 6255, line 6. "...uncertainty in the downscaled outputs". This sentence needs to be rephrased as it suggests that you are actually downscaling data in this paper, when actually you are referring to the high-resolution of the RCM data. Reply: Well, downscaling in the present context is meant to explain the dynamical downscaling from large scale initial and boundary conditions using regional climate models. Such simulation with RCM provides factors at finer resolution than the existing reanalysis. Changes made: Clarification in this regards and associated statement on uncertainty is modified in view of the reviewer's line.

Comment p. 6255, section 3.3. Information should be included here about the reliability of the observed data. The use of the three station series is novel in this paper, and more information is required about the reliability of these series is required. Also information about the reliability of the APHRODITE and CRU data for this region should be included. Reply: Issue of the reliability of precipitation reanalysis like CRU and APHRODITE is a big question. This in itself could be a different debate about the preparation of these synthetic fields using corresponding algorithm. Apart from this it is also important that how many station densities are taken into consideration while preparing these synthetic precipitation data fields. Changes made: A very brief on this debate is included in the revised manuscript. Again since it is in a subject itself not much deliberations are made as it is out of the scope of the present paper. May be a separate study dealing only with reliability of these reanalysis field over such regions will be considered as a future course of action.

Comment p.6256-6259. Throughout Section 4 more discussion is required about the

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potential reasons for the differences between the simulated data and the observations. These are listed in the paper's abstract but need to be worked into and expanded in the main body of the paper. Reply: Author thanks the reviewer for this comment. Comprehension on this detail is made in the revised manuscript under section 4. Changes made: Section 4 is expanded with more deliberation. Comment p. 6256, line. 3. "And hence observed precipitation bias..." This sentence is ambiguous. Are you referring to this bias arising from limitations in the reanalysis forcing? Reply: Primarily this bias is mentioned with reference to the limitation in the corresponding reanalysis forcings as rightly pointed out by the reviewer. However this sentence is subtly modified in the revised manuscript. Changes made: Sentence is deleted.

Comment p. 6256, lines 12-29. This paragraph needs complete re-writing. On first reading it appears that two conflicting statements are being made about the reliability of the RCM data compared to the APHRODITE observed data. On one hand it is stated that the RCM data very successfully capture the inter-annual and inter-monthly variability shown in the observations, and on the other hand reservations are expressed about the usefulness of the RCM data. I disagree that the results show similar variability at inter-annual/inter-monthly timescales in the RCM compared to the observed data. There is some weak correspondence evident at the inter-annual and inter-monthly timescales, but nothing that can be considered similar to the observations, and this is only a qualitative assessment. I consider the most important finding of this section, which is described towards the end of the paragraph, to be that while both the RCMs and observations show a wetter winter and drier summer, they have significantly different variability amplitudes - the annual amplitude in the RCMs is of the order of 5-6 mm/d, while the observations are <1 mm/d amplitude. This needs to be stated more clearly, and probably also presented in a different manner than the contour plots. Also, monthly totals of precipitation may be considered for inclusion in this analysis. Furthermore, the results from the other RCMs tested need to be shown. Uncertainty in the observed data for this region also needs to be included. Consideration should also be given to inclusion of a comparison against the results of Ménégoz et al. (2013). Reply:

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This paragraph is modified as per the reviewer's instructions and guidelines. Changes made: Section 4 (4.1 and 4.2) is provided with broader discussion. Additional figure for precipitation and temperature are included and corresponding deliberation are made. Menegoz et al and Wiltshire reference are made at apt places.

Comment: p. 6257. Figure 2c. The results from the HadRM3 simulation should also be included in this figure. Also, why do the series "flat-line" at 0.08% frequency - does that relate to the 1mm threshold used? Reply: Flat line corresponds to the threshold value of >1mm and hence the flat line limits to that flat line. Changes made: Corresponding comment of threshold is mentioned in the revised manuscript. Since there is limited accessibility of HadRM3 model simulation now and hence HadRM3 model simulation and corresponding averages over the winter period could not be incorporated.

Comment: p. 6258, Figure 3a. I think it is important in this figure to include the average of the CRU data over the winter period (i.e. the absolute values from which the anomalies were calculated). This will provide information to the reader on the spatial variance and hence reliability of the CRU data. It is also important to include a plot showing the density of stations used in the CRU data, and the results from the HadRM3 data. Reply: Average plots from CRU are bring provided in the revised manuscript. Corresponding dissuasion is also added in the revised manuscript at suitable place. Changes made: Average of CRU, APHRODITE and corresponding model simulation is provided in Fig 2. Corresponding discretion in detail is provided in section 4.1. Since there is limited accessibility of HadRM3 model simulation now and hence HadRM3 model simulation and corresponding averages over the winter period could not be incorporated.

Comment: p. 6260, line. 2. Please provide more information on the slope lapse rate for readers not familiar with this work. Reply: Recent paper of Thayyen and Dimri (2014) is referred in the revised manuscript for general readership. Changes made: The concept of slope rate provided here is to include the land slope effect when dealing with valley-ridge microclimatic systems. It is seen that free atmospheric lapse rate is not detrimental when studying the physical processes in the valley-ridge systems.

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Driving mechanism from the adjacent valley and slope govern land flux exchange in controlling the meteorological factors.

Comment p.6265, Figure 1. Figure 1b is of poor quality. Please replace with a better quality image. Reply: These figures are scanned from maps to provide a bit on geographical comprehension for understanding. Author tried level best to improve upon its resolution. Changes made: Included

Comment p. 6258, Figure 4. Please include the altitudes of the stations in the plot, to prevent the reader having to refer back to Figure 1. Reply: Corresponding elevations are provided in the Figure 4 itself. Changes made: Included

REFERENCES Ménégoz, M., Gallée, H., and Jacobi, H. W.: Precipitation and snow cover in the Himalaya: from reanalysis to regional climate simulations, *Hydrol. Earth Syst. Sci.*, 17, 3921-3936, doi:10.5194/hess-17-3921-2013, 2013. Wiltshire, A. J.: Climate change implications for the glaciers of the Hindu Kush, Karakoram and Himalayan region, *The Cryosphere*, 8, 941-958, doi:10.5194/tc-8-941-2014, 2014 Reply: These references are included in the revised manuscript. Changes made: Included

Interactive comment on The Cryosphere Discuss., 8, 6251, 2014.