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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 #2

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This paper is unfortunately very difficult to follow and generally of poor quality and ill conceived. This is a reflection of its poor English and lack of a coherent structure, as well as fundamental concerns as to the methodology applied. It also reflects a lack rigour.

1.. There is a definite lack of coherence/structure making the paper very confusing to read. It is often unclear whether the author is referring to novel work presented in this paper or to existing work. This concern is particular applicable to the abstract (which is completely unintelligible), but is also evident by the author starting the beginning of the Results sub-sections with a 'preamble'. (Or in the inclusion of important information in the middle of Section 4.1 on the lack of representation of key physical processes in the models which should have been included in the section describing the models).

Section 4 is actually labelled 'Results and discussion' which sums up the way this section was written. Indeed, much of the material included in the results section should have been placed in the Introduction. Overall, my impression was that the paper was unstructured and rambling. The paper was also marked by both poor and careless English throughout, e.g. 'Particularly over mountainous regions RCMs have proven to well represent regional climate at mountain and even at event scale' or 'In HadRM3 experimental strategies, simulations for a continuous 18 year period from 1990-2007 were made'.

2. The description of the models used, the observations, and the methodology is incomplete and poor. The description of the models used in Section 3.1 is careless and incomplete. Numerous abbreviations are used which are never explained. It is very unclear what the three RCM simulations are, and what the 'SUB' and 'CONT' experiments are, i.e. the 'SUB' experiment is defined as '10 km with subgrid scheme'. There is no information (either here or in the Introduction) as to the previous performance of these models in the Himalayas. Section 3.2 is similarly indecipherable, discussing a 'fine scale BATS scheme' which is/isn't used in the models without explaining what this scheme is.

3. I believe the experimental methodology of comparing model fields of precipitation and temperature to global atmospheric reanalyses or so-called 'observational gridded data sets' of precipitation is flawed. These datasets have significant biases over complex orography such as the Himalayas, e.g. how can a global reanalyses dataset of spatial resolution $\sim$100 km resolve the temperature/precipitation gradients evident in mountainous regions. The other important point is that the paucity of in-situ measurements over these regions make such datasets highly unrepresentative of such regions. This is precisely why satellite derived estimates of precipitation from TRMM are so important for this region as they are able to better quantify precipitation and its strong dependency on both location and altitude. I therefore simply don't think that such datasets have any merit. Fig. 2b for example shows precipitation values of 1 mm/day

for the region based on the APHRODITE dataset, when other studies such as Winiger et al. (2005) based on in-situ measurements suggest precipitation amounts for the Western Himalayas of 1000 – 3000 mm/year. In any case, such concerns are barely considered in the paper as the datasets and their limitations are barely explained. The overall impression is therefore of a distinct lack of rigour.

Winiger et al., Karakorum-Hindukush-western Himalaya: assessing high-altitude water resources, Hydrological Processes, vol. 19, 22329-2338, 2005.

4. Regarding the observations, there is no information as to what was measured, when the measurements were taken, etc etc.

5. With regard to comparing the model output against temperature, I again view the methodology displayed in Figs 3 and 4 of comparing against either CRU or ERA-Interim datasets as worthless. In any case, again there is seemingly no understanding of the limitations of these datasets. I admit that I am more familiar with ERA-Interim than CRU, but in both cases there was no information about what these datasets are and their representativeness in this region.

6. I find Fig. 5 and its description in Section 4.2 rather puzzling. The author comes to the conclusion that 'the model environment is colder at the surface and warmer at mid atmospheric levels' which is untrue.

7. The conclusion is as confusingly written as the abstract, and decidedly not up to scratch.

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