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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 #2 Received and published: 23 February 2015 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.

Reply: It seems annoyance of the reviewer is mainly to do with author's non-native English and hence presentation of there off. And seems, in a way, agreed with the question framed by the author. Objective of the paper is not at all ill-conceived and is important with ongoing concurrent research and efforts thereon. There are similar

works tried and put into with the modeling framework to assess how the modeling efforts can be integrated with in the glacier research and this is clearly a cut out of that dimension. Having said so there is definitely rigor and new thought over this presented paper and hence author totally respectfully disagrees with the reviewer's view point. It would be much appreciable if reviewer could have provided straight 'scientific' comments on the level of work to strengthen and enhancing the work along with simply viewpoint on English, edits, structure etc. Change made: In the revised manuscript, based on the first and present reviewer, many changes are incorporated. In addition inclusion of some of the figures has brought in larger perspective of the research.

Comment 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'.

Reply: Possible understood restructuring in the manuscript is done. It is novel work and presented and framed first time with specific reference to the glacier and associated studies. Certain changes in the abstract are made in tuned with new discussion in the revised manuscript. No changes in section 4.1 are made, as it is more apt to discussion lack of model processes in the corresponding results and discussion rather than in the model configuration. Model configuration is also elaborated in the revised manuscript. Some of the redundant portion of the results and discussion has been deleted and Introduction as well is corrected as far as English is concerned. Changes made: Other necessary corrections are made in the revised manuscript. English of the paper is modified it to the best of the author's level.

Comment 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.

Reply: Description and model details are provided in the revised manuscript. Relevant explanations for various abbreviations are also included with corresponding reference. Specific information on SUB and CONT is also provided and explained. Similarly information and previous study pertaining to the BATS scheme and its reference are included in the revised manuscript. Changes made: Above mentioned comments by the reviewer are included in the revised manuscript.

Comment 3. I believe the experimental methodology of comparing model fields of precipitation and temperature to global atmospheric reanalyzes 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 âĹij100 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.

Reply: It is an interesting observation reviewer raised. Here we are downscaling global analysis with the help of RCM and then looking into the precipitation and temperature aspects that how good and/or bad they are. Many researchers have supported this idea of downscaling over the region where observations data is still an issue. And particularly over such areas as of Siachen glacier thus such study becomes important-if downscaled data could provide important information definitely with certain uncertainty. In view of this, then this study becomes very important. It is true that global analyses having coarser resolution can really not been able to define the precipitation and temperature gradients-but RCM outputs could provide a basic understanding based on the physical processes involved. Again available in situ measurements as shown in comparisons clearly give the above insight. Thus these findings are really provided added information in that dimension. And specifically above fact supports that there is definitely merit in such study leading to glacier research involving modeling efforts. As far as Winiger et al. work is concerned that work is specifically focusing on Karakorum region (central and south asia) which has altogether different regional climate. And it is with more focus on hydrological issues and note climate. However, it can be an issue of debate that how much actual precipitation is occurring over this region. And also in which form solid, liquid and/or mixed. Since focus of this paper is not to debate on observations or not to debate on observations problems and thus are not highlighted in the revised manuscript.

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

Reply: Relevant information on in situ observations are provided in the revised manuscript. Author thanks the reviewer for this comment.

Comment 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.

Reply: Author completely disagrees with the reviewer. It is very much worth to draw such comparison and analyze them in view of providing synthesis of such utility for glacier studies. There are very many works published which are using RCM as a tool and explaining various processes and associated dynamics.

Comment 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.

Reply: Corresponding section is updated in the revised manuscript.

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

Reply: Well author tried level best to synthesize the result in a coherent manner and hopes that the reviewer agrees to these changes.

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