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Interactive comment on "Snowfall in the Himalayas: an uncertain future from a little-known past" *by* E. Viste and A. Sorteberg

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The manuscript by Viste and Sorteberg is timely and addresses an important question: How do the water resources in the Himalaya change in the coming decades. There will be large interest among several scientific communities on this topic because of the far-reaching consequences of snowfall in the high-elevation regions and the high population density in the downstream areas. It is important that the science is sound and solid.

Overall, the manuscript is well written and has useful figures. The introduction is very well done, and presents an up-to-date and broad overview of climate science in the region. One improvement would be the addition of absolute values for precipitation in the different regions mentioned (pg 443, lines 15-20), as the authors simply mention

that 'meltwater is important in the otherwise dry spring', and then group 'the monsoon-dominated central Himalayas and the Tibetan Plateau' into the same summer-fed precipitation regime. It is somewhat misleading to only talk about annual percentages when the absolute amounts are so drastically different, as well as quite different topographies.

I don't think the debate is quite as settled on the changing strength of the monsoon as they predict on pg 447, lines 10-17. This citation (Ramanathan, V., et al. "Atmospheric brown clouds: Impacts on South Asian climate and hydrological cycle." Proceedings of the National Academy of Sciences of the United States of America 102.15 (2005): 5326-5333.) notes the possibilities of extra black carbon/smog reducing sea surface temps and thus reducing water availability. I am not sure how well this is accounted for in the CMIP models, but would be an interesting thing to discuss.

There are a few key issues that should be properly addressed before this manuscript is published: (I am not trying to be picky here, but try to address some of the key issues of the manuscript. The spatial-temporal resolution and topographic relief of this areas is a challenging factor for every researcher in this area!) (1) Correction factors for MERRA data. Greater attention should be given to the corrections used on the MERRA data, as talking about a 'topographic correction' as simply one line is not sufficient. Downscaling this data is quite complex, and a simple elevation correction is unlikely to improve the data.

(2) The Dai 2008 study that forms the basis of the authors' snowline determination was tuned over land stations which are not representative of the terrain in HMA. As there are quite limited stations at high elevations, this Temp/Pressure snowfall gradient should at least include error bars, which could/should be propagated into their results and discussion. There should also be further discussion of how they treat snowfall permanence, or if they only look at instantaneous snowfall by a temperature/precipitation value per month. For example, snow is more likely to stick and accumulate if the temperature over the following few days is below freezing, rather than at 0-1.2C which are

within the 'snowfall threshold' but are unlikely to lead to permanent snow. As the snow must last through some of the season to be helpful in the 'dry seasons', it might be better to consider non-permanent snow as 'rain', as it will not contribute to late-season runoff.

(3) On pg 451, lines 1-3, the authors discuss an elevation-derived temperature correction, which is downsized from two atmospheric temperatures, neither of which is a LST. The T2 discussed is also simply stated as the temperature at 'the next level'. A better explanation of how this correction was derived is needed, as well as a discussion of what global elevation grid was used in MERRA and how this differs from GLOBE.

(4) Pg 452, lines 15-22, where their distribution mapping procedure is described. I am not convinced that correcting one biased dataset with two other biased datasets will improve results. Especially by using APHRODITE as a correction factor, and then later comparing the MERRA data to the APHRODITE data. Issues with TRMM snowfall should also make this correction somewhat suspect.

(5) Along the same lines: TRMM 3B42 data are mostly rainfall data and do only partially include snowfall. Measuring snowfall with IR remote-sensing technology is very tricky. I suggest to carefully consider this point and add some caveats in the text. As an addition to this point: Please check the usage of precipitation – I think there are some cases where rainfall would be more appropriate.

(6) Pg 453, section 2.4. It seems that their rain-snow line model doesn't account for topographic factors such as relief or aspect, which may have a strong control not only on type of precipitation, but also on how long the snow remains snow. This is probably very hard to correct for, however, and may be impossible at this data scale.

It may help to show the relation between relief and slope as compared to shits in snowline. It seems that the authors posit that steep topography will be less affected by temperature changes, although this may simply be an artifact of how they calculate the rain-snow line. It could make sense that steep topography will be somewhat insulated

from climate shifts. A figure may help to elucidate this.

Figure 2 really needs scale bars for each bar graph, as it is very hard to compare the values when they are not on a single x axis, but are instead floating in space.

Figure 8 and 9: I am wondering if it makes sense to only show the model means here. I have a difficult time deciphering between the different models because most of the lines are on top of each other. Figure 11+12 are very data rich and useful, but difficult to read. Is there a way to split up the figure to enhance readability?

Interactive comment on The Cryosphere Discuss., 9, 441, 2015.