

Snowfall in the Himalayas: an uncertain future from a little-known past

Reply from the authors to B. Bookhagen

Thank you for taking the time to read our manuscript! We found your comments very valuable and have used them to improve both the text and several of the figures.

Comment 1: *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 monsoondominated 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.

Response 1: Good point! We have added a paragraph with absolute numbers, as described below.

Text change in response to Comment 1: On page 443, we have added a paragraph before line 15, and merged the two paragraphs on line 16–30. The result reads:

“Precipitation varies greatly between inner and outer parts of the Himalayas (Singh et al., 1997; Bookhagen and Burbank, 2006; Winiger et al., 2005). While there are regions in the Himalayan foothills and along the Himalayan ridge with an annual mean rainfall of more than 4000 mm, most of the Tibetan Plateau on the leeward side receives less than 500 mm (Bookhagen and Burbank, 2010).

The Indian summer monsoon creates markedly different seasonal cycles in eastern and western parts of the HKH, both in precipitation and in the accumulation of snow and ice. In the monsoon-dominated central Himalayas and on the Tibetan Plateau, more than 80 % of the annual precipitation falls during summer. Precipitation maxima in the western regions occur in connection with westerly disturbances in winter. In the Hindu Kush and Karakoram, as well as in the easternmost Himalaya, summer precipitation amounts to less than 50 % of the annual precipitation (Bookhagen and Burbank, 2010). The seasonal cycle of snowfall varies accordingly. In the western HKH, snow accumulates during winter, while the summer is the main melting season. Further east, the summer is the main season, not just for ablation, but also for accumulation (Rees and Collins, 2006). According to Bookhagen and Burbank (2010), the east-west gradient and the effect of the summer monsoon is most pronounced in the lowlands, below 500 m a.s.l., while the difference is less at higher elevations. “

Comment 2: *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.

Response 2: We agree that the question of how well the CMIP models are able to reproduce precipitation and changes in precipitation in the region is open. As both our references and our Figure 8 (9 in the new version) show, there are large differences between the models, though the model mean is positive. The reviewer's point about aerosols is highly relevant, and we have changed the text based on a more recent reference to ensure that the reader is aware of that. The article we refer to, Guo et al., (2014), is currently a discussion paper in Atmospheric Chemistry and Physics, available at <http://www.atmos-chem-phys-discuss.net/14/30639/2014/acpd-14-30639-2014-discussion.html>. We would have preferred to see a final version of it published before referring to any details, and have thus made a rather general statement in the text.

Text change in response to Comment 2: Lines 18–26 on page 447 have been changed to:

“It should be emphasized that there is a large inter-model spread in precipitation projections. Guo et al. (2014) found that CMIP5 models with a more realistic representation of aerosols had a more negative impact on the monsoon than models that include only the direct effect of aerosols on radiation. Overall, the IPCC AR5 concludes that there is medium confidence in the increase in summer monsoon precipitation over South Asia (Christensen et al., 2013). But although precipitation projections are less reliable than temperature projections, agreement between models increases with time and anthropogenic forcing (Chaturvedi et al., 2012). Also, the CMIP5 multi-model mean has been considered to represent the monsoon and the actual climate in India better than any individual model (Chaturvedi et al., 2012; Sperber et al., 2013).”

Comment 3: *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 area 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.*

Response 3: We have used the MERRA 2 m temperature and atmospheric lapse rate to downscale the temperature to a higher-resolution terrain grid. We do not see any reason why such a correction, based on a more realistic elevation together with the atmospheric temperature gradient, should not improve the data. However, from the reviewer's comments, we realize that we have not described our procedure clearly enough. Thanks for pointing that out for us! In the updated version of the manuscript, we have described this more clearly. We have also added a cartoon showing the data used, as well as the difference between MERRA and GLOBE terrain. We would like to point out that the elevation correction has been performed on temperature data only. For precipitation we agree that elevation-based corrections would probably not add any value, and we have not attempted to correct precipitation based on the terrain.

Text change in response to comment 3: A new figure (2) has been added. On page 451, line 2 and the following paragraph has been changed to:

$$T_{adj} = T_0 - \frac{\Delta T}{\Delta z} \Delta z_0 = T_0 - \frac{T_2 - T_1}{z_2 - z_1} (z_{merra,0} - z_{globe}), \quad (2)$$

where T_0 is the MERRA 2 m temperature, T_1 is the temperature at the lowest pressure level above the ground, T_2 the temperature at the next pressure level, and z_2 and z_1 the height of these levels. $z_{merra,0}$ and z_{globe} are the elevations of the MERRA and GLOBE topography, respectively, and Δz_0 the difference between them. The variables are illustrated in Figure 2. The procedure combines the vertical temperature gradient in MERRA with the MERRA 2 m temperature and the elevation difference between MERRA and GLOBE. We have assumed that the most representative temperature gradient ($\Delta T/\Delta z$) for this purpose, is that of the MERRA layer nearest to, but not touching, the MERRA ground.”

Comment 4: *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.*

Response 4: Dai’s function was based on weather reports from 15000 globally distributed synoptic land stations. We realize that the results are not tuned to the Himalayas, but are not aware of any better alternatives. The only realistic alternative would be to use a constant threshold between snow and rain. In either case, this is not likely to be a major source of error, compared to errors in the input data of precipitation (and temperature). This can be seen from the values in Table 4 in Section 3.2, but was not discussed in the original version of the manuscript. We agree that this should be done and have added a paragraph describing the results.

Text change in response to comment 4: The following paragraph has been added in Section 3.2 (page 455, after line 22):

“The MERRA reference snowfall deviates about 10 % from the original MERRA reanalysis snowfall; negatively in the Indus and Brahmaputra, and positively in the Ganges. Two effects contribute to this: the use of elevation-adjusted temperature, and the use of the function from Dai (2008) when relating precipitation type to temperature. The effect of the function may be seen from the ‘MERRA T2m’ in Table 4. For this variable, the Dai function was applied directly to the MERRA 2 m temperature, i.e., without the elevation adjustment. Comparing this with the original MERRA reanalysis snowfall (‘MERRA’) indicates that the Dai function acts to reduce the snow fraction. The elevation-adjustment of temperature depends on the MERRA vertical temperature gradient, as well as the topography of MERRA and GLOBE. GLOBE is the result of merging various other elevation data, and the quality in each region depends on the available input data. Globally, half of the data points have been estimated to have a vertical accuracy of less than 30 m, whereas some points in Antarctica may be as much as 300 m off (Hastings and Dunbar, 1999, 1998). The effect of elevation-adjusting the temperature, or of using the Dai function, each amounts to changes in the order of 5–20 %. This is much less than the effect of bias-corrections with observation-based data.”

Comment 5: *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.*

Response 5: We find it hard to disagree with the reviewer that the final result of value to dry-season hydrology is the accumulated snow at the end of the season. But the aim of this study is to look only at the first-stage input to accumulation and snowmelt analyses: precipitation falling in the form of snow. We have not considered whether the snow melts within a short time or remains frozen for months. One of our points is to show the uncertainty in precipitation and snowfall values; uncertainties that will then be carried on if using these data sets as input to melt models.

Our monthly sums are added up from values calculated for each hour. This is described on page 450, line 7–8, and with the display of monthly results. To make this clearer, we have added information about the monthly sums in the subsequent paragraph (result shown below).

Text change in response to comment 5: On page 450, a sentence in line 11–12 has been changed to:

“The results were then aggregated to monthly sums for the Indus, Ganges, and Brahmaputra Basins.”

Comment 6: *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.*

Response 6: More details of the elevation-adjustment procedure were surely needed and have been added, as described in Response 3. The figure added there (the new Figure 3a) also shows an example of the difference between MERRA and GLOBE. We have also added information about the accuracy of GLOBE in the paragraph added in response to Comment 4 (see Text change in response to Comment 4).

Comment 7: *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.*

Response 7: The reviewer is not convinced that bias-correcting MERRA with observation-based data sets will improve the results. Neither are we. Rather, it demonstrates the wide range of “ground truths”. Our point in doing this is to estimate the possible range of snowfall, based on data sets that are widely used by the scientific community. Seeing the large differences between the data sets, we decided not to judge whether one is better or worse than the other, rather just show the possible snowfall estimates one gets by using each set. And we do consider it likely that the ensemble represents upper and lower bounds for the true snowfall values.

Comment 8: *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.*

Response 8: We are aware of the limitations of the TRMM data when it comes to representing snowfall and have discussed this in lines 6–13 on page 457. Considering the under-catch of snow by traditional precipitation gauges, the problem also applies to the observation-based CRU TS and

APHRODITE. The likely under-representation of precipitation in these data sets is discussed in Section 3.2 (line 18 on page 456 – line 13 on page 457). We understand the reviewer’s concern about the use of the word precipitation vs. rainfall, but as TRMM includes some snowfall, we have chosen to keep using the word precipitation. We note that the reviewer, in Bookhagen and Burbank (2010), though generally using the term rainfall, also refers to TRMM 3B42 as an estimate of precipitation.

Comment 9: *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 shifts 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.*

Response 9: That is correct: Our rain-snow line definition is based on temperature only. Apart from elevation, we do not consider the effect of topographical features. Steepness is not considered and we cannot see that we mention that anywhere. We agree that a more sophisticated definition would be valuable, but do not see how we could produce such a measure with the available data.

Comment 10: *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.*

Response 10: We have added scale bars to each subplot in the figure, both in the form of vertical axes and as marks crossing the bars at each 100 mm.

Comment 11: *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.*

Response 11: We agree that it was hard to distinguish between the lines in these figures, though we think it’s important to show all the data. Showing only model means would hide the uncertainty represented by the spread in individual model projections. We have made new versions of the figures, with both mean values and individual models represented by horizontal marks.

Comment 12: *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?*

Response 12: We agree that the figure is complex. However, we would prefer not to split it up, as it would then be difficult to compare the subplots. As it is, precipitation, snowfall and rain-snow line values may be compared on a monthly basis, by reading the figure from the top to the bottom. If we split up the figure or changed the arrangement of the subplots, the reader would lose that possibility. As a total, that would make the arguments we make in Section 4.2 more difficult to follow.

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