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Interactive comment on "Climate change implications for the glaciers of the Hindu-Kush, Karakoram and Himalayan region" *by* A. J. Wiltshire

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This manuscript uses two Global Circulation Models to estimate changes in the glacial mass balance in High Mountain Asia (or the Hindu Kush, Karakoram, Himalaya region). The author relies on a modified version of a GCM to understand present-day and future changes in the hydrologic regime with special emphasis on glacial realms. One of the main conclusion is that the spatial pattern of changes is complex and highly variable across the Himalaya orogen (which is not surprising given its size and longitudinal and latitudinal range).

The manuscript is an important step in linking large-scale (or regional) climate analy-

ses within the cryospheric realm. While the used glacial metrics are simple, they are likely to provide good first-order views and should show the expected spatial pattern. My main concern is related to the model output and model validation. As stated below in more detail, I suggest including a 'caveat' or 'model limitation' section that clearly outlines the limits of the GCM. I am not questioning the validity of the approach, but it would be of great help for researchers working in this region to see some uncertainty bounds. Specifically, I am surprised by the present-day (1992-2007) climate outputs. The Summer Monsoon precipitation doesn't closely resemble ground or satellite measurements (but I can see the big pattern in the data and that is mostly correct). Putting these uncertainties aside, my main concern is the snowfall amount and timing. During the past 15 years of field work and data-collection efforts in the western and central Himalaya (mostly at high elevation), I have not noticed a strong summer snowfall component (cf. Putkonen et al., 2004). However, in this manuscript the GCM output predicts significant snowfall amounts (see region HP). If there is snowfall during the ISM, it is limited to small areas at high elevations and not a wide-spread phenomenon (but there is some snowfall during summer). Again, it would help putting some uncertainty constraints on the results – I am fully aware that capturing snowfall in GCMs is difficult and requires careful assessment.

A second general point is that the manuscript would benefit from a clear definition of geographic space. The author uses east and west, but for example it often remains unclear if it is the eastern Himalaya or the eastern HKKH. In the result section of the manuscript, the author uses five clearly identified regions (previously used by Kaeaeb et al.) and this works very well. However, the intro and climatic baseline section do not contain these distinctions.

The manuscript is well written and I have only minor wording suggestions.

Please see more extensive comments below:

3719, Line 20: I am uncertain if this statement is true. This area has a large flow com-

ponent of snowmelt waters (cf. Bookhagen and Burbank, 2010; Jeelani et al., 2012; Vuyovich and Jacobs, 2011) and has bee argued to have a small glacial component (∼2% by Jeelani et al., 2012). I suggest to rephrase this statement.

3720, Line 21: True, but it should be pointed out that most of these studies (as well as others) are based on a limited dataset of ground-control station. All of these data are highly valuable and very important, but can we really draw regional conclusions from a handful of stations > 5km asl elevation?

3722, Line 2: You don't really capture the complex topography with 25-km gridcells (but this is still better than 0.5 degree grid cells). I suggest to rephrase this to point to a moderate spatial resolution that will help to better resolve the complex topography (but still is not fully adequate). Modeling studies using WRF from the Rocky Mountains in the realm of SNODAS indicate that you need 2-4km spatial resolution to capture snowfall distribution in complex terrain (cf. Clow et al., 2012, but also publications by Barlage and Rasmussen). This is not required for this study, but highlights the spatial constraints.

Line 2: The key here are western disturbance (or winter westerly or winter weather patterns) that lead to excessive precipitation in the northwestern Himalaya/Karakoram. The Indian summer monsoon has a diminished role in providing precipitation for this region.

Line 9: There are, in fact, at least 3 climatic systems: The Indian and East Asian Monsoons and the winter disturbances. The ISM and EAM are not necessarily linked, but EAM influences the eastern Tibetan Plateau and the headwaters of some of the eastern catchments (cf. Bookhagen and Burbank, 2010).

Line 13: Not only altitude, but also latitude for the Karakoram (consider solar radiation as a potential driver).

3723, Line 18: Are glaciers in the eastern (and central ?) Himalaya really only ac-

cumulating during the summer? The eastern Himalaya (especially the region around Namche Barwa) receives significant winter precipitation as well. Figure 1 also indicates only moderate fraction of summer monsoon rainfall in the eastern Himalaya. This confusion may stem from a lack of a clear geographic definition.

3724, Line 9: zero degree C

3725, Line 1: high-spatial resolution simulations Still, I am wondering if you want to call this 'high resolution', given the fact that other models run at ∼10 km or less spatial resolution. It is important to point out that the resolution is high for the domain considered.

Line 11: Past climate? You are referring to the past decades not the Holocene/Pleistocene, right? Rephrase to avoid confusion.

3726: Line 3: area averages.

3726 and 3727: The model caveats are very important. True, modeling the ISM is tricky. What about the models ability to predict snowfall? This is clearly a challenge and I am wondering about a quality assessment. Part of the problem may arise from the fact that a 25-km grid cell gives you only the average elevation in a pixel and not the few high-elevation peaks that will effectively capture storm system and receive large amount of snowfall. This section could either be expanded or a separate caveat section could be introduced where the GCM limitations are clearly stated. This is very important to understand the implication of this work. For example, can we trust predicted changes in rainfall to the same degree as predicted changes in snowfall and temperature?

3727, section 3.2: Very important summary. Can be made easier to read by breaking it up into geographic regions with different headers or at least in different paragraphs. Again, this section would tremendously benefit from a 'caveat' or 'model limitation' section beforehand.

Figures:

Figure 2: It should elevation (m asl) and not orography. Orography is the study of the formation and relief (and not the altitude itself).

Figure 3: This is important. Please make sure that labels are clearly readable. I suggest to use m/yr as units I am somewhat surprised to see very high summer-snowfall accumulation in the central Himalaya. I haven't observed these during the past 15 years doing fieldwork in various parts of the western Himalaya (Himachal Pradesh) and central Himalaya (Nepal). Station data from the central Himalaya (e.g., Putkonen et al., 2004) indicate only little snowfall during the summer monsoon.

Figure 6: I have real troubles reading and deciphering this figure. First, there are too many overlapping lines. Second, this figure is too small. Wouldn't it be better to show anomalies (or differences) instead of absolute values? (Same for Figure 7) Nevertheless, I question the snowfall peak during the summer in HP in Figure 6. There is really only a small fraction of the landscape that can receive snow during that time. The figure suggests that the snowfall magnitude is comparable to the winter season and that is questionable.

Interactive comment on The Cryosphere Discuss., 7, 3717, 2013.