Reply to Anonymous Referee #2

We would like to thank the reviewer for their evaluation of our manuscript and the helpful and constructive comments. Please find below our replies to the individual comments (reviewer comments in italics).

1. *The paper is well structured. However, some explanations are not in corresponding chapters. For example, the section 2 describes the study site and data. In this section all available data is explained, except for MODIS and Landsat snow cover area data. In my view the processing of Landsat data and short description of MODIS data belongs to this chapter rather than chapter 4.*

   We agree that the structure of the sections describing the utilized data could be confusing to the reader. However, in the current version of the manuscript in section 2 only the data required for driving the model is described (DEM and derived variables, meteorological data, initial ice thickness distribution), whereas all of the validation data is described in section 4, hence in our opinion the manuscript is consistent in that sense. To express this more clearly, we have renamed section 2 from "Study site and data" to "Study site and model input data" and section 4 from "Validation approach" to "Validation approach and data". However, if the referee and/or the editor feel that all data should be described in a single chapter, we would be happy to restructure the manuscript accordingly.

2. *Without reading Strasser, 2008 for model description or being familiar with AMUNDSEN model, my concern is with groundwater module and its contribution to total discharge which is not discussed in this manuscript. Is the groundwater discharge marginal in study site so it can be neglecting in analyzing the model components? In figure 15, the authors compare total observed discharge with the simulated discharge. Is the fraction described as "unglaciarized" in this figure correspond to groundwater component? Maybe the authors can state it clearly if this is the case.*

   The fraction described as "unglacierized" does not correspond to groundwater but rather to snowpack outflow and liquid precipitation in unglacierized areas (whereas the "snow" reservoir only corresponds to snow on glaciers). Groundwater discharge is in fact not explicitly considered in the model, as the runoff module was originally developed for catchments which are for the most part glacierized. For these types of catchments the linear reservoir concept is, despite its simplicity, often sufficient for glacio-hydrological investigations. Groundwater discharge is thereby often assumed as constant (e.g., Asztalos et al., 2007; Escher-Vetter, 2000). However, for larger and less glacierized catchments the performance of this approach decreases, which can also be seen in the results presented in our manuscript. For future work it is hence foreseen to improve the runoff concentration scheme currently implemented in the model.

3. *The description of cold content and liquid water content is not well understandable. Do you mean by cold content the solid water content? If this is the case, maybe it makes sense to call it this way as this is widely used in literature than "cold content".*

"Cold content" in our terminology corresponds to the energy required to warm the entire (dry) snowpack to 0 °C, which is a definition commonly used in the literature (e.g., DeWalle and Rango, 2008; Marks et al., 1999; Singh and Singh, 2001). By relating the energy with the latent heat of fusion of ice (333.7 kJ/kg), cold content can alternatively also be expressed in units of water equivalent, corresponding to the equivalent amount of liquid water needed to release the required amount of energy by freezing. For the sake of consistency and comparability with the amount of snow water equivalent and liquid water content, we use the latter formulation (expression of cold content in mm w.e.). We have clarified this in the updated version of the manuscript, and added a more detailed description of the cold content and liquid water content parameterization to the article supplement.

4. *Page 7, lines 11-13: The openness values of study site is computed using zenith angle and nadir angle values. Where do you obtain these values?*

   Zenith and nadir angle values are computed according to the definitions given in Yokoyama et al. (2002) on the basis of the 50 m model DEM: Given a grid point A, a radial distance L (in our case, 50 m and 5000 m), and an azimuth D, the elevation angles θ are calculated for all grid points B along D which are within a horizontal distance L of A:

   $$\theta = \tan^{-1}\left(\frac{H_B - H_A}{P}\right), \tag{1}$$

   with H_A and H_B the elevations of A and B, and P the horizontal distance between A and B. The maximum elevation angle ${}_D\beta_L$ is then defined as the maximum value of θ, and vice versa the minimum elevation angle ${}_D\delta_L$ as the minimum value of θ. Finally, the zenith angle is calculated as

   $${}_D\phi_L = 90 -_D \beta_L, \tag{2}$$

   and the nadir angle as

   $${}_D\psi_L = 90 -_D \delta_L. \tag{3}$$

   Resulting openness values are calculated as the mean of the zenith and nadir angles for all eight compass directions. We have added a reference to the definitions given in Yokoyama et al. (2002) to the corresponding section in the manuscript.

5. *The validation approach with "observation scale" was not clear to me (without reading Blöschl and Sivapalan, 1995). Maybe a hint for authors to better explain this in the manuscript.*

   We agree that without further information the scale concept might not be immediately clear to the reader. We have added a figure illustrating the concept to the manuscript.

6. *Page 11, lines 15-18: If one scene shows snow cover and the other snow free, then the pixel was considered as snow. Please give reasons why the scene with snow covered should compensate the scene without snow cover?*

   The approach to assign snow-covered pixels preference over snow-free pixels is similar to methods applied in other studies (e.g., Gafurov and Bárdossy, 2009; Gao et al., 2010; Xie et al., 2009). There, Terra and Aqua images are merged according to a priority principle (with the prorities in descending order being snow > ice > water > land > cloud > polar), where lower priority pixel values are replaced by the higher priority value in case of disagreement. However, for our study area in the period 2000–2013 this issue (disagreement in land/snow classification between Terra and Aqua) occurs on average only in 0.9 % of all pixels per day (whereas 51 % of the pixels are cloud-covered in both images, 33 % are cloud-free and in agreement, and 14 % are cloud-covered in only one of the two images), hence impacts on the further analyses due to the choice of this methodology are likely very small. We have elaborated on this issue briefly in the updated manuscript.

References

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