

General comments

In their revised manuscript and response the authors have successfully addressed the vast majority of my comments and concerns from the first round. As such, I only have a few technical corrections and very minor suggestions outlined below that I am confident the authors will address relatively easily. I would like to congratulate the authors on this work that would be a welcome contribution to *The Cryosphere*.

Specific comments

- L46: I was surprised to see the introduction of the term "*regional circulation model*" since it is completely new to me and seems to be uncommon in the literature. I would thus suggest that the authors switch this to "*regional climate model*". If you are worried about consistency you can also switch "*general circulation model*" to the (roughly) synonymous "*global climate model*".
- L85: The word "*parameter*" gives the impression of something that is more static than the dynamic properties that together describe the seasonal snowpack, such as snow depth, SWE, SNC, and SNCM. I would thus encourage the authors to instead refer to these as "*variables*" throughout the text. This would also be in line with the GCOS use of the term essential climate variables (ECVs) among which many snow variables feature.
- Figure 1: I do not recommend representing the number of stations as bar plots which (in my opinion) are rarely a good idea. The same information would be more clearly conveyed as line plots. This would also make the snow depth line plot more visible.
- L101: Be more specific here and change "*infrared*" to "*shortwave infrared*" since snow is still quite reflective in the near infrared. Consider also adding a reference to Dozier (1989) who was (to the best of my knowledge) the first to suggest the NDSI (without calling it that).
- L102 Change "*indicates*" to "*often indicates*", otherwise you are overlooking false positives (i.e. NDSI > 0 when FSCA=0) which, despite being less frequent than true positives in cold regions, do occur.
- L108 For the advantages of spectral unmixing with higher resolution sensors I would recommend a reference to Cortés et al. (2014) as well.
- L118: Although it is perhaps implicitly obvious, consider adding that the CMIP6 version could be considered in future studies.
- L141: Change "*is temporal*" to "*is a temporal*". Delete the second sentence here ("*It limits...*") since there are many other applications of this data that are limited by polar night. If you want to keep it, say "*such as*" or provide a few more examples.
- L147: Thanks for clarifying the model resolution. As I understand it you are focusing on modeling the seasonal snowpack, yet nowhere does it state that you masked out glaciers in the observations (both MODIS and SNCM). I suspect this masking is done somehow, otherwise you would have a representativeness error (see e.g. Janjić et al., 2018) in your observations, but this needs to be stated somewhere in the manuscript.
- L151: This suggests that the SNOW-17 algorithm was coded in GEE by Anderson (2006), which is of course not the case. Instead, you could just change this to "*The SNOW-17 algorithm (Anderson, 2006)...*"
- L155: Change "*parameters must*" to "*parameters that must*".
- L159: Change "*as followed as it incorporates*" to "*were followed as they incorporate*".
- L162: Clarify what "*10 × 10 DEM*" means in this context. I guess you mean 10 m resolution?

- As a side comment on Table 1, it is laudable that you seek to justify your choice of parameters with references and ancillary datasets. Nonetheless, this exercise is somewhat misleading as these are effective parameters that compensate for the physical processes that are missing in a degree day model. As such, they do not really have universal values that can be extracted from look-up-tables (or similar) based on ancillary data. Instead, their "optimal" values will likely vary considerably based on the forcing data used and other details of the experimental setup such as the spatial resolution of the model. My issue is that this presentation may (inadvertently) camouflage the uncertainty that exists in these parameters rather than embracing it and attempting to calibrate them with the data (e.g. MODIS snow cover) that you have at hand. This becomes especially apparent when one of two references in Table 1 is from a conference abstract, was there no subsequent peer-reviewed publication by these authors that goes into more detail on their methods? In summary, although I understand that calibration is beyond the scope of this work, the uncertainty introduced by the particular choice of parameters should at least be mentioned somewhere in the manuscript.
- L174: This sentence is problematic. I don't see why published datasets would be less uncertain than derived datasets that seek to make improvements. Please change the formulation here. It is fine to say that for simplicity and to stay within the scope of your study you used these datasets without further manipulation.
- L180: To me at least, this is not an explanation of what your p-values measure. Instead, you have merely passed the problem along to the term "*95% confidence level*". There are plenty of definitions floating around, see e.g. Ambaum (2010) or Benjamin et al. (2018), and I would urge the authors' to adapt the wording in these to their particular tests. The reason I recommend this is that null hypothesis significance testing plays a central part in your results (the term significant is mentioned 4 times in Section 4 alone), so I believe it is important that you properly define what is meant by this term.
- L182: Change "*ensemble average*" to "*ensemble mean*".
- L270: Please use \times not $*$ for multiplication when using scientific notation, i.e. $p = 1.54 \times 10^{-5}$.
- L277: Since you are using "*significant*" in a very specific technical sense elsewhere, I would recommend changing this (different) usage of the word to "*considerable*" or similar.
- L290: Unsure how helpful it is to introduce the acronym CESM2 which you then never use elsewhere.
- Table 3 (and elsewhere): I am not sure why you are citing Hall et al. (2006) since you are using the V006 MODIS snow cover product not the V005 product. The correct citation is surely Hall et al. (2016) throughout the manuscript. Also consider following the suggested citation for Hall et al. (2016) on the [product webpage at NSIDC](#) from which yours deviates slightly.

Kind regards,
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References

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- Benjamin, D. et al.: Redefine statistical significance, *Nature Human Behaviour*, <https://doi.org/10.1038/s41562-017-0189-z>, 2018.
- Cortés, G., Giroto, M., and Margulis, S.: Analysis of sub-pixel snow and ice extent over the extratropical Andes using spectral unmixing of historical Landsat imagery, *Remote Sensing of Environment*, <https://doi.org/10.1016/j.rse.2013.10.023>, 2014.
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- Janjić, T. et al.: On the representation error in data assimilation, *Quarterly Journal of the Royal Meteorological Society*, <https://doi.org/10.1002/qj.3130>, 2018.