The Cryosphere Discussions

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Review

Anonymous Referee #3

Referee comment on "Snow model comparison to simulate snow depth evolution and sublimation at point scale in the semi-arid Andes of Chile" by Annelies Voordendag et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-9-RC3, 2021

The manuscript "Snow model comparison to simulate snow depth evolution and sublimation at point scale in the semi-arid Andes of Chile" by Annelies Voordendaag et al. discusses the application of two snow models at a site with an automatic weather station in a dry mountainous area in the Chilean Andes. The snow models provide mass balance components, which is of importance for investigating water resources in such areas. The model simulations suggest a very strong sublimation flux to the atmosphere, depleting significant amount of snow mass on the ground. By perturbing model settings and forcing data, uncertainties are quantified.

Generally, the paper is well written and the topic is highly relevant, and the approach by including two snow models and doing the sensitivity study by perturbing model settings and forcing data is very solid. This makes for excellent ingredients for the manuscript. However, it is surprising to see how poorly the models are able to reproduce the snow cover at the site. This deserves some more attention, because in my opinion, the agreement is so poor, that the trust I have in the simulated mass balance components is also severely limited.

Intuitively when looking at the results and the discussion by the authors, a problem could be that the place is so wind affected, that the models underestimate density. That could explain the much stronger settling in the simulations than observed. As the authors discuss themselves, snow density is an important factor for sublimation, since the snow surface temperature depends on it. So I wonder if it is maybe a better approach to run the models with a fixed fresh snow density of let's say 350 kg/m^3, to see if the agreement improves? Or also to analyze the combined SWE increase and snow depth increase to get an estimate of the fresh snow density? This value could be compared to the parameterizations from SNOWPACK and SnowModel (add observed density from SWE/SD to Fig. S6.1 for example). It is definitely an aspect that is not sufficiently discussed in the current manuscript where the discrepancies between models and observations come from. It's an almost more interesting aspect of the study that the models are apparently very poorly able to capture the processes at this site.

I'm also somewhat confused that the total SWE in the models is so much underestimated. The models roughly produce ~175 mm w.e. in sublimation/evaporation and ~100 mm w.e. runoff. So the total mass input to the models (around ~275 mm w.e.) is less than the maximum SWE observed at the site (300-350 mm w.e.). The maximum SWE is about half in the simulations than what is observed. In L129-123, authors discuss that a precipitation reconstruction based on the SWE time series overpredicts total mass, but I don't find any compelling reason presented why an underprediction from the precipitation gauge is preferred over an overestimation from the SWE reconstruction. Furthermore, I think generally undercatch corrections vary so much, and are often found to be site-specific and setup-specific, that I think authors should take some freedom to improve the undercatch correction for this specific setup.

Specific comments:

- L255-258: this is not a correct description of how the default version of SNOWPACK works. Unless the authors modified the source code specifically regarding this, SNOWPACK will only use air temperature to distinguish between rainfall and snowfall when driven by a precipitation time series. This wouldn't alter the SWE response, unless runoff occurs. The other criteria are only used when SNOWPACK is driven by a snow height time series.

- The introduction may need citation of the recent SnowMIP study, with some recent publications:
> Krinner, G., Derksen, C., Essery, R., Flanner, M., Hagemann, S., Clark, M., Hall, A., Rott, H., Brutel-Vuilmet, C., Kim, H., Ménard, C. B., Mudryk, L., Thackeray, C., Wang, L., Arduini, G., Balsamo, G., Bartlett, P., Boike, J., Boone, A., Chéruy, F., Colin, J., Cuntz, M., Dai, Y., Decharme, B., Derry, J., Ducharne, A., Dutra, E., Fang, X., Fierz, C., Ghattas, J., Gusev, Y., Haverd, V., Kontu, A., Lafaysse, M., Law, R., Lawrence, D., Li, W., Marke, T., Marks, D., Ménégoz, M., Nasonova, O., Nitta, T., Niwano, M., Pomeroy, J., Raleigh, M. S., Schaedler, G., Semenov, V., Smirnova, T. G., Stacke, T., Strasser, U., Svenson, S., Turkov, D., Wang, T., Wever, N., Yuan, H., Zhou, W., and Zhu, D. (2018): ESM-SnowMIP: assessing snow models and quantifying snow-related climate feedbacks, Geosci. Model Dev., 11, 5027-5049
> Menard, C.B., Essery, R. Krinner, G., Arduini, G., Bartlett, P., Boone, A., Brutel-Vuilmet, C., Burke, E., Cuntz, M., Dai, Y., Decharme, B., Dutra, E., Fang, X., Fierz, C., Gusev, Y., Hagemann, S., Haverd, V., Kim, H., Lafaysse, M., Marke, T., Nasonova, O., Nitta, T., Niwano, M., Pomeroy, J., Schädler, G., Semenov, V., Smirnova, T., Strasser, U., Swenson, S., Turkov, D., Wever, N., Yuan, H. (2020): Scientific and human errors in a snow model intercomparison, Bull. Amer. Meteor. Soc. 1–46., doi: 10.1175/BAMS-D-19-0329.1

- Section 2.2: please specify if the rain gauge was heated or not.

- L97: I assume that the full period between 23 June 11:00 and 31 October 10:00 is missing for TA and RH, and not only those two specific times.

- Why was only one year studied while the measurement site has operated for a much longer period (installed in 2009 apparently)? Please also discuss to what extend this year 2017 is representative for the climate at the site (i.e., compare with the full data set in terms of temperature, precipitation and wind speed).