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The Cryosphere Discussions

Interactive comment on "A long-term dataset of climatic mass balance, snow conditions and runoff in Svalbard (1957–2018)" *by* Ward van Pelt et al.

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Van Pelt et al. present a multi-decadal modeling study regarding snow and glacier mass balance on Svalbard that yielded results on a so-far unprecedented level of detail with respect to model resolution and captured processes. I very much congratulate the authors to this very thoroughly performed, documented and discussed modeling study which provides extremely valuable new knowledge to the field of Svalbard-wide glacier and snow research. I have no severe concerns regarding publication of this article. However, in its present form, the model description lacks a couple of important details that need to be added to the descriptions in order to make the methodology easier

to follow. In this respect, three substantial issues need special and more extensive attention, including limited additional data analysis. Taken together, I recommend to accept the manuscript of Van Pelt et al for publication in The Cryosphere after a minor revision along the issues outlined below.

Substantial comments:

1) P4L30f (& P9L16ff): I understand that you first linearly interpolate your 10km HIRLAM precipitation grid to the 1km resolution of your model. So far so good. However, in the next step you describe the application of a fixed linear fractional increase with elevation that you apply in addition. This step causes some concerns. I assume that the 10km elevation information in HIRLAM are not based on the S0 Terrengmodel Svalbard that you use in your mass balance model, right? This means that the average of the 1km elevations in your model across each 10km HIRLAM grid point and the elevation of this HIRLAM grid point itself do not equal each other. If this is the case, it introduces a physical inconsistency. Depending on the area altitude distribution of the 1km model grid points within each 10km HIRLAM grid point you either increase or decrease the total amount of precipitation that falls within this grid point by applying a fixed linear precipitation increase. Hence, the precipitation amounts which had been modeled by HIRLAM in a way that is physically consistent to synoptic forcing, are altered completely by your downscaling scheme. Moreover, this happens completely unstructured with respect to space, as the degree of alteration is only determined by the differences between the means of the 1km model elevations and the 10km HIRLAM elevations. I'm not sure if my interpretation above is what really happens; it could have also been a simple misunderstanding of your descriptions. In any case, I'd suggest that you comment on this issue in detail in the uncertainty discussion and/or revise your descriptions in the methods section accordingly to make them unambiguous in this respect.

2) P8L11ff: You implemented two novelties in your model. While the first one, the physically based percolation scheme, is fully referenced, the second one is not. How

were the parameters of your newly incorporated albedo scheme chosen? If you use a new or updated scheme, then you need to include information about how it was calibrated or how it is justified from a physical point of view. As you have various AWS data available I suppose that you could easily validate your new albedo scheme with shortwave radiation measurements from these stations. I do not ask for including an additional figure on this (even if it would be nice to have), but at least you should provide some comparative albedo numbers to validate the novelty in your model, especially its ability to produce a reliable course over the year. Your Svalbard-wide distribution of albedo values may also be compared to those modeled and described for 1979-2015 by Möller & Möller (2017) on the basis of MODIS data. This would yield important insights into the reliability of your so-far unreferenced albedo scheme.

3) P8L25ff: The application of RMSE minimization for finding the optimum values for K1 and K2 is certainly valid. However, I'd like to point towards a potential problem. In case the stake readings that are used as reference are not distributed equally with elevation, the RMSE minimization might not yield a proper combination of K1 and K2. This is because elevations with the highest number of stake readings are overrepresented and the minimization procedure thus concentrates on getting the accumulation at this specific elevation well while paying less attention to the other, underrepresented elevations. This issue has been detailed and documented for stake-based calibrations across Svalbard before by Möller et al. (2016) and you should at least account for it in the text. However, it would be better to check it in detail in order to avoid potentially wrong gradients or scaling coefficients that might lead to substantial under- and/or overestimation of climatic mass balance towards higher elevations. My concern is backed by the fact that the winter balances in Figure 3 clearly show, that modeled values tend to systematically underestimate the measured ones the more positive they become. If you visually place a linear fit to the blue points, the line would have a slope that for sure is distinctly larger than 1. I do not think that this issue will compromise your overall results as it probably only affects the most positive mass balances. Nevertheless, it needs to be presented in the uncertainty discussions section.

Detailed comments:

P2L19f: The reference to Day et al. (2012) is misleading here. They only calculate changes in surface mass balance on the basis of seasonal sensitivity characteristics. They neither use a mass balance model nor do they calculate absolute Svalbard-wide mass balance numbers. Instead of Day et al. and to complete the ensemble of Svalbard-wide mass balance calculations a reference to Möller et al. (2016) is missing here.

P2L34f: The references to Hagen et al. (2003) and Winther et al. (2003) are completely outdated as numerous studies related to seasonal snow coverage on Svalbard have been published since then, partly even with contributions of one or several co-authors of this study here. Hence, there is a need to include more up-to-date references here (e.g. Grabiec et al. 2011 or else).

P4L27: Include information about which ECMWF reanalysis products are used and over which periods. Even if this is partly deducible from the references it needs to be explicitly stated in the text.

P4L30f: Far more, especially quantitative, information about the applied lapse rates, increases and decays is needed here. The reader must fully understand what has been done without digging into previous literature.

P5L1: How did you calculate the significance of the trends? Information about this needs to be included here.

Figure 2: Just out of curiosity (as it is out of the scope of your study): do you have an explanation for the rather interesting pattern of precipitation trends visible in (d). I especially refer to the east coast of Wijdefjorden here.

P7L8: This value seems to be reasonable as it is quite often used for the transition from snow to firn. However, the choice appears to be rather arbitrary in the present form of the text. Information about how this choice was made, including appropriate

reference, needs to be given here. Moreover, as you give explicit information about the density applied to remaining snow you should also do so for other snow cover to ablation conversions that you consider in your model.

Figure 5b: One might think about the color scale here. People tend to associate blue colors with cooler conditions, which means more positive mass balances in glaciological studies. However, in the current version of this figure, blue represents a negative trend and thus a development towards more negative mass balances. Maybe it would be better skip the in the first instance ambiguous blue-red color scale here in favor of something more "uncommon". But that's just a suggestion as it reflects a rather subjective view.

Figure 9: An additional map showing the distribution of the percentage of melt and rainwater that is refrozen should be added here and any inferable information should to be included in the discussion where appropriate.

Figure 10: Same "problem" with the color scale as in Figure 5b. But this is still only a suggestion.

P22L21ff: You describe the usage of a fixed DEM and fixed glacier mask as a potential source of uncertainty and error. However, in the beginning of your paper you explicitly state that you calculate reference surface balances. Hence, your results do not suffer any "uncertainties" or "errors" due to the usage of fixed glacier extents and elevations. They simply represent a completely different quantity that is not comparable to "real" climatic mass balances which would be based on a time-varying glacier topography. This needs to be made clear in this section. You could of course keep the given descriptions, but treat them as deviations to what really happened on the glaciers and not as "uncertainties".

P23L7ff: The discussion of misestimations of precipitation fits to my substantial comment 3) in the beginning. The issue raised in this substantial comment needs to be included in this discussion, too.

P23L17ff: You might explicitly refer to the influences of wind-drifted snow, i.e. its potential to systematically increase or decrease local as well as regional accumulation rates. This information is certainly assumed in the sentence in question here, but it should be explicitly stated and referenced (e.g. Jaedicke and Gauer 2005; Grabiec et al. 2011).

References:

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Möller M and Möller R (2017), Modeling glacierâĂŘsurface albedo across Svalbard for the 1979–2015 period: The HiRSvaC500âĂŘα data set, J. Adv. Model. Earth Syst., 9, 404–422, doi:10.1002/2016MS000752.

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