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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.

Anonymous Referee #1

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GENERAL

This is an exceptionally thorough and robust modelling-based paper investigating the climate mass balance (CMB), which includes surface and subsurface processes, across Svalbard between 1957 and 2018. It builds on previous similar work by the team, esp. first-authored papers by van Pelt, but this is the first time the latest version of the model (which now includes an improved subsurface scheme based on Marchenko et al., 2017b) has been applied to the whole of Svalbard. The model has a 1 km grid and is run at a 3 hourly time step and is therefore impressive in terms of its spatial and temporal resolution. The CMB is driven by downscaled climate data from

the High Resolution Limited Area Model (HIRLAM) regional climate model, which is forced by European Centre for Medium Range Weather Forecasts (ECMWF) reanalyses. This generates the meteorological forcing fields of air temperature, precipitation, cloud cover, relative humidity and air pressure.

The work uses an extensive data set of measurements to calibrate / validate the model (mass balance stake measurements from 8 glaciers; weather station data from 6 sites (4 on glacier; 2 off glacier); and shallow ice cores from 4 sites). These are listed in Table 1.

The calibration procedure is clearly explained and is logical and the principles have been discussed in two previous referenced papers. Here, parameters that are known to be sensitive are calibrated in sequence as described in section 3.2: 1. Two parameters affecting albedo are calibrated against net SW radiation data; 2. Two parameters in a function describing the downscaling of precipitation are calibrated against winter stake bass balance data; 3. Two parameters affecting summer melt are calibrated with observed summer balance data.

The fact that this model has a good history of being used in Svalbard and the fact that the RMSEs and biases after calibration are small, mean that the results will be the best that are currently available.

Results presented are quite extensive and informative and, as the authors state in the abstract, should be of value for scientists and practitioners interested in runoff to the oceans as well as ecologists interested in, for example, snow extent, duration and character (which has implications for reindeer grazing, for example).

The results / discussion section is focussed around a sequence of Figures showing: i) maps of mean conditions across Svalbard; ii) maps of trends over time (where significant); iii) time-series of spatially averaged trends in conditions. The consistency in the way the data are presented make the paper especially useful. The following results are shows and discussed: i) glacier CMB (Fig 5); ii) glacier ELA (Fig 6); iii) glacier firn pore

space in top 14 m (Fig 7a,b); iv) firn temperatures at 14 m (Fig 7 c,d); v) refreezing on and off glaciers (Fig. 9); vi) snow onset and disappearance dates off glaciers and across glacier ablation areas (Fig 10); vii) glacier and land runoff.

This represents a particularly impressive range of data sets presented and discussed from this type of modelling study.

The paper discusses sources of uncertainty throughout and has a synthesis section on this towards the end (section 4.6). Where results differ from those of similar previous work (but using earlier versions of the model, calibrated in different ways, run over different time periods, and across different spatial domains) the magnitudes and reasons for the discrepancies are revealed. The results and implications of the Svalbard work are also discussed in the context of similar work where appropriate in Arctic Canada and Greenland; this is especially the case when discussing the important finding of decreasing refreezing rates over time and therefore an increase in the likelihood of firn aquifers developing around the ELA.

So overall this represents excellent work by this team and shows the value of long-term monitoring but also the collection of shorter-term field measurements and their rigorous use in model development and application.

The work is exceptionally well presented in terms of the overall paper structure, as well as the clarity and precision of the writing, but also in the consistency and quality of the Figures.

SCIENTIFIC QUESTIONS / COMMENTS These are relatively minor:

As mentioned above, quoting from the paper, the meteorological forcing fields used to drive the CMB model are: air temperature, precipitation, cloud cover, relative humidity and air pressure. The answer is probably elsewhere in previous papers but a brief note on how these are used (together with other fields I assume) to calculate energy / mass balance at the surface would be useful. For example, there is no mention of

wind-speed here, and yet I assume this is required together with air temp and relative humidity to calculate the turbulent fluxes? And I assume theoretical clear sky solar radiation is used together with cloud cover to determine the incoming SW radiation?

P8 L17-19. The Bougamont et al (2015) work is for Greenland. How do you know that parameter values derived for the GrIS for t* are valid on Svalbard. The final sentence refers to the work on the GrIS I assume. Given the importance of albedo for melt and mass balance etc, some clarity is needed here about the validity of using the parameter values relevant for GrIS here in Svalbard. Is this a source of uncertainty that needs better recognition?

P9 L33&34. It's stated that the parameter Tsr has a strong impact on summer melt but most previous work has shown it's particularly important for winter accumulation. I can see it'll have an indirect impact on summer melt because of its direct impact on winter accumulation. Can you better justify why this parameter is tuned to the summer mass balance data and not the winter mass balance data?

P22 L1-2. There is a bit of confusion here as you seem to be discussing runoff rates due only to snow melt on land and comparing them to runoff rates due to snow and ice melt across glaciers. But, as you say later, runoff from land includes rainfall. Does runoff from glaciers also include rainfall? A better articulation of precisely how runoff is calculated for land and for glaciers is needed before the two values are compared. Can you separate out runoff from snow(ice) melt from runoff due to rainfall?

TYPOS / TECHNICAL ISSUES

Abstract P1 L4. Could say: "climatic mass balance (CMB) for the glaciers, snow conditions and runoff. . ."

L8. Suggest "small" not "weak"

P2 L4. "reveals" not "reveal"? The Longyearbyen time-series is singular not plural?

P2 L19. Could add the following reference to this list of previous studies here:

Rye, C.J., Willis, I.C., Arnold, N.S. and Kohler, J., 2012. On the need for automated multiobjective optimization and uncertainty estimation of glacier mass balance models. Journal of Geophysical Research: Earth Surface, v. 117,

P4 L21 “altitudes” (i.e. plural)

P5 Table 1. Table is not quite self-contained. Suggest adding to Table Heading and referring to Fig 1 heading for abbreviation names. Also to explain variables or say they’re explained in the text.

P5 L10. Could add ref to Table 1 after final sentence here.

P6 L8 suggest “made” not “done”

P7 L5. Suggesting adding months when end of summer measurements are typically made (like April is stated earlier in the sentence for when Spring measurements are made). I’m guessing this is August or September (since 1 Sept. is stated as an average time below)?

P7 L5 Could delete “above described”

P11 L4 and Table 2. The term ‘bias’ is introduced here and referred to as “modelled minus observed”. There are different definitions of bias so it might be worth clarifying precisely how it’s defined here. Is it simply the Mean Absolute Difference (MAD)?

P11 L29. “five” should read ‘six” here I assume? There are 6 sites mentioned in Table 1 and 3.

P11 L32. “. . .temperatures for both. . .”

P13 L11. Should this say “net CMB” to distinguish it from winter or summer that are also reported? Could clarify the first time you refer to net CMB, e.g. say “net CMB, hereafter just CMB. . .” or some such. In Abstract you might then also add the word “net”?

P17 L25-27. There is also some similar work to this reported recently from the Larsen C ice shelf, Antarctica that could also be compared / referenced. e.g.

Hubbard, B., Luckman, A., Ashmore, D.W., Bevan, S., Kulessa, B., Kuipers Munneke, P., Philippe, M., Jansen, D., Booth, A., Sevestre, H., Tison, J.L., O’Leary, M., and Rutt, I., 2016. Massive subsurface ice formed by refreezing of ice-shelf melt ponds. Nature Communications, 7.

Bevan, S. L., Luckman, A., Hubbard, B., Kulessa, B., Ashmore, D., Kuipers Munneke, P., O’Leary, M., Booth, A., Sevestre, H., and McGrath, D. 2017. Centuries of intense surface melt on Larsen C Ice Shelf, The Cryosphere, 11, 2743-2753.

P20 L3-5. There is a lack of clarity here. Here and the few sentences above need to better distinguish between a discussion of snow onset date and snow disappearance date. There’s ambiguity here as it seems as though you might be comparing the trend in onset date (+1.4 days / decade) found in this study with trends in BOTH the onset date AND the disappearance date in a previous study. There is a bigger discrepancy in the disappearance date trends in the two studies than there is between the two onset date trends, and this probably needs stressing and discussing. I wouldn’t say a disappearance date trend of +0.7 days / decade is comparable with 0 days per decade.

P23 L34. I think this should just read “. . .simulation, using the climate forcing. . .”.

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