Response to R. H. Giesen (Referee)

(original comments are in black, our response in *red and italic font*)

General comments:

This paper describes the application of two subsurface snow models of different complexity to the Col du Dôme accumulation zone. An extensive set of subsurface measurements at this site allow for a detailed validation of the model results. The energy and water balance model illustrates the main processes taking place in the firn column during melting events. The simplified approach allows for the simulation of longer time periods and may be applicable to other locations, although recalibration is needed. The combination of measurements with modelling approaches and the vulnerability of cold accumulation zones to climate change make this a valuable paper. The manuscript is well-written, but the methods need clarification at some points. I have several comments and suggestions to further improve the paper, please find them listed below.

Specific comments:

5544,23-5545,1: At what height are the measurements done? I later found out that this is listed in Table 1, please include the reference to this table already here.

Done

5546,6-12: The SEB model provides input (heat and water) to the second model, but includes a simple heat diffusion model without the important effects of refreezing melt water to determine Q. This may cause considerable errors in the calculation of Q and Qm . Why have the two models not been coupled, such that the effect of refreezing can be taken into account in the SEB? Was this not possible? Please mention this here.

It would have been possible to couple both models but we do no think that is neither necessary, nor consistent. Indeed, the effect of refreezing is already accounted for in the SEB model. As specified page 5547, lines 23-25, all the energy used for surface melting is then released in the first underlying cold layer below the surface to simulate water percolation and refreezing. We suppose that this simple way to simulate water percolation and refreezing is accurate enough to calculate Q and Qm, because refreezing is indeed taken into account. There is a good agreement between measured and modeled temperatures with the SEB model at different depths (figure A), which gives confidence in the calculation of Q.

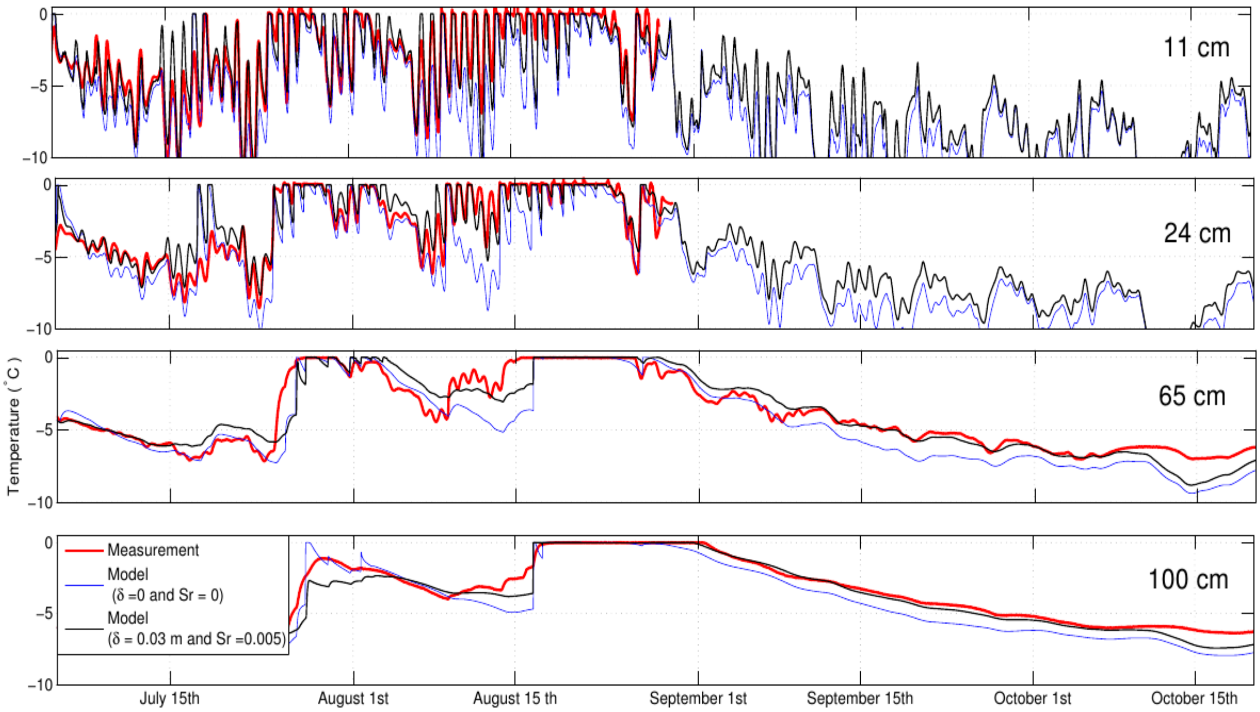

Figure A– Measured and modeled temperature at four different depths using SEB model with and without taking into account the solar radiation penetration.

5546,16: I can understand that heat added by precipitation is very small at this cold location, but neglecting the penetration of solar radiation should be better motivated. Are you sure this will not significantly affect the thermal profile in the firn?

In order to quantify the impact of the penetration of solar radiation, we have performed new simulations with model 1 taking into account this effect and the water content retained by capillarity forces (Sr) into the snow layers that can also affect both the temperature profile and the surface energy balance. The penetration of solar radiation was modeled assuming that short-wave radiation exponentially decreases as a function of depth [Colbeck, 1989]:

$$F_{Sw}(z) = (S_w\downarrow - S_w\uparrow) * \exp(-z/\delta)$$

where F_{sw} (W m^{-2}) is the radiative flux at depth z (m) and δ (m) is the characteristic length of penetration (m). We assume δ not to depend on the wave length.

Taking into account these processes does indeed change the surface energy balance. The roughness length z_0 had to be tuned again to match the measured firn internal energy variation (Figure 5 in the manuscript). figure B shows values of z_0 as a function of δ and Sr that allows to match the measured firn internal energy. Associated modeled melting and RMSE for surface and subsurface temperature are also plotted. This shows that taking into account both these effects improves the RMSE for subsurface (RMSEΔ_{Tsub} and RMSEΔ_{Ts}) and surface temperature. The best RMSE is obtained for Sr = 0.005 (good agreement with model 2 study) and δ=0.03 m (in agreement with Brandt and Warren, [1993]). However, those values for δ and Sr imply a somehow too high value of 15 mm for z_0 [Brock et al., 2006]. However, looking at the bias between modeled and measured surface temperatures (Figure C) suggests that δ should be comprised between 0.02 and 0.025 m. Selecting the minimal RMSE for $\Delta_{Tsub,}$ gives a value of Sr = 0.005 and an acceptable value for z_0 of 4 mm. Therefore, those parameters have been used for our simulation.

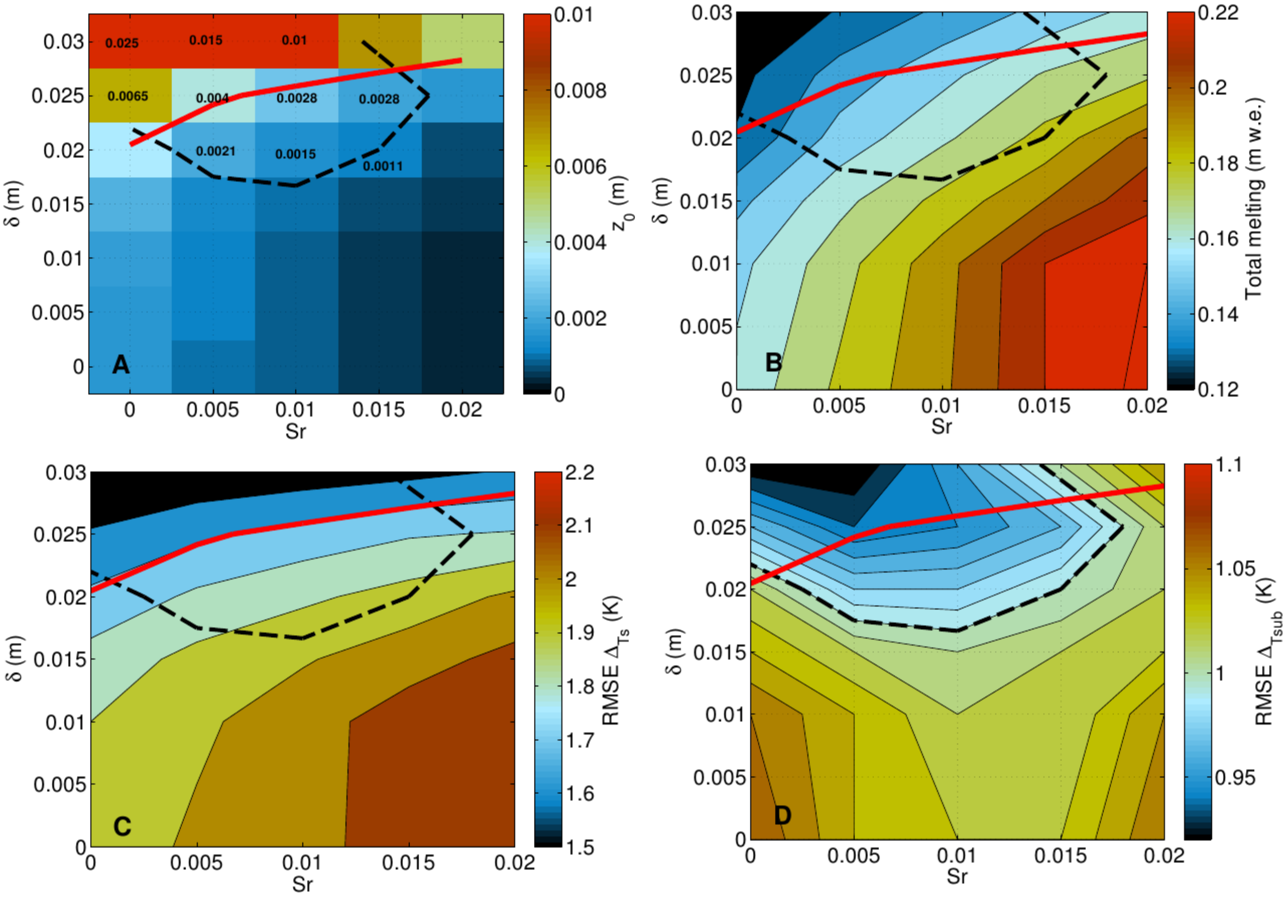

Figure B – (A) Roughness length (z_0) as a function of Sr and solar characteristic length of penetration (δ) that allows matching the measured firn internal energy variation (figure 5 of the manuscript). (B) Associated modeled surface melting as a function of Sr and δ. (C and D) Associated RMSE between measured and modeled surface and subsurface temperature. Red line is the curve bias between modeled and observed surface temperature equal to 0 (see figure C).

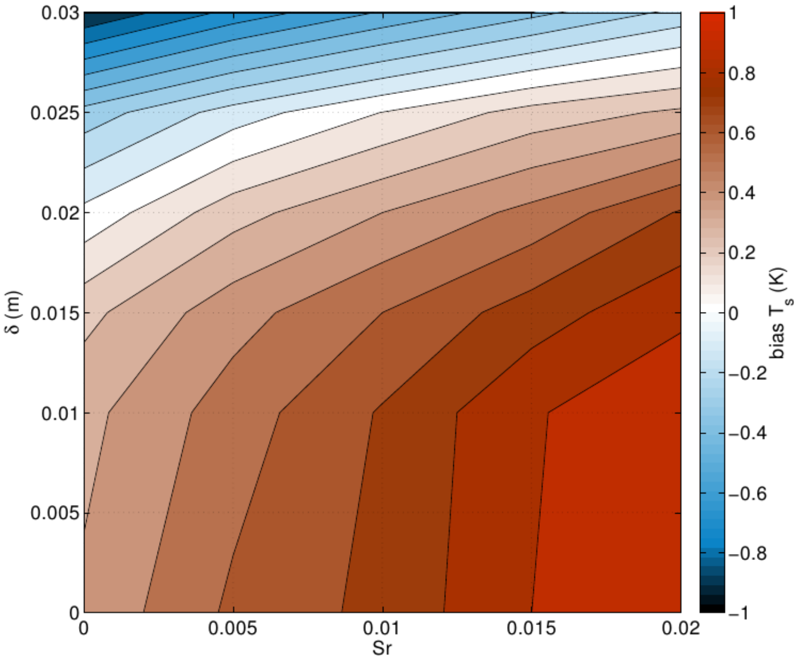

Figure C – Bias between modeled and measured surface temperature as a function of Sr and δ

Accounting for solar radiation penetration in the surface layers clearly improves the modeling of subsurface temperature (see figure A). Modeled surface melting is also in better agreement with the measurements (see figure 3 in the revised manuscript).

Finally, this analysis shows that melting was overestimated for compensating the fact that energy supply due to radiation penetration was not taken into account. The impact of irreducible water saturation in snow also affects the surface energy balance and thus has to be taken into account. When there is no melting, the cold bias between measured and modeled subsurface temperature is compensated by taking into account the radiation penetration that clearly influences the temperature profile.

5547,6-8: Please give some more information about the turbulent flux calculation, instead of only referring to another study. Does the approach include corrections for the stability of the boundary layer and how are the different roughness lengths calculated/defined?

More details are now given in the manuscript. The approach includes corrections for the stability of the surface boundary layer and all roughness lengths (momentum, humidity and temperature) are assumed to be equal. Since z_0 has been tuned to match the variation of the internal energy variation, selecting a single value for z_0 or using distinct values for every roughness length (momentum, humidity and temperature) would have changed the values of the roughness parameters, but would not have changed the final results of our simulations. As a consequence, z_0 should be considered more as a tuning parameter than as a true roughness length.

In 5550,22 I read that the roughness length for momentum is tuned, is the same value used for the other roughness lengths?

Yes, see revised manuscript

5547,18: Which density data, are these the near-surface densities mentioned in 2.2.3?

Densities were measured into the borehole where the thermistors were installed. Explanations have been added in the revised manuscript.

5548,4: What is meant with ‘homogeneous snow’? Does it mean that the density is the same everywhere, or only constant for every layer in the model? What density value(s)is used? This should be mentioned, preferably together with the initial temperature profile (5550,1-2).

Densities are considered constant over time and thus set to the measured density profile. This is now mentioned in the revised manuscript.

5549,24-5550,5: What is the vertical resolution of this model? Is it applied only at the location of the weather station?

The vertical resolution varies from 10 cm near the surface to 40 cm at 16 meter depth. This model was only applied at the location of the weather station. This is now clearly specified in the revised manuscript.

5550,14-16: The wind speed record in Fig. 2 appears to contain at least two periods without data. Have the mean wind speed and the dominant wind direction been calculated using only periods with data or has some kind of interpolation been used? What wind speeds are used to calculate the turbulent fluxes in the SEB model for the periods without measurements?

There is only two periods between July 12th and August 6th and between October 7th and 12th without data for wind speed. During these gaps, the mean wind speed measured over the whole period with data has been used to calculate the surface energy balance. Explanations have been added in the revised manuscript in section 4.1.

5551,4-5: What density value is used to express the SR50 record in m w.e.? Is it the 380 kg m−3 mentioned a few lines further down? Then please move this sentence.

Yes density of 380 kg m^{-3} is used. The sentence has been moved up.

5551,7-11: I do not think this argument is convincing. The slow reduction of the surface height after September 10 could result from settling of the snow, although the speed seems to be higher than after the previous snowfall event. The largest drop in surface height occurred around September 22, when wind speeds were not particularly higher than during other parts of the measurement period. The strong winds the authors refer to were measured at the same time as the snowfall and cannot cause the large surface drop more than a week later. Did the wind perhaps blow from a different direction? A few days after the large drop in surface height, the surface height is back at the same height as before the drop, was there another snowfall event? If not, can the drop perhaps be the result of a misinterpretation of the SR50 measurements? More generally, did the authors correct the SR50 readings for the air temperature between the sensor and the surface? Since the sensor assumes a zero degrees Celcius air temperature to determine height changes, measurements at different air temperature should be corrected, especially when the distance between the sensor and the surface is large.

SR50 measurements have been corrected for the air temperature between the surface and the sensor. We agree that reduction of surface height most likely results from settling after September

1st and September 10th. However we cannot explain the drop in surface height on September 23th and 24th. It may have been due to wind ablation considering that wind speed was probably underestimated during this period due to the presence of hoar on the anemometer as observed on October 4th, a few day after (figure 4). The manuscript has been modified accordingly.

Figure 4 – Picture of the anemometer showing the presence of hoar on October 4th.

5551,17-18: What is meant with the cumulative surface energy balance, how is it defined? I find it hard to understand that a balance between fluxes can have a value of its own, is it one of the fluxes or a sum of several fluxes? In Fig. 5 this term is called Modeled energy input, perhaps this is a better term to use?

We agree, we now use "modeled energy input" everywhere in the manuscript.

5552,5-28: This is an interesting experiment and provides increased understanding of the processes. However, I suggest to move the lines with the motivation (22-28) to the beginning of the paragraph to make the purpose of the comparison directly clear to the reader.

Done

5553,25-27: A comparison of measured and modelled snow/firn temperatures shows that especially at the depths of 24 and 65 cm, the modelled firn temperature is significantly underestimated around 10-15 August, just before the start of the major melt event. Do the authors have any idea of the cause for this large discrepancy? Does it affect the amount of modelled melt?

The new simulation performed by taking into account solar radiation penetration clearly attenuates this discrepancy (see figure A). During this period we have the evidence of sub-surface melting due to solar radiation penetration. Indeed, in the figure 3 of the manuscript, you can see that, during that period, melting is occuring in snow (0°C zone in subsurface temperature measurement) whereas surface temperature stays below 0°C. Taking now into account radiation penetration significantly improves the simulation.

5556,27: How has the atmospheric transmissivity been determined, from the AWS measurements? Has one value been used for the entire period or have daily/half hourly values been used?

A mean value has been used over the entire period and this value has been determined by comparison of measured short wave radiation during clear sky with calculated theoretical potential solar radiation.

5557,14-25: I understand that the authors aim for a very simple relation between temperature, potential solar radiation and melt, but the current formulation does not have a physical basis. Especially because through the fit of aPSR , PSR ends up in the relation as a squared quantity as well. A comparison with at Eq. 1 shows that PSR is included in R ($R = (1 - \alpha)PSR+L$ with L net longwave radiation), while L and the turbulent fluxes are an approximate function of $T_{max} - T_0$. So a simplified relation of the form $M = (1 - \alpha)PSR + (T_{max} - T_0)b$ would be more appropriate, in my opinion.

Yes, we agree. We tried to calculate melting using this more physical formulation but it does not change the result. Consequently,we decided to keep the Hock [1999] formulation.

5558,22-25: Why not use the values derived before, do they not give satisfactory results? I suggest to first mention the melt factors derived from the PSR values (as given in 5559,5-7) and also show the profiles calculated with these values. If necessary, the melt factor values that give the best match with the observations can be provided as well.

As suggested, we are now first mentioning the melt factors derived from the PSR values in the revised manuscript. Modeled profile plotted in figure 11 use then those melt factors. We still stress in the revised manuscript that the best match with observations for site 3 would require a different value of the melt factor.

5559,19-22: As mentioned before, I have problems with assigning values to the SEB. Alternatively, you could write that the sum of the radiative and turbulent fluxes becomes more positive.

We agree. Changes were done in the revised manuscript.

Fig. 4: I am not sure what the authors exactly mean when they refer to the energy flux balance. In Fig. 4 they show this last term, which presumably are the terms in Eq. 1 other than the radiative and turbulent fluxes, so $Q_m - Q$? What is the physical interpretation of this term, why is it referred to as the energy flux balance? Would it not be better to show these two terms separately? Furthermore, Fig. 4 does not seem to be discussed in the text, is it necessary to include?

Yes, here, energy flux balance is the sum of radiative and turbulent flux, i.e. $Q_m - Q$. It is now clarified into the manuscript and the figure 4 has been deleted.

<u>Technical corrections:</u>

5542,17: ‘the surface temperature reaches’ or ‘surface temperatures reach’

Done

5543,21: ‘by the Dirichlet’

Done

5545,12: ‘half-hourly’

Done

5545,16: ‘characteristics’

Done

5547,2: If you define all fluxes towards the surface to be positive (5546,17-18), then fluxes directed away from the surface are by definition negative and no minus signs should appear in Eq. 2.

Changes were done but a minus sign is needed in front of σT_s^4 because it is a positive value although the flux is directed away from the surface.

5547,17: Is this the heat capacity of snow? Is it the same as the heat capacity of ice listed in Table 2? Perhaps the variables/constants used in the SEB model can also be included in Table 2?

This is the heat capacity of ice, same as listed in table 2. A new table has been added with variables/constants used in the SEB model.

5548,15-18: As the values of the constants are listed in Table 2, I suggest to leave them out of the text, to improve the readability.

Done

5548,18: Consider using a subscript s (snow) or f (firn) for the snow density, as opposed to the water density ρw . Furthermore, in 5549,7, it is called firn density, please be consistent.

Done, we have replaced ρ by ρ_f and call it firn density everywhere.

5549,5: I assume the d in the denominator is indicating a time increment, not the mean grain size? Can you choose different symbols to avoid any confusion?

Done. Time increment is now Δt.

5549,6: ‘the snow/firn temperature (K)’ as opposed to air or surface temperature

Done

5549,8: Q is also the subsurface heat flux in Eq. 1, please choose different symbols

Done. It is now Q_{lat}.

5549,8-9: ‘released by refreezing meltwater (W m−3) in time interval dt’, to explain why Q does not have unit W m^{-2}

Manuscript has been clarified on this point.

5549,11: ‘exceeding’

Done

5550,2: ‘numerically’

Done

5550,11: ‘the most marked event’?

Done

5550,12-14: The surface elevation measurements are shown in Fig. 3, please refer to this figure here.

Done

5550,18-19: Please refer to the contents of Figs. 3 and 4 separately, if possible.

Figure 4 was deleted

5551,25-5552,1: You can refer to Fig. 3c here.

Done

5552,2: ‘This energy is released when the water refreezes from ... (Aug 20?) onwards.’

Done

5553,5,9: This sentence is too long and hard to understand. Since the same is said in the next lines, consider removing this sentence.

Sentence has been shortened.

5554,19-22: Move these sentences to line 17, before you describe the two striking features. Then the reader directly understands why these features are signatures of meltwater percolation.

Done

5554,11: ‘by our temperature measurements’?

Done

5555,28: ‘Calculated firn temperatures’

Done

5556,14: Please move the reference to Fig. 10 to the previous line, now it seems like Andes results are presented.

Done

5556,23: ‘has been considered’, or has it perhaps NOT been considered?

The true sentence is “ONLY the density anomaly above the horizon identified on 28 October, 2010 *has been considered”*

5557,6: ‘the whole domain’?

Yes. Change was done

5557,9-10: Move the reference to Fig. 11 to the next sentence, it is the quadratic fit that is shown in the figure.

Done

5557,11-12: ‘the frequency and the duration of melting events’

Done

5559,27: ‘that surface temperature is limited to’

Done

5560,24: ‘IPCC’

Done

Table 1: Listing the unit and the sensor height in separate columns would make the Table more readable. Alternatively, the unit can be given in brackets.

Done

Table 2: I could not deduce any logic in the order of the variables, could you perhaps use alphabetical ordering of the symbols?
Done

Fig. 3: The label ‘c’ in the third panel is not visible in the dark blue colour, please put the label at a different spot or use a white colour.

Done

Fig. 4: Make sure that all symbols used in the figure are explained and are consistent with the main text. For example, Lheat is probably the latent heat of fusion which is L in the text and Table 2.

Figure deleted

Fig. 6: Please use a lighter colour for the shaded areas, the green and black lines are not visible. I would also suggest to separately show Qm and Q, because the sum of the two fluxes is harder to interpret.

Done. We plot the sum R + LE + S because it represents the energy which is absorbed by the firn.

Fig. 7: It is impossible to read the text in the lower two panels, please use white colour here.

Done

Fig. 8: Introduce all symbols in the caption (Tair and Tsurf). Also, be consistent with the main text, where Ts is used for surface temperature. Can you use Tair and Tsurf in the fitted relation in the second panel instead of x and y?

Done

Fig. 9: The square at site 8 is not explained in the caption, I assume this is the AWS as in Fig. 1? Since most of the locations are shown in both Figs. 1 and 9, I wonder whether it would be possible to combine the two figures into one?

Square is now explained. We keep two figures because a single figure would be too overloaded.

References :

Brock, B. W., Willis, I. C., & Sharp, M. J. (2006). Measurement and parameterization of aerodynamic roughness length variations at Haut Glacier d'Arolla, Switzerland. *Journal of Glaciology*, *52*(177), 281-297.

Colbeck, S. C. (1989). Snow-crystal growth with varying surface temperatures and radiation penetration. *Journal of Glaciology*, *35*(119), 23-29.

Essery, R., & Etchevers, P. (2004). Parameter sensitivity in simulations of snowmelt. *Journal of Geophysical Research*, *109*(D20111), doi:10.1029/2004JD005036.

Hock, R. (1999), A distributed temperature-index ice-and snowmelt model including potential direct solar radiation. *Journal of Glaciology*, *45*(149), 101-111.