

Interactive comment on “Modeling near-surface firn temperature in a cold accumulation zone (Col du Dôme, French Alps): from a physical to a semi-parameterized approach” by A. Gilbert et al.

R. H. Giesen (Referee)

r.h.giesen@uu.nl

Received and published: 14 January 2014

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

C2991

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.

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 Q_m . 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.

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?

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

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

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)

C2992

is used? This should be mentioned, preferably together with the initial temperature profile (5550,1-2).

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

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?

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.

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.

5551,17-18: What is meant with the cumulative surface energy balance, how is it de-

C2993

efined? 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?

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.

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?

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?

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 a_{PSR} , 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 ($R = (1 - \alpha)\text{PSR} + L$ with L net longwave radiation), while L and the turbulent fluxes are an approximate function of $T_{\text{max}} - T_0$. So a simplified relation of the form $M = (1 - \alpha)\text{PSR} + (T_{\text{max}} - T_0)b$ would be more appropriate, in my opinion.

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.

C2994

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.

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?

Technical corrections

5542,17: 'the surface temperature reaches' or 'surface temperatures reach'

5543,21: 'by the Dirichlet'

5545,12: 'half-hourly'

5545,16: 'characteristics'

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.

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?

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.

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

C2995

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?

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

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

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}

5549,11: 'exceeding'

5550,2: 'numerically'

5550,11: 'the most marked event'?

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

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

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

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

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.

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.

5554,11: 'by our temperature measurements'?

5555,28: 'Calculated firn temperatures'

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

C2996

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

5557,6: 'the whole domain'?

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

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

5559,27: 'that surface temperature is limited to'

5560,24: 'IPCC'

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.

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

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.

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

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 Q_m and Q , because the sum of the two fluxes is harder to interpret.

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

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

C2997

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?

Interactive comment on The Cryosphere Discuss., 7, 5541, 2013.

C2998