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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "Air temperature thresholds to evaluate snow melting at the surface of Alpine glaciers by T-index models: the case study of Forni Glacier (Italy)" *by* A. Senese et al.

Anonymous Referee #2

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The study is based on a long and continuous time series from an on-ice AWS, spanning years with rather different snow cover conditions. It presents an uncommon application of a PDD model used to reproduce the occurrence, rather than the magnitude of snow melt, and it is a relevant and interesting contribution to the field. The method applied seems to perform successfully but more details should be provided as discussed below.

The language needs to be improved significantly, as some parts are barely understandable (e.g. most of the 'Discussions' section) and grammar is often incorrect. Most sentences will need to be rephrased so I do not include specific suggestions. The title and abstract should be more focused. A thorough clean-up of the abundant repetitions is needed.

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Interactive Discussion

Discussion Paper

It is important to note that the aim of the paper is to model the occurrence of surface melt conditions rather than quantifying melt. This is explicitly stated (1566, 4) but I did not pick it up immediately during the first reading and it may need to be better emphasized, starting with a more specific title. This is also important because it makes the study significantly more interesting and original than a mere exercise in applying the very well-known PDD concept to even one more site.

Some justification needs to be provided for taking the indirect and complex route of tuning the T_t and sDDF to best fit melt predicted by the surface energy balance model, instead of directly tuning them to best reproduce the occurrence of surface melt conditions as detected from emitted LW at AWS. It seems at times the focus of the paper oscillates between the stated aim and the modelling of surface melt totals. See also below my comments to Tab. 1 and Fig. 4.

The 'Introduction' section does not explain sufficiently the importance and interest of detecting surface melt conditions at sites where no on-ice AWS exist. None of the methods routinely used to detect melting conditions, notably microwave and thermal IR remote sensing is mentioned. Limits on spatial and temporal resolution/availability of these remote sensing techniques may make them not usable for a relatively small and complex topography glacier, but this will indeed provide additional justification for looking into a different method like the one presented here. No review of the state of the art is provided except for some information on PDD models, and nothing is said of what could be gained by adapting a PDD model for this use. The last point is particularly important because PDD models are essentially statistical tools empirically calibrated to fit observations over a longer period (the entire snow melt season in this study). Most importantly, the assumption that PDD, or any other single observable for that matter, is a workable proxy for available energy to melt snow and ice breaks down when pushed to sub-daily time scales, because the individual energy fluxes in the energy balance display a marked daily cycle and they each relate in a different way to air temperature. This issue should be introduced here and the validity of the proposed approach should be argued for in 'Discussions'. A direct comparison of 'melting surface detected from upwelling LW at AWS Forni' vs. 'melting surface predicted by PDD model' would be especially convincing.

1565, 20: correct, but then why use these daily averages at all, given hourly T_B were available (ad were used to produce Fig. 2)?

eq. (2)-(5): please make it more explicit what exactly these equation calculate. Qualify terms like 'melt' and 'melt amount' as 'ice melt', 'snow melt', 'ice and snow melt' as appropriate. I think M_EB is 'snow and ice melt', M_3C is 'snow melt', M_PEB is 'snow and ice melt', M_Tindex is 'snow melt' - are these so?

eq. (5): is DDF the same as sDDF of eq. (6)? If so consider using the same symbol, if not explain the difference.

1569, 7: Some more detail about temperature inversions is needed here, perhaps showing how strong and how often inversions affect the actual lapse rate between the valley and glacier station, as they will be reflected in the choice of optimal T_t value. The text indicates that more valley stations exist from this region, so it should be possible to describe statistically how much the real lapse rate deviates from the assumed -6.5 K/km. Temperature inversion causing lower T_B may well be the reason for the low T_t threshold temperature needed to adequately reproduce snow melt. This is important in order to assess how much the results reported by this study are of local relevance (i.e., specific to this particular glacier and valley climate) or indeed of more general applicability as claimed in 'Conclusions' by analogy with findings from Greenland .

1569 last paragraph: consider clarifying the description of 'temporal length classes' and why these specific ones (0, 4,6, 12, 24) where chosen

It may be a language issue, or there may be a problem with the physics implemented into the surface energy balance model: page 1570, 17-22, does this mean that the

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surface energy balance model can at times produce melt even though surface T is below freezing? How else to explain that larger modeled melt is modeled when the Ts check is removed?

The paper uses many words but is still confusing about how exactly T_t and sDDF were estimated when calibrating the PDD model. While T_t is the primary focus of the paper, and the point is touched several times (1568-1569, 1572, 1574, 1575, and these repetitions must be removed), nowhere the interaction between these two parameters (as well as the impact of uncertainties in the actual lapse rate) is addressed rigorously. The current text gives the impression that the procedure was a qualitative one based on (visually?) comparing results obtained from various T_t values. If so, providing the reader with a plot of the values being compared would be helpful. Perhaps a schematic flow chart may be provided if there is no expressive way to accurately describe the procedure mathematically. Even better, provide some scatter plot showing how the chosen 'temporal length class' 'better explains magnitude and variability of snow melting' (1572, 26) compared to other possible choices.

The optimal values found for T_t critically depend on the details of how albedo was used to identify the exact date of complete snow melt. Calculating and using albedo values from on-ice AWS data is in general not straightforward, so a bit more details are needed. How was albedo calculated from AWS radiometer observations? How sensitive is the optimal value found for T_t to the choice of 0.4 as the albedo threshold between ice and snow? In the model, albedo alone controls the date when SWE is assumed to have completely melt, but the text mentions that snow pits data are available for all but one year. Please show how well the modeled cumulative snow melt compares to the actual measured SWE at the date albedo drops below 0.4.

1571 (and Tab. 1 and 2): here and elsewhere in the paper there are many details about melt in April-June vs. rest of the melt season, why is this of interest for the purpose of this manuscript? Why is the focus on calendar months instead of the actual snow melting season for each particular year? And why are modeled cumulative snow melt figures discussed relative to ice melt (tangential to this study) and not to observed SWE from snow pits? Snow pit data is mentioned but not presented at all in the paper I think.

1571, 6: M_EB has units of length not time

Tab. 1: the paper deals with modelling the occurrence of snow melting conditions and I understand the PDD model was calibrated to that purpose. Why is it relevant to provide this much detail on snow and ice melt totals, and how do these modeled values compare to actual mass balance observations at the site of AWS1? The total for Annual M_EB seems to match exactly the cumulative melt in Fig. 3, but is Fig. 3 showing measured or modeled values? Assuming Fig. 3 shows measured data (does it?) and the model delivering M_EB was calibrated to reproduce that total, how well do individual years perform compared to observations? Add a column to show this, as it is important to assess how good a benchmark the surface energy balance model is for the calibration of T_t and sDDF.

Tab 2: 'number of days' do you mean 'Number of days with snow melt'? This table is incomplete without a column showing the ground truth of observed surface melt conditions from emitted LW at AWS1 Forni.

fig. 2 are these temperature points from the entire year or only during the snow melt seasons examined in the paper? Frequency and strength of inversion layers can vary seasonally quite a bit, and of interest here are exclusively those points during the snow melt season (possibly for more than one year, using different colors).

Fig. 3 is this measured or modeled? If modeled, how does it compare to observations?

fig. 4 These are curves of cumulative melt, not melt rates. Regardless, as with most time series plotted as time series, this figure is scarcely effective at showing which T_t performs best. Consider a scatter plot of (M_Tindex-M_PEB) vs. T_t, perhaps using different markers for different years. 1...638 as 'day' numbers on the x axis is unhelpful if the intention is to show the date snow disappears. Finally, neither cumulative melt

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nor melt rates are the focus of this paper, so please consider whether PDD model performance in matching the surface energy balance cumulative melt is the key point to focus on, or rather the performance of the PDD model in matching observed surface melt conditions, which is not shown in any table or figure.

Interactive comment on The Cryosphere Discuss., 8, 1563, 2014.