The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-188-RC1, 2017

The Cryosphere Discussions
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Interactive comment on "Relative performance of empirical and physical models in assessing seasonal and annual glacier surface mass balance in the French Alps" *by* Marion Réveillet et al.

Anonymous Referee #1

Received and published: 27 October 2017

This study evaluates the performance of the Crocus snowpack model and a temperature index model in simulating seasonal and annual surface mass balance of the Saint-Sorlin Glacier in the French Alps. The models are forced using SAFRAN reanalysis data; both in raw form, and corrected using in situ AWS measurements. The authors also examine model performance sensitivity to season, DEM resolution and temporal variation, inputted wind speed, albedo, and roughness lengths. The authors conclude that model performance decreases substantially without in situ meteorological measurements, and that without access to these measurements, an empirical temperature index model may be more appropriate than using an energy balance approach for future mass balance projections. The authors also state that Crocus model performance

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was more sensitive to wind speed input than ice albedo.

This study presents a useful exercise in evaluating multi-year modelling of surface mass balance and highlights some of the uncertainties in the methods. It will make a good addition to the literature, but I recommend some changes before publication; outlined in detail below. Broadly, the paper suffers from a lack of detail, both in describing some of the methodology, and in presenting only limited portions of the observations and results. As a result, some of the findings and conclusions of the paper feel unsupported. The choice of metrics used to present results, in some cases, limits the information provided to the reader, and may be inappropriately used in places. Some sources of uncertainty are not mentioned in the study, and while investigating each of these may be beyond the scope of the paper, they should at least be recognised.

Specific Comments: P1L26: 'an energy balance model'.

P1L27: 'requires'.

P1L27 – 29: 'With the current…' This sentence is unclear in its meaning. Consider rephrasing to clarify you are referring to the uncertainty in the temporal evolution of the relevant meteorological and surface properties of individual glaciers.

P2L28 – 31: Include references for both energy balance approaches mentioned here (parameterised and complete components).

P3L21: Include a few more details that are relevant to this study, for example slope angle and aspect at each station location. These could also be added to table 1.

P3L28: Missing an m in units for $\pm$0.15 m w.e. yr-1.

P3L29: Include units for 0.30.

P4Figure1: Remove black outline from triangles in legend or add them to those used on the map.

P4L10 – 12: Provide some brief details on the DEM creation and kriging method, and references if these maps were created in a previous study.

P4L18: Was the relative humidity sensor also in the aspirated shield? I'm assuming T and RH are from the same sensor (HMP45C), so clarify this here.

P4L18: Specify which directions (incoming, reflected/outgoing) you have measured for short and longwave radiation (as given in table 1).

P4L19: 'Data were quality. . .' This statement is very vague. Please provide detail on the quality control applied.

P5L1 – 4: Are the glacier stations mast mounted or sitting on the ice surface (relevant for maintaining constant height above surface)? Please specify. A picture of the stations would be useful. The height of the wind sensor on AWSm is mentioned in section 2.3.4 as being around 2 m. Can you provide specific height values for this and the wind sensors on the glacier stations? Near surface gradients of wind speed can be substantial.

P5L10: Some further details relevant to this study would be useful (in addition to supplementary details in the referenced paper). For example, the installed height of the EC system and how much on average its height above the surface ranged in between adjustments.

P5L24 – 25: No further details on the slope correction are provided in section 3.1.1. Some additional information would be useful, such as the slope angles at each station, how the partitioning of diffuse and direct incoming shortwave is estimated in SAFRAN (could this be affected by the presence of low-altitude clouds not considered by SAFRAN, as mentioned in section 2.3.4?), and what the impact of the slope correction is on the magnitude of the shortwave fluxes at the station sites. These values could also be useful when discussing the effects of changing the DEM resolution in section 4.2.1.

P6Table1: Variables and instruments are not properly aligned in table.

P7L5: Section 4.2 rather than 4.3

P7L6 – 8: In addition to R2 values, the mean bias error (or similar metric) between the SAFRAN and AWS-observed variables would be a useful metric to include to quantify any bias or lack off.

P7L8 – 10. 'The comparison between SAFRAN and AWSm. . .' I may be misinterpreting you here, but I'd like to clarify what your meaning is. You are saying that when there is little cloud cover, SAFRAN overestimates incoming longwave. Why then would the presence of low-altitude cloud, which has not been considered by SAFRAN be an explanation for this? Surely, the low-altitude cloud would lead to an increase in observed incoming longwave relative to the SAFRAN data?

P7L17: How do the estimates of incoming (slope-corrected) shortwave from SAFRAN compare with the values you calculate from the observed reflected shortwave and the albedo scheme?

P7L19: What is meant by wind speed 'generally considered' to be at 2 m?

P7L21 – 22: 'This underestimation is . . .' This statement could use a reference to supporting studies. In addition, it would be useful to present a comparison of wind direction in addition to wind speed. Is the observed wind direction in the downslope direction during these periods of large wind speed differences and suspected katabatic flow? How well does SAFRAN represent the local wind direction i.e. is the influence of the glacier accounted for?

P9L4 – 5: Comma required between 'model, implemented' and '2013), was'.

P9L9 – 10: How are the empirical values selected for microstructure? Are they glacier specific? What is the uncertainty in them?

P9L17: The density value of 917 kg m-3 is generally assigned to pure glacier ice. Have you examined your model's sensitivity to varying this value i.e. to account for uncertainty in the actual ice density?

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P10L10: When running the ATI, what data are used for the incoming potential direct solar radiation (IPOT)? Is this simply the incoming direct shortwave radiation from SAFRAN?

P10L16: I know the ATI model uses the WSMB from crocus, but is summertime precipitation and potential summertime snowfall accounted for when using the ATI?

P10L31 – 32: 'Peformance was evaluated…' Strictly speaking, you are not evaluating ATI performance with winter SMB measurements as you are using Crocus WSMB values (as mention above in L26 -27). Consider restructuring this sentence.

P11L33: Typo; missing an 'in' between presented and section, and section number should be 4.2.3.

P12L22 – 24: 'Indeed, the ATI…' This sentence needs to be rewritten, and the figure reference should be Figure 4a. Looking at Figure 4a, when observed SSMB is above -2 m w.e. (i.e. less negative than -2), the ATI model shows a tendency to underestimate SSMB (more negative), or in other words, to overestimate ablation.

P12L26 - 28: 'In addition, the temporal evolution…' By temporal evolution, do you mean you compared the cumulative simulated surface mass balances from the two models over the summer? If so, was this using a daily time step? Was this carried out just for one summer season or all? You have presented the maximum differences for the SSMBs, but perhaps if you are interested in the temporal evolution, it would be more interesting to describe the biases/differences that occurred within the season (e.g. are the SSMB differences driven by a general bias or by individual days with large differences etc).

P12L30: 'Here again…' Following on from my point above for L22 – 24, SSMBs in the accumulation area from the ATI model appear to be underestimated; it is the ablation that is overestimated.

P13Figure3: Axis labels should specify surface mass balance (SMB) not MB.

P14Figure4: Same point as for fig 3. Also, in caption, using ‘a-c’ etc. suggests a to c, inclusive (i.e. a,b,c). Consider using ‘a and c’.

P14L8 – 10: ‘various stakes’. How many stakes was this performed on? Some results/plots from other stakes would be useful. Maybe provide the mean and standard deviation (over all tested stakes) of the differences in annual SMBs for the extreme summers and winters mentioned here.

P15L5: Have you quantified this as ‘significant’?

P15L23: ‘First, correlations were computed…’ Correlations with what?

P17L1 – 4: Repeated referencing of table 3 is probably unnecessary in the same subsection.

P17Table4: Caption appears to be the same as that used for table 3. Why are the performances for ASMB without LW and wind corrections so poor compared to the seasonal performances?

P18Figure6: It would be interesting in these plots to see the effect of removing the precipitation correction also.

P18L11: Swap the position of ‘blue’ and ‘black’ to avoid confusion.

P18L13: ‘The use of…’ Ensure that you refer to correcting the wind speed data rather than ‘wind data’, as you have not discussed correcting the wind direction in this paper. The ‘Without wind correction’ label in Table 4 should also be corrected.

P19L5: Provide mean values of the turbulent fluxes using observed and SAFRAN wind speeds.

P19L11 – 12: Replace ‘up to’ (e.g. ‘up to 20°C’) with ‘max. increase of’. The use of ‘up to’ here sounds like the surface temperature is increased to this temperature!

P19L21: Can you clarify what you mean by a positive feedback in this case? An increase in ice/snow surface temperature can reduce the turbulent heat flux into the surface.

P19L23 – 24: Add speed to '…correction of wind on the winter…' Also, just to clarify, does wind speed not affect how the model deals with snow accumulation?

P19L26 – 27: 'A larger impact…' Are you suggesting this is due to it being an ice surface or because the surface is warmer at this time and melt is occurring?

P20Figure7: Explain dashed line (i.e. melting point).

P20L8 – 9: Why only test for roughness values that were larger than the employed roughness value?

P20L10 – 13: It would be useful to present the values for the % difference in SMB between the different roughness value scenarios, as it is hard to distinguish in figure 8 if there really are different responses for snow and ice surfaces, or if it just that the magnitudes are greater because there is greater levels of ablation over the ice surfaces. Clarify what you think your results are showing. Are you suggesting that varying the implemented roughness lengths for snow will not affect the turbulent heat fluxes estimated by the model over this surface? This would not follow the theory of the bulk aerodynamic method implemented in the model.

P20L17 – 18: 'Note that…' Was this a single sum over the full season, or did you examine the simulated fluxes over shorter timescales e.g. daily sums? Using shorter timescales might allow you to look for the temporal variation in roughness length you have mentioned.

P20L20: You have arbitrarily selected the values for the scalar roughness lengths to be 1/10th of that for momentum. However, there are other schemes suggested in the literature to estimate the scalar values, such as assuming a 1/100 ratio, assuming they are all equal, and utilising a surface renewal method (Andreas, 1987). Have you considered the model sensitivity to these values? Another major source of uncertainty in the estimating the turbulent fluxes is how the model accounts for changes in atmospheric stability. While an investigation of this uncertainty may be beyond the scope of this paper, it is worth mentioning it as a source of uncertainty, particularly when using reanalysis data where the localised effects of the glacier on stability may not be resolved. Andreas, E. L. (1987). A theory for the scalar roughness and the scalar transfer coefficients over snow and sea ice. Boundary-Layer Meteorol. 38, 159–184. doi: 10.1007/BF00121562

P21Figure8: Reposition '(b)' and '(c)' in the caption to be in front of the relevant text.

P21L14: This finding regarding the importance of ice albedo to model performance may not be very transferable. The same test performed on a glacier in a different climate setting (e.g. one where radiation has a larger contribution to melt energy) may find model performance to be more sensitive to albedo parameterisation.

P22L31 – P23L2: Following on from the point above, it would be interesting to see the partitioning of the melt energy for the study glacier, and a summary of the meteorological conditions. These model sensitivity findings may be very dependent on the ratio between the turbulent heat and radiation fluxes.

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