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The Cryosphere
Discussions

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 #2

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Review of Relative performance of empirical and physical models in assessing seasonal and annual glacier surface mass balance in the French Alps by Reveillet et al

The paper by Reveillet and others describe the results of the modeling of the surface mass balance of Saint-Sorlin glacier in the French Alps. The SMB is calculated with CROCUS, an energy balance model originally designed to calculate snow pack. This model is forced with SAFRAN reanalysis data that is corrected to the in-situ weather observation at the location of the glacier. The authors explore the sensitivity of the simulated SMB to different components of the climatic forcing and to various model parameters. Furthermore is the result of the energy balance model compared with the

results of an empirical model. The study is well written and the results are interesting. I have the following comments, spit up in general comments, specific points and some technical points, the latter two per page and line number.

general points

- the difference between energy balance model and empirical model is not just the model formulation, but also the way the models are calibrated. The parameters of the empirical model are calibrated by fitting calculated SMB to the observed SMB, while for the energy balance the meteorological input of the model is fitted to observed weather. Would the energy balance model perform better if its model parameters and input corrections are optimized to the observed SMB?

- I do not fully see how the conclusion that empirical models would be better suited to model glacier SMB for periods, or glaciers, without AWS measurements. The results of this paper show that the energy balance model performs better, mainly in the accumulation area, than the empirical model. This is also true for the period where the input of the model is not directly corrected with the AWS data. So therefore I do not see why the empirical model should be more suited to model future glacier mass balance, when also no direct correction of the meteorological forcing with in-situ observation is possible.

- It would be instructive include information and figures on the climate at Saint-Sorlin Glacier in the paper. And to include in these figures a comparison between the original climate given by the SAFRAN data and the climate as measured with the AWS.

specific points

page 2: -l7 in addition to the fact that it is questionable if the calibrated parameters are valid over long time periods, a disadvantage of the temperature-index models is that the parameters have no validity for other glaciers. Maybe you could add this here.

page 3 - l2-5 Here you discuss variability, but you are, also, interested in the absolute value of the SMB, not only of the variations from year to year. - l20 I do not see that the aspects of Saint-Sorlin vary a lot in Figure 1, it is mainly N - NE - E slopes. Please be a bit more specific.

page 4 - Figure 1 Could you add to surrounding topography in Figure 1b? That could give a better understanding for your discussion of wind speed and snow distribution. - l10 What is the accuracy of the DEMs? Do they give the same values for stable ground?

page 5 - l23 So SAFRAN is not gridded, but has one output per mountain region as given in Figure 1a? And this is then distributed following elevation and aspect?

page 7 - l5 High correlation doe not say it all, as you can have a significant bias while having a high correlation. Could you indicate how well the values correspond, for example with a RMSE? - l9-10 How does a lack of low-altitude clouds in SAFRAN explain an overestimate of incoming long-wave radiation in SAFRAN? I would expect that low altitude clouds are warmer than high-altitude clouds and thus emit more long wave radiation, such that not including these low-altitude clouds would lead to an underestimate of the incoming long wave radiation. - l17 Also here, could you give an estimate of the bias/rmse in addition to the correlation? - l22 How likely is it that you measured the katabatic wind on the site of AWSm? It is located off the glacier and well above the nearest glacier surface (glacier extends to below 2700 m and the AWSm is at 2720 m), while the katabatic wind on a small glacier can be quite shallow. If you look at the wind direction, do you then see a consistent down-slope wind?

page 10 - l30 It is not clear why you perform the calculations on a 200 m resolution. Here you refer to 2.2.2, where in turn is referred to 2.3.3 where it is stated 'we linearly interpolated on the 200-m horizontal resolution grid' without any further reason why this interpolated to this 200 m. Please explain.

page 11 - l10-33 I would add the content of these section 3.2.2.x to the respective subsections in the results section 4.

page 12 - l10-11 Here you write that the interpolation used to create the precipitation can explain the difference between measured and simulated winter SMB. But if all winter SMB measurements are included in the determination of the precipitation fraction maps, then how does the interpolation between these points affect the model results on the stake locations? And how do melting events in the accumulation period explain the difference? They should be in the simulations as well? - ln29 This contradicts the conclusion that when no correction in the forcing is possible due to lack of AWS data, the ATI performs better than the energy balance simulations

page 14 - Figure 4 You do not plot the correlation here. - section 4.1.3 I think the results of this section could be clarified with a scatter plot where you plot annual SMB vs summer SMB with constant winter SMB and one plot with annual SMB vs winter SMB for constant summer SMB.

page 16 - l6-9 This part is not so clear to me. How can differences up to 25% be explained by 'only slightly affect the simulated SMB for a limited number of stakes'?

page 23 - l24 I do not really understand the conclusion that empirical models would be better fitted to model future SMB. From your results it is clear that using observations to correct forcing the energy balance model performs better than the empirical model, also for the period where no AWS is available. It is unclear whether the forcing corrections remain valid in the future, but the same holds for the parameters in the empirical model (as you have pointed out). So why is then the empirical model more reliable for future projections?

technical points

page 1 - Why not include Saint-Sorlin in the title, replacing the French Alps?

page 3 - l23 Here and elsewhere in the paper: I am not a big fan of these acronyms WSMB, ASMB, SSMB. I feel it provides easier reading by just writing out 'winter SMB', 'annual SMB', and 'summer SMB'. - Table1: add some separation between the different stations, like a line, or a blank text line.

page 5 - l10 Please rephrase this sentence on the instrumentation height adjustment. You probably mean that you lowered the instruments in order to keep the distance between instruments and the ice surface constant. - l27 'emitted long wave radiation and reflected short wave radiation', the earth surface does not emit short wave radiation.

page 7 -l9 could you replace 'explained' with 'caused'? 'explained' would require a more in-depth analysis

page 9 - l24 If I understand correctly, you have changed the lower albedo limit from 0.7 to 0.5, keeping the time decay. Then I suggest to replace 'fixed at' with 'set to', as 'fixed' could indicate that you eliminated the time evolution of the albedo.

page 14 - Figure 4 caption: replace the hyphen in 'blue (a-c)', 'orange (b-d)', etc. with a comma 'blue (a,c)'

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