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Interactive comment on "Robust uncertainty assessment of the spatio-temporal transferability of glacier mass and energy balance models" *by* Tobias Zolles et al.

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The manuscript of Zolles et al presents ensemble simulations of glacier mass balance with an energy balance model applied on 2 glaciers and 3 seasons. The main innovation compared to the existing literature consists in a relatively comprehensive analysis of model sensitivities and uncertainties. The paper is well written and well structured. The statistical framework is clearly explained. I especially appreciate the effort of the authors to give simple examples to explicit the formalism (pages 7; 9). The conclusions are clearly summarized and consistent with the obtained results. The complex equifinality between the parameterizations of such models is well demonstrated and the implication in model calibration and model transferability is very interesting. Therefore, I think this paper deserves publication after a minor revision which would account for my few comments below when it is possible.

Page 2 line 19: I think most studies base this statement on an evaluation of the energy fluxes and surface temperature, not only the melt rates.

Page 4 line 7: I understand the deficiencies of the cited references but given the number of studies which just present simulation outputs without any uncertainty quantification, I think that the word "inadequate" is a bit severe.

Page 4 line 11 / Table 2: the 23 parameters include a "precipitation perturbation" which disappears in the results section (Figure 3) without any specific explanation.

More generally, it is not completely clear if the authors want to incorporate the spatialization of meteorological data as part of their model uncertainty study. The decision to exclude the longwave parameterization from the free parameters has a strong consequence in the results. Indeed, large errors are introduced here because Equation A2 is a strong simplification of the real behaviour of the full column of atmosphere. Snow models are usually extremely sensitive to these errors (Sauter and Obleitner, 2015; Quéno et al, 2017). The authors acknowledge this limitation (page 19 lines 5-12) but I do not really understand this choice. Why should the impact of temperature gradient uncertainty on longwave radiation be accounted for if the parameters of equations A3 and A4 are not? Similarly, what is the logic in considering the uncertainty of precipitation gradient but not the uncertainty of the mean precipitation forcing? I think it could also be more explicit in the text that Figure 7 does not represent the full contributions of uncertainties. The very narrow range obtained for longwave radiation is unlikely to represent the real uncertainty of this component as the incoming flux is highly uncertain whereas it is not accounted for.

Table 2: Can you comment the range of precipitation density? This range is not realistic for snowfall in the Alpine area (too high values, Helfricht et al, 2018). It may

compensate some deficiency in a simple model which does not represent accurately compaction but this should be detailed. The authors could also comment the implication in the uncertainty analysis of using some potentially unrealistic values for some parameter ranges. The same could apply if the precipitation perturbation was really considered because a 10% error is not sufficient to represent precipitation uncertainty in moutainous areas.

Page 13 lines 21-22: I am not sure to correctly understand this sentence. Could you develop what you mean by "less constrained" and what is the relationship with a narrow initial range of parameters?

Page 19 line 1-4: This is true but rather utopic at the moment. Such models need a forcing of impurity depositions. The existing products are not sufficiently reliable nor sufficiently detailed to depict the processes responsible for the spatial variability of albedo on a glacier.

Page 19 lines 13-20: The authors discuss the impact of the possible variability of roughness lengths. However, I think they could also discuss more generally the relevance of applying this theory of turbulent fluxes formulation in mountainous environments where the turbulence is probably more affected by the surrounding topography than by the surface roughness itself (Conway and Cullen, 2013).

Page 19 lines 21-22 Which effects are you talking about? From experiments with a detailed snowpack model (with a sufficient vertical discretization), it is rather clear than the absorption profile has an impact on surface temperature and on the temperature gradient close to the surface (and therefore on snow metamorphism). However, the effect on more integrated variables is likely to be much less significant.

Page 20 line 15 I do not know whether new field experiments on that topic are really required right now. The authors should first mention that the relationship between albedo and grain shapes and sizes is already implemented in detailed snowpack models such as Crocus (Vionnet et al, 2012) or SNOWPACK (Lehning et al, 2002).

Page 20 lines 30-32 I agree and the same applies for various variables, especially surface temperature which is a good indicator of the correct resolution of the energy balance.

Page 20 line 33 The lack of a full quantification of the meteorological uncertainty is probably the main limitation of this paper. This is only stated here in a small paragraph which would have deserved to be more developed based on the existing literature. Indeed, this is probably the most studied uncertainty in previous studies in snow modelling (e.g. Raleigh et al, 2015) and in glacier modelling. However, the possible compensation errors between meteorological forcing and model parameters may deteriorate the relevance of model uncertainty studies which do not account for forcing uncertainties. I did the same thing myself in the context of a detailed multiphysics snowpack modelling (Lafaysse et al, 2017) but I just think that this limitation could be more discussed.

Page 21 line 27Âă: To what does 1 kg/m^2 refer? In which duration?

Typos: Abstract line 5: "which" introduces Page 16 line 26: energy melt energy Page 19 line 4: change Page 21 line 32: For

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