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Geoscientific Model Development Discussions

Interactive comment on "COSIPY v1.2 – An open-source coupled snowpack and ice surface energy and mass balance model" *by* Tobias Sauter et al.

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The manuscript by Sauter et al., describes the COSIPY v1.2 open-source coupled snowpack and ice surface energy and mass balance model. This model is designed to simulate the energy and mass balance of snow and ice covered surfaces, with applications for glacier mass balance simulations. The model builds on several decades of research in the field of snow cover and ice simulations. The main originality of this model is that it is implemented in Python. Given the scope and the content of the manuscript, it is fully appropriate for publication in Geoscientific Model Development. Overall, the manuscript reads well and I have not identified major flaws in the manuscript. Note,

however, that I haven't checked one by one all the equations in Sections 2 and 3, which are based on classical concepts and frameworks for snow and ice energy and mass balance. I have several comments, which can rather be seen as suggestions, to the authors, and a series of minor comments.

Main comments:

Section 4 : While Sections 2 and 3 are in fact of limited added-value given that the equations are concepts are already outlined in a number of previous publications (it is fine to leave them in the manuscript, this is a useful reference for users of the model or its output, perhaps complemented by recent publications such as Essery et al., 2013, http://dx.doi.org/10.1016/j.advwatres.2012.07.013 and Lafaysse et al., 2017, http://dx.doi.org/10.5194/tc-11-1173-2017), I find section 4, addressing "Model architecture", quite short and it could be expanded to better address the novelty and added-value of the model compared to previously existing models. For example, I think that it could be useful to provide more details regarding the Python libraries used for this model, their common dependencies, added-value, etc., and how the "modularity" of the model structure is implemented. This could be addressed not only by adding text, but also figures, providing an overview description of the model structure and the interlinkages between them.

Section 5 : The section 5 provides an example of the model use for the Zhadang glacier, High Mountain Asia, with illustrations of model output (Figures 1 and 2) and model performance (Figure 3) for this case study. The results appear to be reasonable for a typical energy and mass balance model applied to a glacier setting. However, this does not correspond to a full model evaluation exercise, and I think this model description article would greatly benefit from a more robust evaluation. In this respect, I think the dataset used for the ESM-SnowMIP intercomparison could be particularly useful. All the relevant data have been made available in Ménard et al., 2019 (https://doi.org/10.5194/essd-11-865-2019), and paper such as Krinner et al., 2018 (https://doi.org/10.5194/gmd-11-5027-2018) can be used as inspiration for providing

the evaluation indicators of snow models. Regardless of how this is handled, I consider useful for this model description article to provide some evaluation metrics relevant to the performance of the model described in this article.

Minor comments:

Page 4, line 15 : how do the re-meshing algorithms compare to existing re-meshing algorithms used in other snow cover models ? I think in particular of Crocus (Vionnet et al., 2012, https://doi.org/10.5194/gmd-5-773-2012), there are other models with re-meshing approaches. I think it would be good to position the approach taken here within other existing models.

Page 4, line 24 : “useful feature” : would it be possible to elaborate on what is meant by “useful feature” ? What metric was used to address the “usefulness” ?

Page 7, line 3 : I suggest to use the LaTeX symbol \varepsilon instead of \epsi, this seems to better match the graphical design of the “epsilon” symbol, when it refers to the emissivity.

Page 9, line 3, I suggest replacing “Von” by “von” for the name of “von Karman” (ideally with “accents” on the “a”s).

Page 10, line 7 : I don’t think it is adequate to refer to “snow grain settling”, but “Snow settling” would be less ambiguous and more accurate.

Page 11, line 20 : I think more details should be given on what is referred to here as “dynamic mesh” ?

Page 12, line 9 : More explanations could be given to better explain the content of the parenthesis “(not recommended for distributed simulations)”

Page 12, line 31 : I think more explanations are needed for “driven by ERA-5”, in particular whether downscaling was applied, and if yes, how.

Page 13, Figure 1 : I suggest replacing “modelled” by “simulated” in the legend and

captions.

Page 14, Figure 3 : Would it be possible to provide a definition for the term “Speedup” ? I think this would be a useful clarification. If possible, it would be useful to provide a comparison of this metric with other existing models, in order to address to what extent the scalability of this Python-based model is comparable to implementations using other programming language.

Page 15, line 15 : I think it would be appropriate to also refer to multiphysics modelling, and it would be good to know to what extend COSIPY can be used for such applications (see e.g. Pritchard et al., 2020, https://doi.org/10.5194/tc-14-1225-2020).

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