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

Anonymous Referee #1

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COSIPY v1.2 - An open-source coupled snowpack and ice surface energy and mass balance model
Tobias Sauter, Anselm Arndt, and Christoph Schneider

This paper describes a distributed surface energy and mass balance model coded in Python and available as open source on github. The paper describes in much detail the physics included in the model.
The paper is well written and concise, although sometimes a bit too concise, see remarks below.

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Discussion paper

My main concern with this manuscript is that in my view it does not present anything new. There are several distributed energy and mass balance models available, some are more sophisticated than this one, some less, and at least one of them is also available as open source on github. The model itself is also not new, there are several publications with an earlier version of this model (Huintjes et al. 2014 and 2015), and the model physics in general is used in the other models as well and is already described in similar detail in other studies. I am not sure whether there are more of these type of models programmed in python, but that does not seem the key point here.
Thus, what makes this model special or new to warrant publication?

Besides this general concern, I have a few other comments.

General comments
It should be made much more clear what is new about this (see above).

In my experience it is often not so much the model formulation and running of it that is a problem, but the preparation of the input data. In this manuscript there is almost no information on how the input data is prepared and how it is distributed over the grid. Is this provided for in this package or should the user do that him/herself? And if it is included, how is it done? Make clear what the user is suposed to do him/herself and what is included.

Other information I am missing is on initial conditions, tuning and spin up. What procedure do you use? Is this also something provided for in the package or has the user do this him/herself?

After the model description, the model is applied to a Tibetan glacier as an example. I appreciate that you show that the model is indeed producing reasonable results, but I would have liked a bit more evaluation, analyses and interpretation on how well it is doing, and why there are differences, compared to observations and to other models.

Abstract
P1 Lines 1-7 are a very general introduction. Is that necessary in an abstract? I suggest to either remove it or make it much shorter. Formulations are also not clear. For example, 'key role' in what (line 1)? and where do 'these changes' (line 2) refer to?
P1 L8: remove 'lean'. I have no idea what you mean by this.
P1 L16: remove 'in'.

Introduction
P2 L2: What do you mean with 'many scientific aspects'?
P2: Note also the work by Ostby et al. 2017 TC, and by van Pelt et al. (several studies) for Svalbard.
P2 L30: The Hock and Holmgren 2005 JGl model is available on github.
P2 L33: Remove 'lean'.
P3 L6: Make much more clear what is new. I do not see it.

Model concept
eq(3): The second term on the richthand side reads: $k_s \frac{\delta^2 T_s}{\delta z^2}$ Shouldn't this be $\frac{\delta}{\delta z}\left(k_s \frac{\delta T_s}{\delta z}\right)$. In your case you ignore the effect of the gradient in k with depth. Furthermore, what is the functional form you take for k_s? And why use it? Bartelt and Lehning already note that they think this is an inferior description of k_s.
P3 L25: check the equation, for cs in combination with eq(3). I think there is a ρ_s to many in eq(3).
P4 L7: Is your model indeed as deep that it reaches the base of the glacier? Most

models only go 20 to 30 m deep. More is not really necessary for climatic surface mass balance studies.
P4 L16: In my own experience, re-meshing, complete making of a new grid, is not necessary to do every time step, but can be made depended on melt and snow fall. This speeds up the model considerably when nothing is happening to the snowpack. Or do you also refer to re-meshing when only thickness of the layers changes a little, and thus also depth, due to densification?
P4 eq(5) Where does this equation come from? ColeÌĄou and Lesaffre, 1998, provides 1 equation for the full range of θ_i
P5 L1: Does the model include saturation of the snow? And if so, how is it described, and if not, please mention.
P5 L2: What happens in the accumulation/firn area? When does runoff occur in that area?
P5 L17: Especially with respect to solar radiation it is important to mention how you distribute the input forcing over the glaciers. Do you include a formulation to distinguish between direct and diffuse radiation, shading? Or does the user have to do that separately?
P6 L2: Also in case of longwave radiation, how do you distribute this over the glacier? Do you then always use eq (16)?
P6 eq(17,18): I do not understand the term 1/Pr in this equation. In my opinion this factor should be included in how you calculate Ch and Ce, since not all methods that you present to calculate Ch and Ce should include this term.
P7 L1: How do you determine z0q and z0t, you only mention a factor. Do you indeed only apply a factor on z0m to obtain z0q or z0t or do you use a method such as described by Andreas, 1987, BLM?
P8 L4: chang to 10 superscrip (-2). Confusing as it is now.
P8 L11-15: What you describe here is a variation on what is described in Bartelt and Lehning. However, you refer here to a French report, which is hard to find. I prefer you to change the reference to something that is general available.

Example
P12 L10: Please make more clear that you start by running the model for a single location and only run it in distributed mode from line 30 onwards. It is often not clear whether you refer to a result for a single location or the whole glacier.
P12 L14: Capital RH instead of rH.
P12 L16: Where do you get the precipitation from?
P12 L24: You first have to mention that you obtain the surface temperature from Lout observations, else this statement makes no sense.
At this point I would like a bit more information on when the model is doing a good job, and when it struggles, and why. What are the limitations, how does this compare to other distributed mass and energy balance models?
P12 L24: Where is the stake you refer to here located with respect to the weather station and your grid point?
P12 L26: Typo: modelleld should be modelled.
P12 L24-30: In this analyses I suggest that you distinguish between the time the glacier is snow covered and when the ice surface appears. The model should be well capable to reproduce the amount of ice melt, whereas surface changes in case of snow cover are much more difficult, since that also includes firn densification processes. Presenting ice melt separately also gives an indication of how well you reproduce the energy fluxes at the surface. Unless this is all snow covered period. But you have to make that clear. Figure 1: Make clear whether this is m ice/snow or m w.e. And indicate in the figure when ice is exposed (or not).
P12 L33 - P13 L2: When you refer here to the distributed version of the model, do you compare the grid point of the weather station with the results when running the model only for the weather station location? If that is the case, why is there a difference in annual mass loss? What is done differently?
P13 L2: To what results do you refer here? Glacier wide? or Point location? Or the difference between them?

Conclusions
P15 L8: Remove 'of its kind'.
P15 L12-17: Other models can do this as well, and moste of the topics mentioned have been done, at least for individual regions. What does this model add to that?

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