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The Cryosphere Discussions

Interactive comment on "Distributed vs. semi-distributed simulations of snowpack dynamics in alpine areas: case study in the upper Arve catchment, French Alps, 1989–2015" *by* Jesús Revuelto et al.

Anonymous Referee #1

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1 General comments

1.1 Summary of goals, approaches and conclusions

Revuelto et al. studied the quality of a semi-distributed and a distributed energy balance snow cover model (Crocus) in an alpine catchment covering large differences in elevation and other topographic features. They were able to define an observation dataset which consists of satellite snow covered area (SCA), snow depth measured at five stations, glacier surface mass balance (SMB) data and the annual equilibrium line

altitude (ELA) of glaciers. Most of the validation data covers a period of 14 years. The semi-distributed approach integrates different elevation bands and aspects, while the distributed approach models shading, slope and elevation effects explicitly on a 250 m resolution. Meteorological forcing was obtained in the same 14 years' time period by a re-analysis model called SAFRAN, which delivers semi-distributed outputs. For the distributed version this data was interpolated to a grid.

Model output as snow depth, SCA, SMB and ELA was compared with observations using a variety of quality measures as root mean squared error, mean absolute error, and R-squared, amongst others. The authors also accounted for spatial similarities of the satellite derived spatial SCA products and modelled outputs.

The authors conclude that both approaches were able to reproduce observations, while there are slight advantages recognizable when using the distributed model approach.

1.2 Conceptual overview of the contribution of the paper

Evaluating different model approaches to model snow cover processes is important. Many research teams worldwide incorporate distributed or semi-distributed modelling approaches. Thus, it is helpful to evaluate a given modelling setup.

The aim of comparing two different model approaches based on the presented results is difficult to my opinion. My impression is that model deficiencies in both models may be able to override differences between them. Two model deficiencies could be mentioned. First, there is a scale issue between the meteorological forcing (massif scale 1000 km2) and the potential differences between the models (terrain shading effects). Second, both models handle SCA in a binary way using unrealistic thresholds for complex rough terrain.

Two other issues are the similarity to previously published papers, and the quite subjective validation procedure. Finally, the communication of the results is quite poor. Sometimes I am not able to follow statements in the text after studying tables and

figures, sometimes I am not able to follow the content of the sentences.

In the next subsections I will be more detailed to the here mentioned issues. I am convinced the authors are able to address my specific comments below. Thus I recommend to publish this manuscript in The Cryosphere after Major Revisions.

2 Specific comments

2.1 Potential model deficiencies

I understand that the authors want to establish a long data set for validation, which may have led to the decision to use SAFRAN with 14 years of meteorological forcing. However, after reading this manuscript I think SAFRAN does not vary precipitation amounts in one elevation/aspect category within the massif scale (i.e. 1000 km2). I am not sure if such a rough meteorological forcing is helpful to establish differences between a high resolution distributed model (250 m) and a semi-distributed model. I would suggest that a numerical weather model with a fine resolution (e.g. AROME with a 2.5 km resolution, Queno et al., 2016; or Vionnet et al., 2016) may be used as well for a few available years to show more clearly the potential of high-resolution modelling.

I am a bit surprised that an author who measured snow depth in alpine terrain in high resolution suggests a model approach that defines SCA to be zero, when modelled snow depth in a pixel is below 0.15 m, and one otherwise. 15 cm mean snow depth is not sufficient to cover large parts of a 250 m pixel in complex terrain. This may be true after a new snow fall, but not when a seasonal snow cover is melting out. While this is a common model approach, there were new SCA parameterisations developed in the past years to account for heterogeneous snow depth distribution based on terrain characteristics (e.g. Helbig et al., 2015; Cristea et al., 2017). This model evaluation has a large emphasis on SCA. In such a case I think a state-of-the-art SCA parameterisation is necessary. This is also necessary for a 250 m resolution grid since snow depth varies largely at smaller scales (e.g. Trujillo et al., 2007).

To my opinion, the benefit of terrain shading in the distributed approach may be overridden by these model deficiency.

2.2 Validation

The authors chose a quite subjective definition what a good model performance is. This lead to formulations as “appeared reliable” (line 439), “Overall, the ability [. . .] was satisfactory” (line 568ff). For many measures it is not clear to me if a difference in model performance is substantial or negligible. Here I would suggest a more objective procedure integrating a baseline model or quantifying error metrics in relation to year-to-year differences, for example.

2.3 Differences and similarities to previous papers

Last year this group of authors published two papers validating a SAFRAN/AROME-Crocus model (Queno et al., 2016; Vionnet et al., 2016). I would like to see more clearly what the advance of this manuscript is and how findings in those paper compare to this study (e.g. effects of meteorological forcing using SAFRAN). Some differences are obvious (250 m vs. 2.5 km resolution), long-term dataset with a spatial focus, but should be clearly mentioned in the Introduction.

2.4 Communicating and presenting results

I had large difficulties with understanding this manuscript. Statements in the text were sometimes not documented in the presented Figures and Tables. For example, I do not see in Figure 6 a higher consistency between observations and simulations in the winter in relation to the summer, which is stated in line 470. In Table 6 and 7 the ASSD values are higher for the distributed approach, but oppositely stated in line 506. Furthermore, I do not see that the distributed version is simulating the surface mass balance in the winter similarly well compared to the semi-distributed version (lines 757ff), while in Table 8 the RMSE for Mer-de-Glace and WSMB is much worse.

Many sentences remain unclear to me. Sometimes I am not able to determine if the

authors refer to winter or summer mass balance, or to which elevation band, or to what the demonstrative “this” is referring to. Improving the English grammar would certainly help to reach a clear and concise communication of the results.

3 Technical comments

The following comments show that some methods are not described in a sufficient way in order to be able to replicate results.

Lines 231ff: Please add more details on how the coarse SAFRAN categories were interpolated to the fine 250 m grid. The given citation (Vionnet et al., 2016) does not describe the procedure, but refers to another citation. Since energy balance models are quite sensitive to input data, the chosen interpolation methods should be presented here to understand the differences between the distributed and semi-distributed model approach.

Lines 291ff: Please add more details to the conclusion, why a full evaluation of the simulation is possible with the chosen validation data. What is a full evaluation and how do you apply Hanzer et al. (2016) on your dataset?

Line 325: I cannot find the name MODImLab in the two references. Is this a software built on these publications? Please clarify.

Line 329: I cannot find the term unmixing_wholesnow (UWS) in Charrois et al. (2013). Please clarify.

References

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