The Cryosphere Discussions

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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.

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Author's comments:

Below we provide a detailed response to all comments, and indicate resulting changes in the manuscript. Please, note that lines referred in this response are these of the manuscript tracked with changes. Additionally to changes referred in this response other changes have been accomplished to improve the final manuscript. For instance the manuscript title has been changed to better describe our study. Now the title is:

"Multi-criteria evaluation of snowpack simulations in complex alpine terrain with two spatialization approaches". Also some sections of the article have been reduced and others include further information; please check the manuscript with tracked changes to see them.

Reviewer 2 (R2) I generally disagree with the interpretation of results and the conclusion that "... distributed simulations... are the recommended modelling approach". Quite the opposite! I think the results make a case for the promotion of a semi-distributed snow model when carefully designed to parsimoniously maximize on relevant physiographical and meteorological information content while remaining computationally tractable.

A: The comparison between both approaches has been improved in the revised manuscript (see response to comment 2.2 from Reviewer 1). In particular, a bootstrap approach is now used to test the significance of the differences between the scores. Thus, we can now provide an objective comparison of simulation results. Furthermore, despite the statistically significant improvement obtained with the distributed simulations, we agree with Reviewer 2 that the skill of the semi-distributed simulations is sufficient in many applications with much lower computational requirements. Therefore, following the reviewer's comment, the abstract and the conclusions of the paper were modified as follows: Abstract final sentence: "Slightly better results were obtained using the distributed approach. The improvement is statically significant mainly because it includes the effects of shadows and terrain characteristics (local values of aspect, slope and elevation for each grid cell). However, the minor improvement observed with a much higher computational time does not justify the recommendation of this approach for all applications as long as distributed simulations are not combined with new data assimilation techniques and higher-resolution meteorological inputs. " Similarly in the conclusions: "Overall, the results of this study demonstrated that distributed simulations reproduce slightly better snowpack dynamics in the alpine terrain of our study area. Distributed simulations take into account the specific topographic characteristics

of each pixel (local values of aspect, slope and elevation) and more importantly the effects of terrain shadowing by surrounding areas. Accounting for these two effects over long time periods led to statistically significant better results for the distributed approach. However the lower computational requirements of semi-distributed simulations together with the flexibility on the design and application scale of the simulation make this approach also suitable to simulate snowpack evolution."

R2: In my opinion, the study comes up short of providing a comprehensive evaluation of the subjective modeling decisions and scaling issues that differentiate the two approaches. Thus, the paper misses an opportunity to offer a clear scientific advance. The authors present point-scale, semi-distributed, and fully distributed modeling as if they were three independent techniques with predefined structure. Rather, don't these approaches exist on a spectrum of scale and design, offering substantial flexibility to the modeler? Worse, there is little description of the authors' decision processes: 1) how were the # of semi-distributed units decided upon, 2) how was the 250 m grid scale of the distributed model determined (why not 350-m or 100-m), and 3) how sensitive might results be to these decisions?

A: 1) The design of the semi-distributed approach corresponds in terms of elevation, aspect and slope classes correspond to the design of the operational system used for avalanche hazard forecasting in France for more than 20 years (Durand et al, 1999 ; Lafaysse et al 2013), as mentioned in the paper in section 3.1 2) and 3) We have better described in the introduction why the 250 m spatial resolution was chosen and we have also discussed which consequences these decision may have on results. Here are the new sentences included in the introduction (Line 182-189): "...The final products of both simulations are 250 m gridded snowpack datasets. This spatial resolution was selected because it renders slopes sufficiently well to describe small valleys with significant shadowing effects. It will also allow to explore snow mechanical stability in future avalanche hazard forecasting applications. Indeed broader resolutions imply a too strong smoothing of terrain to represent slopes steep enough for avalanche re-

lease. The 250 m grid cell size of the simulations also enables a direct comparison with optical satellite products at the same spatial resolution. ...." Moreover, the discussion also includes the following lines addressing this issue (lines 925-931) "The results obtained in this study, i.e. slightly but significantly better skill for the distributed approach, are sensitive to the choice of the spatial resolution. Using resolution coarser than 250 m would lead to smaller differences between both spatialization approaches because the pixel elevations would be less accurate and because all the shadows would not be resolved. Conversely, higher resolutions may improve the accuracy of shadowing effects but with computational times which can become unaffordable for large areas applications."

R2: The basic theory should be better explained in the Introduction with clear examples (mention unstructured grid design). I also missed a consideration of lateral flux exchange amongst grid-cells, which has been previously applied to both semi-distributed (i.e., HRU models) and fully distributed snow models. The authors state that data assimilation, snow transport, and shading of solar radiation treatment are not possible in a semi-distributed model configuration. This is incorrect and a more careful literature review must be conducted (e.g., MacDonald et al. (2009) for blowing snow; Marsh et al. (2012) for shading). This reasoning is the basis for the authors' conclusion that "...distributed simulations... are the recommended modelling approach". is unconvincing and unsupported by what little evidence is presented and discussed. In fact, the very topic stated in the title (Distributed vs. Semi-distributed) is not mentioned in the Discussion until the 5th page of that section.

A: The reference to an "unstructured grid design" is now mentioned in line 85 and 297. We have also conducted a more comprehensive literature review including the articles cited by Reviewer 2 and some more. In this regard the introduction now includes more theoretical background and the discussion was also improved on how snow transport and terrain shadowing could be included on semi-distributed simulations. Moreover the order of the different sections of the discussion was changed. The possibility to

implement satellite data assimilation and blowing snow schemes in semi-distributed approaches are now detailed in the introduction (lines 129-140): "Semi-distributed simulations may also allow the implementation of satellite data assimilation techniques (Mary et al, 2013) but they would require specific routines for aggregating observations and would reduce potential benefits of high resolution satellite observations. Similarly, blowing snow can be simulated in the semi-distributed approach (MacDonald et al., 2009, Vionnet et al., 2018). Vionnet et al., (2018) show strong assumptions on the topography are necessary to transport snow mass from one aspect to another (virtual ridge between opposite aspect classes for any elevation band). In MacDonald et al., (2009), the model parametrization requires a discretization of the study site based on a strong knowledge of the area from previous works (McCartney et al., 2006, Pomeroy et al., 1999, 2006). Thus the transferability of these results to large domains for which detailed information on the landscape features is not available is questionable." The representation of shadows in intermediate spatial discretization is now discussed (line 1047-1052): "Other approaches halfway between our distributed and semi-distributed snowpack simulations are also showing promising results. This is the case of unstructured triangular meshes, which allow better capturing horizon- shadows of surrounding topography than the semi-distributed approach used in this work. These methods are able to improving energy balance simulation results while preserving computational costs (Marsh et al., 2012)." Finally we included a short sentence regarding lateral flux exchanges not implemented in Crocus snowpack model (lines 900-902): "Similarly other processes such as lateral heat flux exchanges amongst grid-cells are not implemented in Crocus snowpack model and thus could impact the final result of simulations (Harder and Pomeroy 2017)."

R2: The paper could be greatly improved. I encourage the authors to provide more theoretical background in the Introduction. In the Discussion, please thoroughly consider the subjective nature of model decisions (both generally and your own decisions) involved in the construction of a semi-distributed model. For example, could critical information (i.e., high-res. distributed forcing, satellite information, climatological in-

formation, and /or fully distributed model output) be leveraged to build a better semi-distributed model? The numerical parsimony offered by a semi-distributed model is not considered until Line 777! A: In the response to the previous comment, we illustrate how we improved the introduction and the discussion to discuss the possibilities to improve a semi-distributed approach by this different potential complementary information. The discussion section now incorporates a boarder analysis on the impact of model decisions and the possibility of developing better semi-distributed models integrating; satellite data, distributed forcing etc.: The numerical parsimony is now mentioned earlier in the discussion (line 877 in the document with tracked changes) as a main advantage of the semi-distributed approach and better emphasized in the abstract and the conclusion.

R2: A related issue that could be considered is the increasing need to assess potential climate change impacts on mountain cryosphere systems. This requires resolving snow and ice melt and river runoff at grid scales sufficient to resolve climate / elevation gradients, yet remaining computationally nimble to run extremely large ensembles for century-long historical and future periods. A semi-distributed model configuration could indeed help in this regard.

A: Discussion section, now also presents a short discussion on the importance of using semi-distributed simulations to analyse the impact of climate change on mountain areas. Section 5.2 now ends with the following paragraph (line 883-885): "A good example of an application in which the computational requirements have a determinant weight are ensemble simulations for projections in several climate scenarios (e.g. Verfaille et al, 2017). "

R2: It is incomplete to evaluate a snow model against snow depth alone. A true and fair assessment should be conducted using snow water equivalent, which is a more relevant model state variable for water resources applications, and is a more direct evaluation of the energy balance. Are SWE measurements available in this region? Please include them.

A: We agree with Reviewer 2 that it is incomplete to evaluate a snow model with only snow depth observations. However, a large number of Snow Cover Area and Snow Water Equivalent (i.e. glacier Surface Mass Balance) measurements are included in the evaluation datasets (see description in section 3.4). Note that unfortunately for the five stations used in the punctual evaluation, only SD measurements are available.

References:

Charrois, L., Dumont, M., Sirguey, P., Morin, S., Lafaysse, M., and Karbou, F. (2013). Comparing different MODIS snow products with distributed distributed simulation of the snowpack in the French Alps. Proceedings of the International Snow Science Workshop Grenoble – Chamonix Mont-Blanc - 2013 (Grenoble, France), 937-941

Cristea, N. C., Breckheimer, I., Raleigh, M. S., HilleRisLambers, J., and Lundquist, J. D. (2017). An evaluation of terrainâĚŸARĚĞ based downscaling of fractional snow covered area datasets based on Lidar-derived snow data and orthoimagery. Water Resources Research, 53, 6802–6820,

Durand, Y., Giraud, G., Brun, E., Merindol, L., and Martin, E. (1999). A computer-based system simulating snowpack structures as a tool for regional avalanche forecasting. J. Glaciol. 45, 469–484.

Harder, P., Pomeroy, J. W. and Helgason, W. (2017) "Local Scale Advection of Sensible and Latent Heat During Snowmelt." Geophysical Research Letters 44.19 (2017): 9769-9777.

Lafaysse, M., Morin, S., Coléou, C., Vernay, M., Serça, D., Besson, F., Willemet, J.M., Giraud, G., and Durand, Y. (2013). Towards a new chain of models for avalanche hazard forecasting in French mountain ranges, including low altitude mountains. Int. Snow Sci. Workshop Grenoble-Chamonix Mont-Blanc.

MacDonald, M. K., Pomeroy, J. W., and Pietroniro, A. (2009). Parameterizing redistribution and sublimation of blowing snow for hydrological models: tests in a mountainous

subarctic catchment. Hydrological Processes, 23(18), 2570-2583

Marsh, C. B., Pomeroy, J. W., and Spiteri, R. J. (2012). Implications of mountain shading on calculating energy for snowmelt using unstructured triangular meshes. Hydrological Processes, 26(12), 1767-1778.

Pomeroy, J.W., Hedstrom, N. and Parviainen, J. (1999). The snow mass balance of Wolf Creek: effects of snow, sublimation and redistribution. In Wolf Creek Research Basin: Hydrology, Ecology, Environment, Pomeroy JW, Granger R (eds). Environment Canada: Saskatoon; 15–30

Pomeroy, J.W., Bewley, D.S., Essery, R.L.H., Hedstrom, N.R., Link, T., Granger, R.J., Sicart, J.E., Ellis, C.R. and Janowicz, J.R.. (2006). Shrub tundra snowmelt. Hydrological Processes 20: 923–941.

Trujillo, E., Ramírez, J. A., & Elder, K. J. (2007). Topographic, meteorologic, and canopy controls on the scaling characteristics of the spatial distribution of snow depth fields. Water Resources Research, 43(7).

Verfaille, D., Déqué, M., Morin, S. and Lafaysse, M. (2017). The method ADAMONT v1.0 for statistical adjustment of climate projections applicable to energy balance and surface models. Geoscientific Model Development, 10, 4257-4283

Vionnet, V., Six, D., Auger, A., Lafaysse, M., Queno, L., Reveillet, M., Dombrowski-Etchevers, I., Thibert, E. and Dumont, M. (in prep). Influence of precipitation datasets on sub-kilometric simulations of snowpack and glacier winter mass balance in alpine terrain.