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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.

Anonymous Referee #2

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The authors present a comprehensive evaluation and comparison of a snow model run in a semi-distributed and a fully distributed manner in an alpine basin in the French Alps. The model output is evaluated against an impressive collection of measurements including point-scale depth, glacier mass balance, glacier equilibrium elevation, and satellite-derived snow-cover area. The authors conclude that both models effectively simulate snow distribution over the study period, with the fully distributed version obtaining slightly better results.

The paper is well written and relatively easy to understand. The methodology and mod-

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Discussion paper

eling is at the forefront of the field. The model application and verification is carefully crafted; however, many studies have successfully applied and validated a distributed model of seasonal snow and ice, so this aspect is not a scientific advancement. Rather, the novelty of the study is in the direct comparison of a semi-distributed and fully distributed snow model. This is an important and engaging science question.

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.

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?

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". The conclusion 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.

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 information, 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 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.

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.

References:

MacDonald, M. K., Pomeroy, J. W., & 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., & Spiteri, R. J. (2012). Implications of mountain shading on calculating energy for snowmelt using unstructured triangular meshes. Hydrological Processes, 26(12), 1767-1778.

Detailed Comments:

Abstract: Too much information on the methods . . . only two sentences on the results and conclusions.

Line 46: Mention atmospheric feedback?

Line 77: I'm not familiar with the term 'punctual' used in this manner. I prefer the term 'point-scale' as used in the Abstract.

Lines 102-105. This is incorrect. See my primary comments and references to HRU and TIN-based model application to blowing snow simulation and shading, respectively.

Line 106: The evaluation of performance shouldn't depend on the use . . . rather, determining which approach is optimal should depend on the use.

Lines 124-126: Shouldn't improved meteorological forcing fields also improve the meteorological forcing for semi-distributed / representative slopes?

Lines 132-135: This is not well explained. It may be better to state that the careful verification of the point-scale simulations helped to better interpret the results and comparisons between the semi-distributed and fully distributed models.

Line 208: Note in this section how many 1-D simulations were used in the semi-distributed run.

Lines 276-287: This entire section could be removed (not directly relevant to the results or conclusions).

Line 248: soil "moisture"? "humidity" suggests water in vapor phase.

Line 254: Explicitly state the specified ice thickness.

Lines 318-323: This information could be removed.

Line 388: the model doesn't technically "evolve" ice, but presumably only melts it.

Line 453: How is an underestimate inferred from this figure? I notice a 50% overestimate of snow depth in the Jan. event.

Line 476: "In winter the simulation . . .:" what simulation, exactly?

There are a number of 'in prep' citations, which are not relevant until published.

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