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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hyper-resolution ensemble-based snow reanalysis in mountain regions using clustering" *by* Joel Fiddes et al.

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This paper presents a new data assimilation framework to generate snow reanalysis based on MODIS snow products. The method was tested using various datasets in Switzerland and the results are very convincing. The novelty is not the data assimilation scheme (particle batch smoother, Margulis et al. 2015) or the model (GEOtop, Endrizzi et al. 2014), but the method to optimize the numerical cost of the data assimilation by using a clever spatial reduction of the simulation domain by topographic clustering. A limitation of the clustering approach is that it does not allow an explicit representation of lateral transport (avalanches and wind transport), whereas these processes are known to be significant at "hyper-resolution" (here 30 m). In addition, if I correctly understood it implies that the DA assimilation can mix the contribution of spatially remote pixels in

the weighting of the particles.

The data assimilation pipeline was implemented at three different "experimental scales". I agree with the first referee that the approach is interesting and that previous work may be better acknowledged. I do not have any major comment (but many minor comments, see below), except that in my opinion the paper would have been easier to read if only one of the DA approaches was described and evaluated (including at different scales). In particular, the "coarse scale DA" was only briefly illustrated while it is in my opinion the most promising approach.

P1L18: "Spatial resolutions of 100 m are commonly recommended for modelling of land surface variables such as snow cover or surface temperature in complex terrain": the authors may also check Baba et al. (2019) where we specifically studied this topic (see below).

P3L1: what is hyper-efficient?

P3L6: "earths surface"

P4L25: this idea was surely introduced before 2018

P5L14: these parameters were obtained in Greenland. This should be explicitly stated in the method and discussed later.

P5L27: I do not understand why the TopoSUB approach is not compatible with an iterative approach and sequential resampling of the particles.

P6L23: Thirel et al. (2013) do not use a threshold to convert SWE to SCA but the snow depletion curve of Zaitchik & Rodell (2009). This point should be clarified.

P7L7: It is odd to derive the MODIS error from a study in Svalbard while there are multiple evaluation studies of MODIS snow fraction in temperate alpine regions which are more similar to Switzerland including the original paper by Salomonson and Appel (2006).

P7L21: Masson not Mason

P7L21: "as the as"

P8L28: cumulative distributions of what?

P10L5: It is not sure if there is a need to run a snow model at 30 m resolution especially if it does not represent wind transport and avalanches. We showed that a 250 m resolution can be sufficient to capture the main energy balance processes (Baba et al. 2019). If the resolution is set to 300 m then N_r becomes 10ˆ6.

P12L2: "which are an"

P12: 400 mm, 350 mm and 826 mm, it may be a coincidence but why not using the same precision?

P14L2: the noise does not come from the NDSI-SCF relationship

P14L14: another important limitation is the poor accuracy of the MODIS product in dense forest areas. In particular, I wonder if it could be the cause of the DA failure observed near Zermatt rather than the “urban effect”. In any case the consequence of the lack of reliable snow detection in dense forest areas must be discussed since the DA scheme is presented as applicable at global scale.

P15L23: The reference for the Theia snow products is Gascoin et al. (2018).

P15L30: "1 km not ideal" but the results show that 500 m is useful.

P16L23: "data was obtained from We"

P17: I tried to explore the code in the Github repository but it contains tens of R and Python files from multiple projects; it would be a great addition to the paper if the code was a bit more documented to allow reproducing the results of this paper or even better to allow other interested people using the DA scheme in another study area (just a suggestion!).

P18 Endrizzi et al. not a discussion paper

Figure 3: figure labels are too small.

Figreu 8: what does represent the spread? (full ensemble?)

Figure 11: top panels are HS not fSCA.

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