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## **Comment on tc-2021-322**

Bertrand Cluzet (Referee)

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Referee comment on "Large-scale snow data assimilation using a spatialized particle filter:  
recovering the spatial structure of the particles" by Jean Odry et al., The Cryosphere  
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Large-scale snow data assimilation using a spatialized particle filter:

recovering the spatial structure of the particles

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and

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Review by Bertrand Cluzet

GENERAL:

I discovered and read this paper with a great pleasure. Data assimilation of snow observation, in particular using the Particle Filter (PF) has been a growing topic in the snow/hydrology modelling community over the last five to ten years. However, this algorithm suffers from two strong limitations when it comes to large scale, data-scarce problems (which are the general case in the field). The first one is the curse of dimensionality, which can be solved by localizing the PF. But this solution generates a second strong limitation: individual members of localized PFs exhibit discontinuous spatial fields and noisy spatial correlation fields which are often detrimental for the PF performance itself.

Capitalizing on a localized PF variant using a spatial interpolation of the PF weights from Cantet et al., (2019), the authors efficiently introduce an approach coming from the hydrological modelling, the Schaake Shuffle to solve for both limitations: the localisation mitigates the curse of dimensionality, and the Schaake Shuffle is used to enforce the spatial structures of a deterministic run in the individual ensemble members in an elegant way.

Overall, I find that this paper is well within the scope of the journal, and has the potential to be a significant contribution to the snow/hydrology data assimilation community and beyond, because they address an important problem in a convincing and elegant way. I must admit, however, that I'm not satisfied with the theoretical justifications for the use of the interpolation of the weights within the PF from Cantet et al. (2019) (IDWPF), instead of the classical localised (LPF, e.g. Farchi and Bocquet, 2018). I would ask for more justifications, or a comparison between the LPF and the IDWPF.

The scientific quality of the writing is sometimes lacking rigor, especially in the Sections 1-3. I would ask for a significant effort on that. Nevertheless, I really appreciated the compactness of the paper and the efficiency of the results and discussion sections, which make a very clear and straightforward demonstration of the author's point.

To wrap up, I see a lot of potential in this paper, and despite my concerns, I am very confident that the authors will be able to address my comments in a revised version of the manuscript. Please find below some details on my main comments. I'm pleased to provide an annotated pdf version of the manuscript with comments and suggestions throughout.

## MAJOR comments

(1)

The authors use an interpolation of the particle filter weights (IDWPF) from Cantet et al., (2019) rather than a classical PF localisation (LPF) (see the review from Farchi et al., (2018)). I agree that the basic idea as formulated in 71-73 (a good-performing particle at a close location must also be good locally) resembles the theory. But in the classical LPF, based on Bayes theorem, and assuming equal prior weights, the posterior weights are computed by multiplying the likelihoods of the particles at the different (independent) observation sites (Eq. 27 from Farchi et al., (2018)) and then, normalising. Here, the IDWPF averages the normalised likelihoods of the particles at the different observation sites. There is a substantial conceptual difference here: averaging instead of multiplying. I suspect that this results in less sharp and potentially suboptimal (more conservative) PF analyses. For example, if a particle is given a zero likelihood at one location, the LPF will reject it, while there is still a chance for it to survive in the IDWPF.

Moreover, the arguments in Cantet et al., and the present manuscript used to justify the use of the IDWPF instead of the LPF failed to convince me: the LPF also proposes a 'tapering' method to smoothly reduce the influence of the observations with the distance (Eqs. 28-29 from Farchi et al., (2018)).

To wrap up, I'm not saying that the IDWPF method is wrong, and should be rejected. I can actually imagine that it could be more resilient to outliers in the observations, and its conservativeness could be an advantage. There may also be references in the literature to serve as a base for the IDWPF. But in the present form, the justifications provided to substantially deviate from the main theory, the LPF, are too weak for me. I would suggest to make a considerable effort on justifications, or even to compare the IDWPF with the LPF. The latter would have the benefit of significantly increasing the potential impact of this paper thanks to the use of a more 'orthodox' method.

To help with the discussion, I'm pleased to provide a toy example comparing the IDWPF and the LPF in the form of a jupyter code attached or publicly available at:

[https://github.com/bertrandcz/da\\_notebooks](https://github.com/bertrandcz/da_notebooks)

(2)

The scientific quality and rigor of the writing is often not satisfactory in its present form, in particular in Secs. 1-3:

Even though there is no doubt that the authors have a deep understanding of the PF and its terminology, there is sometimes a lack of rigor in the terminology and approximations that make several sentences turn wrong, and arguments fall short (e.g. l. 54, l.207-208, l. 218-219).

Even though the arguments are there, the logical formulation behind certain paragraphs is too loose to be convincing. I have no doubt that the authors can address that, but a considerable effort is required here.

There are some approximations in the description of the literature which may induce the reader into having misconception on the references (e.g. l. 69, l. 227-228), and change the conclusions of some paragraphs

the observation dataset and study area (Sec. 2) must be described with more details and rigor.

I'm pleased to provide several suggestions on these points in the attached pdf.

MINOR COMMENTS:

(1)

With the Schaake shuffle as used here, the authors enforce the spatial distribution of individual particles to match those of the model, (instead of historical observations): but by doing so, don't we miss the opportunity to adjust the ensemble to the observed spatial structures? I'd be curious about overlaying Figs 3. and 6 with observed (in-situ) values to assess that.

(2)

The abstract is lacking of a general scientific context to start with. I think that the start is too technical for the scope of TC, the notion of 'particles' should be introduced, and given the level of technicity of the paper, a brief sentence describing the particle filter might be required in the abstract, in particular to make the need for a reordering possible to understand. Details on the ensemble construction might be appreciated also. Would benefit from a more rigorous description of the observations and validation data sets.

(3)

When computing global metrics (Secs. 4.2 and 4.3, Fig 7,8 and 9), it could be fair and interesting to compare the assimilation products with their ensemble counterpart without assimilation, not only the deterministic run (called 'open loop' in the paper). Ensembles are often favored compared to deterministic runs in terms of RMSE and SWE, and it would enable the authors to put into perspective the impact of the assimilation in terms of spread-skill and CRPS.

References:

Cantet, P., Boucher, M.-A., Lachance-Coutier, S., Turcotte, R., and Fortin, V.: Using a particle filter to estimate the spatial distribution of the snowpack water equivalent, *Journal of Hydrometeorology*, 20, 577–594, <https://doi.org/10.1175/JHM-D-18-0140.1>, 2019.

Farchi, A. and Bocquet, M.: Review article: Comparison of local particle filters and new implementations, *Nonlinear Processes in Geophysics*, 25, 765–807, <https://doi.org/https://doi.org/10.5194/npg-25-765-2018>, 2018.

[https://github.com/bertrandcz/da\\_notebooks/](https://github.com/bertrandcz/da_notebooks/)

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2021-322/tc-2021-322-RC1-supplement.pdf>