The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-286-RC2, 2018

The Cryosphere Discussions

Interactive comment on "A Particle Filter scheme for multivariate data assimilation into a point-scale snowpack model in Alpine environment" *by* Gaia Piazzi et al.

Anonymous Referee #2

Received and published: 9 March 2018

General comments

In this study, the authors test different particle filer setups for jointly assimilating a set of snowpack variables, such as snow depth, SWE and snow surface temperature. The study is a valuable contribution to previous studies, which have assessed the performance of the particle filter for the assimilation of only one snowpack variable in most cases. However, the study needs some improvements before final publication. Four important issue are:

- The authors only use 100 particles when testing the performance of the filter. In some situations, such a low number of particles might give good filter performance.

However, in the case of multivariate assimilation of several variables, more particles may be needed. Therefore, I would urge the authors to test the sensitivity of the filter performance by varying the number of particles. The authors should also present results showing the effective sample size after each update in order to test whether the number of particles is sufficient.

- I could not find any information about the uncertainty of the different measurement. The specification of the observation uncertainties is critically important for the filter behavior and should be reported.

- Some of the figures contain too much information, foremost figure 4, 6 and 10. The large number of results shown in these figures makes it hard to see which filter setup performs best. This is further complicated by the different axes limits used in the figures (see for example the performance metric NER for CPD and SWE in figure 4, 6 and 10). Overall, I think the presentation of the results would improve by removing some of the performance metrics. The conclusions from this study may also be clearer if the authors could summarize their results in fewer graphs.

- First, the result sections contain more discussions about methods, rather than presentations of their results and quantitative comparisons between them. Second, the authors often states that one filter setup performs better than another setup. However, how large those improvements are is not presented in numbers between the setups. It is therefore very hard to judge whether the simulation results actually improved.

Specific comments

Abstract: I think the abstract lacks clear "take home messages". What are the most important results and conclusions obtained in this study?

Page 2, Lines 1-8: It is also possible to include observation uncertainties using the optimal interpolation scheme.

Page 2, Lines 11-15: I think the Enkf was not invented "with the aim of overcoming the

inaccuracy of the linearization procedure", but to avoid the need for linearization of the system equations, which in many cases is impossible or simply unfeasible.

Page 2, Lines 24-32: Please state the study goals in more detail using, for example, research questions or hypothesis. Perhaps remove the summary part stretching from line 27 to 32.

Section 2.1: The Torgnon site description includes information about the measurement equipment, whereas the other site descriptions lack this information. I think it would be better to present the same amount of information for each of the field site. If including information about the measurement equipment for all field sites, perhaps better add a table to the paper with this information. Furthermore, the Torgnon site description includes some numbers about climatic conditions. Such information should be included for the two other field sites as well.

Page 7, Lines 26-28: I do not understand this part of the sentence: "a resampling procedure is frequently introduced to restore the sample variety through a Markov chain chaotic Monte Carlo". What is a "Markov chain chaotic Monte Carlo"?

Equation 8: The effective sample size should be calculated using the square root of the weights.

Section 2.3.2: What uncertainty was used for the different observations? This information is essential and must be included in the manuscript.

Page 8, Lines 19-20: Why was not longwave radiation perturbed?

Page 9, Line 14: I do not understand this sentence: "Therefore, tuning parameters are properly set to guarantee a significant variance of the parameters distribution."

Section 2.4.2: How are the parameter values perturbed? By additive or multiplicative noise?

Page 10, Lines 10-12: Perhaps remove: "The SWE is one of the most relevant snow-

related quantities from a hydrological point of view, since its accuracy in estimate strongly impacts discharge simulations".

Equation 11 and 12: These two equations are probably not needed since the two metrics are very common.

Page 12, Lines 1-21: In this part of the manuscript, I think the authors are mainly discussing filter degeneracy, which is a well-known problem for these kind of applications, in particular when the dimensions of the observation space is high. Please shorten this general discussion by citing relevant literature (e.g. Ades et al., 2013), and provide results more specific to the actual study. Whether such a degeneracy occurs can be assessed by either calculating the efficient sample size, or qualitatively by plotting the time series of the particle spread after assimilation. It would probably be good to include one or both of those analyses to the result sections.

Ades, M. and P. J. Van Leeuwen, 2013: An exploration of the equivalent weights particle filter. Quartely Journal of Meteorology, 139, 820-840.

Page 14, Line 8: In many places throughout the manuscript the authors refers to "parameters resampling" or similar terminology. I do not understand this terminology, and I am not sure it is a correct since particles are being replicated or terminated in the resampling step, and parameter values are only affected indirectly. Please consider rephrasing.

Page 14, Lines 8-10: Please add some quantitative measures on how large this improvement actually is.

Page 14, Lines 10-11: How are the model parameters better estimated? What are the best values of the parameters?

Page 15, Lines 8-20, including Figure 8: The spread between the particles in the figures seems very small, indicating sample impoverishment. I suspect that the number of particles is not high enough for these kind of experiments, or that a more appropriate

filter technique for high dimensional problems should be used requiring fewer particles. Please analyze whether sample impoverishment is occurring or not, and provide appropriate results from such an analysis in the manuscript.

Page 16, Lines 9-11: Provide quantitative results on how much the simulation improves.

Page 16, Lines 9-20: What observation uncertainty was assigned to the “proxy information of snow mass-related variables”?

Page 16, Line 30: I think it should be “perturbations of parameters” instead of “parameter resampling” as mentioned above.

Conclusions: The conclusions mainly lists problems with the SIR-PF for the current application. In addition, I would like to know the answer to questions like these: What filter setup worked best for the current application? Does the filter work better for sites with low (CDP) or high (WFJ) snow amounts? What assimilation frequency worked best? Such information is currently missing in the conclusions.

Technical corrections

Page 1, Line 10: Perhaps remove “multivariate Sequential Importance Resampling”.

Page 4, Line 34: Probably wrong reference: Wever, 2015?

Page 10, Lines 15-21: All variables are not explained (e.g. Exp, Obs). It is pretty clear what they mean, but I think for completeness they should be described.

Page 12, Line 14: Change to: “Firstly, it is intended to properly identify the parameters affecting the model simulations most” or something better.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-286, 2018.