The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-286-RC1, 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.

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Received and published: 6 February 2018

General remarks

- I think that this manuscript is a very significant contribution in the field of data assilimation for snowpack modelling. The originality of this paper comes from the multivariate assimilation in the context of the particle filter algorithm. Another added value is the multi-sites application whereas recent applications of the particle filter in snowpack modelling were only focused on one specific site. The multivariate assimilation exhibits some promising advantages but also some discrepancies and challenges which have to be accounted for in the development of such systems. The paper gives a very interesting overview of these positive and negative effects, and their links with the model structure and with the frequency of available observations.

- The introduction gives a very good overview of the position of this work among state-of-the-art methods.

- I have the feeling that the structure of the paper could be a bit improved before publication in two ways:

1/ First, the results section is a bit too long because it includes some details of the methodology itself which should be described in section 2. This is especially the case because the section already includes both results description and discussion. In particular, the beginning of sections 3.1.1, 3.1.2, 3.2.1, 3.2.2 and 3.3.1 introduce many methodological elements which could be detailed in section 2. I would suggest a paragraph 2.6 describing all the assimilation experiments and their objective. Thus, the presentation of results can become more concise.

2/ Then, the authors should better emphasize the lessons of their work for current and future developments of data assimilation in snowpack modelling systems, in a more general point of view than their particular study. This could be done either by introducing a dedicated discussion section either by adding complementary informations and perspectives in the conclusion. For example, the challenge of spatialization at larger scales should be mentioned because it will be a major issue for hydrological modelling. Then, can the authors give general recommendations for the implementation of data assimilation algorithms further than their particular case? From their results, do they recommend to always include parameter perturbations? Do they recommend to include parameter perturbations this way or to test other methods? Do they recommend to apply restrictions in terms of availability of observations to decide to assimilate a given variable? Do they recommend a minimal model structure to decide to assimilate some specific variables ?

Major issues

- My main concern is the fact that the skill of data assimilation is assessed by the comparison of deterministic scores between ensemble simulations including data assimilation and the deterministic reference simulation which is forced by in-situ meteorological measurement. However, in the real world, the quality of the meteorological forcing will be much lower that the quality of the forcing at the three stations of Col de Porte, Weissfluhjoch and Torgnon. Therefore, it makes sense to use perturbations which are not really representative of the uncertainty of these meteorological dataset but more typical of common meteorological errors. Although it is not clearly said in the paper (section 2.4.1), this is what is done here because the error statistics of Charrois et al, 2016 and Magnusson et al, 2017 come from a comparison between a meteorological analysis and in-situ observations. These errors do not represent the observation error, they represent the meteorological analysis error. As a consequence, data assimilation is expected to reduce the meteorological error introduced in the forcing. But it is very demanding to expect from data assimilation to come back to results of the same quality as simulations forced by in-situ measurements when perturbations higher than the observation uncertainty are introduced. There are several options to solve this issue:

Option 1) using lower perturbations consistent with the meteorological forcing. The main limitation will be a low spatial transferability of the results as very few stations provide this quality of meteorological data.

Option 2) using a meteorological forcing of lower quality more consistent with the perturbations. This option would require to run again all simulations.

Option 3) changing the evaluation metrics to provide a comparison of skill between 2 ensembles, the first one with the perturbations but without assimilation and the second one with assimilation. This option does not imply to change the simulation runs, it only requires to compute new evaluation metrics. Therefore, I would recommend this option for this work. The easiest way will be to keep the same metrics but to apply them to the ensemble without assimilation instead of the reference run without perturbation. Thus, the blue points in Fig. 4, 6, 10 will be replaced by a boxplot which can be compared with the red boxplot (ensemble with assimilation). Note that it would also be possible to use ensemble metrics instead of deterministic metrics. For example you could compute the Continuous Ranked Probability Score (CRPS) of the ensembles with and without assimilation.

- The second major issue is the fact that the scores are presented in a very high number of subplots (Fig. 4, 6, 10) which are very small. The comparison of the different experiments is difficult with these figures due to the lack of more synthetic metrics allowing a quicker comparison of the experiments. It is probably interesting to see the interannual variability of the scores for one example but I do not think that this is necessary for all scores, sites, and experiments. It is impossible to analyze in details all the scores provided in these 3 figures. Page 16, line 11, clearly the authors do not need all the metrics of Figure 10 for such a general conclusion! I think the authors should try to present multi-year scores in a synthetic table allowing a quick and representative overview of the model skill for the different experiments.

Other remarks

Page 1 line 24: It would be useful to also mention that snow models are based on uncertain parameterizations and parameters (Essery et al, 2013; Lafaysse et al, 2017). Thus, it would become more natural to introduce further the perturbations of model parameters.

Page 1 line 28: It is not obvious that there is a link between the complexity and the skill of the data assimilation algorithm.

Page 1 line 29: "they allow to process" → they allow taking benefit from

Page 2 line 4: snow models (plural)

Page 2 lines 5-8: Optimal interpolation also allows accounting for observation uncertainty.

Page 2 line 15: EnKF can also be based on ensembles obtained from other methods than the Monte-Carlo approach.

Page 2 lines 21-30: The authors could also add that in the context of more complex models, EnKF is also complicated by the need to compute averages of the snowpack profiles. This can be a challenge for the models based on a lagrangian discretization with a variable number of snowpack layers.

Page 2 line 33: "the full prior density" → coming from the ensemble

Page 3 lines 18-20: Please add "at the local scale" because these conclusions might not be true in spatialized simulations.

Page 4, lines 9-10: I do not agree that instrumental biases are representative of observation uncertainties. Even on these well-maintained sites, environmental errors are the prevailing source of uncertainty. Therefore, the instrumental accuracy provided by manufacturers does not provide a good assessment of observation error. For example, the radiation sensors are generally more affected by environmental issues (hoar or snow on the sensor) than for instrumental accuracy. Similarly, precipitation measurement is mainly affected by undercatch in case of wind.

Page 4, line 13: "all the requirements" → to force and evaluate a snow model

Page 4, lines 23-28; page 5, lines 2-7: I think that it is not necessary to provide so many details about the available observations at Col de Porte and Weissfluhjoch. The observations which are not used in this paper (temperature profiles, ground temperatures, liquid water content, runoff, etc.) do not need to be described.

Page 5, line 30: Can you detail what represent the 2 distinct layers? I assume that there is a surface layer? Does it have a fixed depth?

Section 2.2 Can you explain how the energy balance is computed without the availability of a longwave radiation forcing?

Page 6 line 27: model input vector → meteorological input vector

Page 6 line 32: why do you prefer here the word ÂńnoiseÂż to ÂńerrorÂż? I think it would be more accurate to talk about observation error.

Page 7 line 4: missing space after t-1

Page 7 line 12: This statement could be more general. Indeed, as mentioned before, the Monte Carlo sampling is not the only method to build an ensemble.

Page 7 lines 17-18: I followed the formalism until here but I do not fully understand the sentence ÂńParticles are drawn from a known proposal distribution according to the Sequential Importance Sampling approachÂż. Can you clarify this part so that it can be understood without reading the references associated with the SIS approach?

Page 8 line 6: I think that the reference to Fig. 2 in the text does not take all the benefit of this figure to clarify the methodology. I would suggest to refer separately to the different subplots in the text to be more illustrative. Can you also comment the reasons which explain the slight differencies between Fig 2b (weights) and 2d (number of resampled particles)?

Page 8 equation 10: Can you explain by words the practical implication of this equation?

Page 8 line 23: Âńadditive stochastic noiseÂż Can you detail the process applied for precipitation? I assume it is probably not possible to apply directly an additive noise in that case? Is there a different treatment between occurrency and intensity?

Page 9 line 1-2: I agree with this remark. However, following my first major remark, the perturbations used in this study are not representative of the error of the in-situ measurements at Col de Porte.

Page 9 lines 4-6: The perturbation of model parameters is introduced through a very "mechanical" point of view for the data assimilation algorithm. I think it would be useful to remind that errors exist in the snow model itself and that it is natural that perturbations of the meteorological inputs are not sufficient to cover all uncertainty.

From page 9 line 29 to page 10 line 3: Are the authors aware that weekly measurement of bulk density are available at Col de Porte at 3 different places in the plot? These data should be preferred for data assimilation than a computation from SWE and snow depth. Indeed, very unrealistic values are obtained with such a computation because the spatial variability in the plot is responsible for a different accumulation between both sensors.

Page 10 Line 14 Can you provide the time step and the hour used for the surface temperature?

Page 10 Line 16 "snowless periods are neglected" How do you define the snow free periods? Is it only based on observations? This choice can lead to eliminate some data for which some particles do have snow and to include data for which some particles do not have snow. This is a usual issue in the evaluations of a snowpack model so please be accurrate on that point.

Page 10 line 28: "spurious trends" → unexpected biases

Page 10 lines 25-28: The goal of section 3.1.1 should also be to check if the perturbations are able to realistically depict the uncertainty of snow simulations.

Page 11 line 5: It would be interesting to notice that despite unbiased perturbations, the control run is not identical to the ensemble mean.

Page 11 line 6: A more comprehensive assessment of the fact the control run is included in the ensemble spread would be to use Talagrand rank histograms over the whole period to ckeck that the control run has a random position in the ensemble. Note that it would be even more informative to check if the observation is included in the ensemble spread with a random position. This would be very useful to strengthen the discussion between lines 1-21 of page 12.

Page 12 line 7 "their measures" → the measure of the corresponding variable

Page 13 line 15 "uselessly" Can you be more explicit? Larger computation requirements without a significant improvement of the spread and further of the filter efficiency.

Page 13 lines 18-20: It is unclear if more parameters were also tested in a preliminary sensitivity analysis and which specific metric was used to select the parameters to disturb. Note that the parameters could also be chosen a priori based on previously published sensitivity analyses of other snowpack models.

Page 13 lines 22-23: The albedo parameters could also have an impact of snow mass during the melting season.

Page 13 line 30: Can you provide a better description of Fig 5 in the text? The fact that the spread of viscosity is increasing in the melting period should be noticed. Does it suggest that melting issues in the model are compensated by this parameter?

Page 15 line 28: Can you give more details about the new density function and how it differs from the original relationship between SWE and snow depth in your model?

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