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Interactive comment on "Ensemble-based assimilation of fractional snow covered area satellite retrievals to estimate snow distribution at a high Arctic site" *by* Kristoffer Aalstad et al.

Kristoffer Aalstad et al.

kristoffer.aalstad@geo.uio.no

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Reply to Reviewer 2

We are grateful to the reviewer for the thoughtful comments and suggestions to our manuscript. We have compiled a revised version and in the following provide a point-by-point reply to all issues raised. The reviewer's comments appear in bold font and our replies in normal font. Excerpts from and changes to the manuscript are quoted in italics. Page and line numbers refer to positions in the original manuscript.

Ensemble-based assimilation of fractional snow covered area satellite retrievals to estimate snow distribution at a high Arctic site

The Cryosphere, Aalstad et al., 2017

This paper shows an analysis of the results of three different assimilation algorithms applied to snow variables (in particular SWE, fSCA and sub-grid coefficient of variation) over an Arctic study site. The assimilation algorithms used are the Ensemble Smoother (ES), Particle Batch Smoother (PBS) and a newly introduced Ensemble Smoother Multiple Data Assimilation (ES-MDA) technique. The results show significant improvements in all evaluation metrics for the ES-MDA technique, matching or improving the results obtained using the ES and PBS. The ES-MDA is more robust as it avoids degeneracy and other problems of the other two techniques however this comes at an expended computational cost.

The paper is well written (albeit it needs more work in terms of grammar/phrasing, I recommend one final review by native-english speaker) and very clear. The methodology section might be improved by including examples of the method using figures. As it is right now is very mathematical, which is fine but reduces the possibility of understanding the workings of the method by other researchers on the field. The literature review is very comprehensive and might be improved if condensed. The paper further illustrates the extreme utility of data assimilation frameworks in the context of snow process estimation and I recommend it for publication after minor revisions.

Following the reviewer's suggestion the entire paper has been revised for its grammar/phrasing.

We agree with the reviewer that our presentation is quite mathematical. In a sense, this is unavoidable given the mathematical nature of data assimilation. With Figure 3 we provide a more schematic overview of the work flow in the methodology. We find that even after the revisions the manuscript is still quite long and thus chose to avoid adding additional figures.

The literature review has been condensed by removing many of the fine details regarding the results of different studies. We also refrained from stating the spatial resolution of the cited studies.

Specific comments:

Page 2

2-3: The amount of smoothing depends on the type of terrain - wouldn't expect this effect to be significant beyond smoothing microtopography (i.e., 1-2 m vertical scale).

We have removed this sentence.

13: Probably only precipitation and wind are space-time variant, topography and vegetation shouldn't be considered as dynamic. Radiation is also space-time variant however the direct component climatology might be relatively invariant every year - though I would expect that for high latitudes this is not necessarily

true,

Yes, topography and vegetation are relatively fixed. In clear conditions the direct shortwave component of the radiation is mainly fixed by solar geometry and the local topography and is as such relatively invariant from year to year in the absence of clouds. Still, clouds are common in the Arctic and along with variations in the surface albedo this can make net radiation a highly dynamic variable. To clarify, the sentences starting on P2 L12 and running up to the start of P2 L15 have been reformulated to

"The primary controls on the distribution and variability of SWE are topography, vegetation, precipitation, wind, radiation and avalanching (Sturm and Wagner, 2010; Clark et al., 2011). While topography and vegetation are relatively fixed in time, the other controls vary strongly over a range of spatiotemporal scales."

Page 4

17: Maybe it is worthy citing Cortés et al. (2016; 2017) for a more direct comparison with PBS metrics derived over similar study regions. Both papers include similar validation data (snow surveys), while Margulis et al is focused on point-data (stations).

We agree that both Cortés et al. (2016) and Cortés and Margulis (2017) are valuable references for probabilistic SWE reconstruction in sparsely instrumented regions and have thus added the following to P4 L19

"Cortés et al. (2016) applied the same PBS framework to construct a 30 year reanalysis of SWE over 6 instrumented basins in the Andes. Cortés and Margulis (2017)

subsequently adopted this approach to perform a 31 year SWE reanalysis over the entire extratropical Andes."

Page 5

31: Define undulating.

By undulating we mean gentle topography with small hills. The sentence was changed to

"All sites feature gently undulating topography with small hills and surfaces characterized by patterned ground features, leading to strong differences in snow cover due to wind drift."

32: Typo (ground)

Thanks for spotting this typo. It has been corrected.

Page 9

7: Please clarify what do you mean by this term? Also - clarify what "external" processes are not considered (wind redistribution?)

By this we mean any process occurring inside the snowpack itself. Since refreezing is treated at sub-daily resolution and metamorphism is treated implicitly by the snow

albedo parametrization we have reformulated the sentence to

"Many internal snow processes (occurring inside the snowpack), including heat conduction and melt water percolation, are omitted. In addition, several external processes such as sublimation and deposition are ignored."

Wind redistribution is treated implicitly by the probabilistic SDC of Liston (2004) through a non-zero peak coefficient of variation (χ) that accounts for a non-uniform peak subgrid SWE distribution (SSD). The shape of this peak SSD (and thus χ) will be partly controlled by wind redistribution during the accumulation season.

Page 10

26: Is there a range defined for this parameter?

Yes, in Table 4 (previously Table 3). A reference to the table was added to the text:

"Q_0 is a perturbation parameter (see Table 4) that is updated in the assimilation,"

Page 11

10: Is the daily time step a result from aggregating internal hourly calculations?

Yes, the forcing is aggregated from subdaily to daily resolution as discussed in Section 3.1.3.

Page 13

4-5: It would be useful to include a quantification of how many images were available per assimilation season for each site. How were clouds identified and masked out?

A new Table (Table 3) has been added that lists the number of available scenes per melt season for both MODIS and Sentinel-2 for each study site.

For MODIS, clouds were masked out automatically by the MODIS cloud mask (see Riggs and Hall, 2016). The following sentence has been added to Section 3.2.1:

"We average over all the pixels for each day and study site (see Figure 1). This average is only taken if cloud free (as determined by the MODIS cloud mask) retrievals are available for each of these pixels."

For Sentinel-2, clouds were masked out manually in the scene selection as specified in Section 3.2.2.

Page 15

7: Curious if the use of independent multiplicative biases for accumulation and melt would result in inconsistent accumulations? (For example b*M>b*P?)

This is an interesting point. We have added the following to the discussion (Section 5.1):

"An inherent equifinality problem (see Beven, 2006) exists in SWE reconstruction since different perturbation parameter sets can provide similar results. For example, if the prior fSCA melts out earlier than the observations this could be due to the prior precipitation having a negative bias, the prior melt having a positive bias or a combination of these two. The opposite would be true if the prior fSCA melts out too late. It is not possible to resolve this equifinality problem with observations of fSCA alone. A key assumption in deterministic SWE reconstruction is that the melt flux is more constrained than the precipitation so that uncertainty in the melt is ignored (Slater et al., 2013). We perturb both the precipitation and the melt, although the latter is assigned a lower uncertainty (Table 4). Through the assimilation we obtain snowmelts that are consistent with the observed snow cover depletion. The close match of the posterior peak mean SWE estimates to the independent field measurements (Figure 7) suggests that the assimilation yields consistent accumulations and that the inherent equifinality problem is of minor consequence."

8: When you mention constant multiplicative biases - does this mean the bias is unaltered throughout the year?

Yes, this has been clarified in the text where we have changed *"constant multiplicative biases"* to *"constant multiplicative biases (fixed throughout the annual integration)"*.

13: The PBS requires that the ensemble includes the observation, thus if no bias is assigned a priori then the PBS might not be applicable as some degree of bias correction is needed.

Both of the prior bias parameters are modeled as lognormal random variables with unit mean but a non zero variance. So a considerable bias is assigned a priori for some of the ensemble members. We agree that the PBS requires that the prior ensemble encompasses the assimilated observations and we view this as a weakness. For example in 2008 for Bayelva the prior fSCA ensemble is positively biased and does does not encompass the observations, so the PBS performs poorer than the ES-based schemes. We could have changed the prior mean of the bias parameters for this particular year but decided not to. In the application of Bayesian data assimilation the prior should always be set without knowledge of the observations that are considered in the likelihood otherwise it is by definition not a prior. See also the reply to the comment concerning Page 28 L3-4.

Page 20

6: The reduction in spread is a direct consequence of any assimilation algorithm, it would be more useful to assess if the constrain in uncertainty of the posterior is consistent with the observations (i.e., are you underestimating uncertainty after assimilation?)

We agree with the reviewer. As such, the following paragraph was added to the end of Section 4.1:

"In ensemble-based data assimilation the spread of the posterior ensemble should represent the uncertainty. To verify this one can compare two metrics: the residual, i.e. the instantaneous posterior RMSE of the ensemble relative to the corresponding independent field measurement, and the ensemble standard deviation (e.g. Evensen,

2009). For this comparison we define the relative residual as the ratio of the residual to the standard deviation. Ideally this ratio should have a value of 1 which indicates that the two metrics are equal so that the posterior ensemble spread accurately captures the estimation uncertainty. For the fSCA, peak mean SWE and peak subgrid coefficient of variation the average (over all available field measurements) relative residuals were 2.22, 1.53 and 1.66 respectively, so the posterior ensemble underestimates the uncertainty. This effect has been extensively described by Evensen (2009), it arises in part because of model structural errors related to neglected physical processes (Section 3.1). Still, the assimilation is generally able to simultaneously (but not to the same extent) reduce the spread and the error in the ensemble (Figure 4). "

Page 21

A scatterplot would be useful to compare the posterior results for all methods. Including the stats is correct but scatterplot allows for more context.

We agree and have included a scatter plot for a single run of all three schemes and the prior. This is included for orientation as a new Figure 6. The following line was included at the end of P23 L10:

"The scatter plots in Figure 6 visualize the performance of the prior and all the considered DA schemes relative to the field measurements."

Page 26

Table 5: A perfect correlation of 1.0 was obtained? Would be useful to have the

scatterplots in order to inform the reader with more details on the results.

This perfect correlation is based on a comparisson to just 3 observations. Because of the low number of observations a scatter plot is not informative and would unnecessarily add to the length of the manuscript. We have added the following cautionary statement regarding the limited number of observations used in this evaluation to Section 4.3 (P26 L9):

"We emphasize that this evaluation is based on the only 3 available field measurements of μ and χ in 2016 (from the snow surveys), so that these preliminary results need to be consolidated by future studies with more validation data."

Page 27

32: It is very difficult to compare RMSE across studies due to the differences in methodology/data. I would stick to the comparison performed within the paper as it allows for more controlled conditions.

We agree and have removed this comparison.

Page 28

3-4: More than biased, if the prior ensemble doesn't cover the observations then the PBS would be unable to replicate the observation. Bias in the ensemble per se is not a problem for the PBS. The comparison between PBS/ES from previous papers with the current method is not as straightforward, asides from the

obvious differences in regions there are differences in validation methodology and particularly in the number of fSCA measurements assimilated. Landsat fSCA assimilation results in 10-15 observations per year, while MODIS probably results in an order of magnitude greater.

To clarify, we have changed this sentence to:

"Thus, if the prior ensemble is so biased that it does not encompass the observations, the PBS is incapable of correcting the posterior towards the observations outside the bounds of the prior."

For the next part of the comment we assume that the reviewer is referring to the sensitivity analysis around the middle of P28 L18. All other comparisons to the results of previous probabilistic reconstruction schemes have been removed. Here we are just comparing the relative performance of the ES to the PBS. Of course the locations and assimilated data sets are different, with MODIS definitely having a higher temporal coverage. Still, it is positive to see that the results achieved from previous studies that the PBS generally outperforms the ES matches our own findings and so we do not see why this should not be included. We do not say that our studies are the same but simply that the results agree. The same applies to the study of Emerick and Reynolds (2013), in a completely different field, we still expect the same kind of relative performance for the data assimilation schemes in a sensitivity analysis with a non-linear model which is indeed what we find.

Thank you once again for all the helpful comments and suggestions,
On behalf of all the co-authors,
Kristoffer Aalstad

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