

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.

Anonymous Referee #1

Received and published: 1 August 2017

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

The paper presents a case study in which MODIS (and Sentinel-2) fractional snow cover area retrievals are assimilated into a simple snow model to infer peak snow water equivalent and subgrid snow estimates. The ensemble smoother with multiple data assimilation technique is used. The paper adds interesting new insights to the discipline of snow data assimilation and could be considered for publication, after addressing the topics below:

1) - There is a weird mix of poor sentences in a very weak English language (too much to list, 1 random example: p.8, L5: “In addition, not accounted for. . . contribute to. . .”) and parts with excellent English. My hope is that the excellent English parts are inserted by one of the co-authors and not copied from elsewhere. Please revise the entire paper for its language. Related to this, I would also suggest to revise the title to something like: “Ensemble-base assimilation of satellite-based fractional snow cover area to estimate the snow distribution at Arctic sites” (Arctic is already high-latitude; the sites do not have a particularly high elevation - I have no idea what "high" was referring to)

- Text: nice literature review, well done. The other text is a bit long in general, and has quite some repetitions in the discussion section in particular – please condense where possible. E.g. referring twice to the adaptive version of the ES-MDA, referring twice to the spatially distributed modeling, etc.

- what are “patterned ground features”? A Nordic term?

2) MODIS, p.13, L.14: how exactly do you average the pixels for each study area? What do you mean by “all the pixels”? Would you include pixels with any (low) cloud fraction? (cloud fraction is given as additional information as a confidence measure for the fSCA retrieval estimates) What is the cloud cover limit? Mention explicitly that you are averaging the satellite data to the 1 km (I suspect), both for MODIS and Sentinel-2. Yet, it is mentioned that the Sentinel-2 data are averaged to the footprint of the snow surveys. . . so perhaps the latter is not right. If the resolution of the Sentinel-2 data, MODIS data and model are different, please explain how you reconcile the space-mismatch. Also mention the 1-km spatial resolution upfront in the modeling for clarity.

3) Ensemble data assimilation:

P15, L4 explicitly name the “parameters” as “perturbation parameters” to avoid confusion with model parameters.

[Printer-friendly version](#)[Discussion paper](#)

P16, L8-9: “remaining parameters”, “prior parameter ensemble”: again refine the word choice here or define “state”, “parameter” and “perturbations” more clearly or efficiently upfront, because this sentence is referring to two types of parameters and confusing.

P16, L18: identify here (again) what is in the state and parameter vector. I suspect that there are no “snow model parameters” included anywhere, but it would be good to explicitly mention this. Clarify upfront that perturbation parameters are updated and not state variables or model parameters, as is often the case in other hydrology/cryosphere research.

Fig.3: replace block with “Update states” → “Propagate state” (I believe that the updating is done on the parameters in the ES-MDA Analysis step)

4) Have you evaluated the updated parameters (result of Eq. 21) themselves? Do the bias estimates make sense? E.g. can you compare the re-analysis forcings, the bias-corrected ones and in situ observed meteorological data?

5) P.20, L17 and Fig 4: peak measurements. . . who or what knows that this measurement is taken at the peak? How can we know for sure that it is a peak measurement, if there is only one data point? Why is that peak measurement always located on May 1st in Fig 4? The peak measurement must be at a different time every year. . .

6) Fig 5: these distributions seem not to be cross-masked, i.e. why are the prior and posterior estimates not cross-masked to the times and locations of the in situ observations?

7) Table 4, Figure 7: clarify in the caption which validation data are used, where and during which period.

8) P.23, L6: in situ fSCA retrievals – are these the camera-based data? Please clarify. It would also help to use the same term to refer to validation data throughout the paper – i.e. in situ vs ground-based vs ground truth vs “Field measurements” (section 2.2)

9) P24, 15: “lowest improvements”: how confident are you with this statement? With

[Printer-friendly version](#)[Discussion paper](#)

only 8 samples, it is hard to get statistical significance of any sort.

10) Table 5: not sure if this exercise has any value with only 3 observations as validation. . . what is the confidence interval on these metrics?

11) Section 4.3: “Effects of observation error and assimilation frequency” ?

12) Conclusions, P31: “For peak mean SWE. . . lower than in previously. . .”: this is an apples to oranges comparison to published work over different regions and different periods and thus invalid; please remove.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-109>, 2017.

Printer-friendly version

Discussion paper

