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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assimilation of passive microwave AMSR-2 satellite observations in a snowpack evolution model over North-Eastern Canada" *by* Fanny Larue et al.

Anonymous Referee #1

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Review of "Assimilation of passive microwave AMSR-2 satellite observations in a snowpack evolution model over North-Eastern Canada", by Larue et al.

The authors present an excellent case study, using a particle filter to do radiance assimilation for snow for the first time in the literature. They build on a previous synthetic study, and validate at 12 sites with in situ stations in Quebec.

Overall, I highly recommend publication in HESS. This is excellent work. However, I think the presentation could be much improved. The standard of English usage is a little bit short of the HESS standard; I flagged some of the problems below, but there are many more. There are places where the symbols are undefined, or things are not

explained well. A bit more work would greatly improve some of these things.

The only other comment is that overall, the authors find (in my summary) that they run at 12 sites, and find a quite marginal improvement in RMSE: from 45 kg mˆ2 to 43.1 kg mˆ2 over all sites. Given the small sample size, those may be statistically indistinguishable. There's a lot of encouraging results too: the bias present in the open loop runs is much reduced, and is essentially zero over the eight sites with less than 75% forest cover. The authors start the results presentation with a deep dive on three sites that do quite well. They ought to give a rationale there, to avoid looking like they are "spinning" the results too much. The authors should acknowledge the small sample sizes involved; they start with 12 and then split things out into low and high forest cover, so they are looking very few sites. This is understandable, but it does mean they need to acknowledge that sample sizes are perhaps not statistically large enough to be able to make all of the claims they might want to.

Minor Comments

1. Page 2, line 16: Please also cite: Andreadis, K. M., and D. P. Lettenmaier (2012), Implications of representing snowpack stratigraphy for the assimilation of passive microwave satellite observations, Journal of Hydrometeorology, 13(5), 1493–1506, doi:10.1175/JHM-D-11-056.1. 2. Page 3, line 9-10: Please provide a recap the main findings of this previous study, especially to the extent they bear on this paper. Recommend moving page 13, lines 4-8 up to the introduction. 3. Page 5, line 13: Here and elsewhere (e.g. Page 12, line 11): Presumably Crocus is running at a 1-hour timestep, and you are outputting daily. Please clarify. 4. Page 5, line 17: "total of precipitable water". Remove "of" 5. Page 6, line 11: "the observations errors were". Grammar doesn't work here. Accepted usage should be "the observation error was" but you could also just change to "observation" and otherwise keep the same. 6. Page 7, line 2: "Database" should be "Data". 7. Page 8, line 14: "dense forested" should be "densely forested". 8. Page 8, line 15: The signal is not in this case biased. I don't think you can talk about the T_B observation being biased unless e.g. AMSR-2 is

measuring TOA values that are biased compared to true TOA values. Instead, I think you mean that it's contaminated or significantly affected by the forest. Treating the TOA measurement as if it were a measurement of T_{B} just above the snow would result in a biased comparison. Anyway, please revise. 9. Page 9, line 21: Crocus has several options for computing grain size. Please give the details here of how this was done for this study, even if they are already reported in the previous Larue et al. 2018 paper. As the authors know so well, T_B is more sensitive to grain size than to SWE, at least at 37 GHz. So this is a really key part of the paper. 10. Page 10, lines 11-22. I read this a few times, but am still confused. So once detected, an IL is added at the top of the snowpack. Then on the first timestep with precipitation, it is subsequently buried 4 cm beneath the surface? So e.g. it would exist in the model at the top indefinitely as long as there is no snowfall? Why not just add it 4 cm under the surface from the time it is detected? 11. Section 3.3, pages 10-12. Overall, I found the notation and presentation to be confusing enough to be distracting here. I would start out the section with an equation that includes both forest and atmosphere; it is frustrating that it starts with an equation neglecting the atmospheric contribution, given the title of the section. I also find it confusing that the atmospheric contributions are presented in a section entitled "Vegetation contributions." Please revise. 12. Page 11, line 12. I believe that "simple" should be "single", correct? 13. Page 12, line 5. What does 0.1 represent? Probably better to define as a symbol, and give the value in the text. 14. Page 12, section 3.3.2. Overall I think that you ought to be able to read the section on soil contributions and know which of the parameters are dependent on frequency, and which are frequency invariant. You'll need to revise 3.3.3 a bit too, I think, to avoid duplicating too many explanations. 15. Page 12, line 6. What is the definition of r_H in equation 8? 16. Page 12, line 8. Is the "." supposed to represent multiplication? If so, please remove, and just take advantage of implied multiplication, writing e.g. \sigma_s = k \sigma. 17. Page 12, line 9-11. Is \Gamma frequency-dependent? 18. Page 12, line 12-13. I think I see now that you are using \nu to note frequency-dependent variables, and to distinguish from those that are frequency-invariant. However, it took

me a while to work this out. Can you reword this, maybe: "Note that we will often use "\nu" subscript to denote quantities that are dependent on frequency, hereafter." 19. Page 12 line 15. Sometimes the process of backing out model parameters is referred to as "calibration" and sometimes as "inversion" in this paper. Later (in the results) it's referred to as "optimizations" (Page 17, line 8-9, e.g.). Please just pick one of those two names and use it at all times, to avoid confusion. Else readers wonder if you are referring to the same thing, or to something they missed somewhere in the paper. 20. Section 3.3.3, pages 12-13. This section required far too long to parse. I found it to be unnecessarily opaque. If this is the same procedure as Roy et al. 14, I would just say that you used the same procedure as that paper. If not, can I recommend a thorough rewrite? Something like: "We thus have two frequency-dependent parameters (eta_nu, beta_nu), and two frequency-invariant parameters (omega, sigma_s). We perform a sort of two-stage calibration. We permute all possible combinations of the two frequency invariant parameters. Specifically we varied omega from 0.02 to 0.16 in steps of 0.01, and varied sigma_s from 0.01 to 1.1 in steps of 0.05. This yields a total of 300 possible combinations of the frequency invariant parameters. Then, for each possible combination of the frequency-invariant parameters, we performed a calibration of the frequency-dependent parameters, eta and beta, for each frequency; thus a total of 900 frequency-dependent calibrations are performed. Finally, for each possible combination of the frequency-invariant parameters, we compute the total post-calibration Tb_RMSE across all three frequencies. The combination of frequency-invariant parameters resulting in the lowest Tb_RMSE is chosen." 21. Page 13, lines 9-12. Is the implication that everything is identical to the previous paper except for the covariance inflation? If so, please make this explicit. If not, then no need to highlight covariance inflation prior to beginning the first subsection, in my opinion. 22. Page 14, line 14. I think ideally you'd have the observation error be larger than 2 K. It really represents all mis-match between observation and model: i.e. what error is expected if the model in its current form is run with "correct" inputs? Of course, this is only a sort of initial value, since you are using covariance inflation. May want to make that explicit here. 23. Page

14, line 22. Can you clarify that observation error covariance here is just observation standard deviation squared times the identity matrix? 24. Page 15, line 25. I don't think it is 15% for CoreH2O for shallow snow. I think the requirement was given in absolute SWE (mm) for shallow snow, and a percentage for deep snow. Please double check. 25. Page 15, line 21. What do you mean by "punctual"? Please reword. 26. Page 15, line 32. I believe there are twelve total sites. Please make that explicit. Usually you want >20 for e.g. large-sample statistics to hold, right? 27. Page 15, line 33. Why do you think the site selections are random? In the Western US mountains (albeit a very different environment), it is assumed that logistics of site selection end up leading to a highly biased spatial distribution. E.g. seeMolotch, N. P., and R. C. Bales (2005), Scaling snow observations from the point to the grid element: Implications for observation network design, Water Resources Research, 41(W11421), doi:10.1029/2005WR004229. 28. Page 16, line 4. I recommend retitling the first subsection "Results of model calibration". 29. Page 16, line 5-7. This entire first paragraph is methods. It must NOT be in the results section. Please move it to the methods section, probably §3.3.3. Also please see my suggestions for reworking §3.3.3. 30. Page 17, line 10. I thought you were not calibrating over the winter? Please clarify. Is this using the optimal parameters you obtained over the summer and combining with the open loop model run? Or are you also calibrating over the winter? Recommend describing the winter error statistics very carefully; to be honest, I think having them in there is not worth the added confusion it brings to the reader. The calibration should really be in the background, here, as it has been done in many previous papers. The focus should be on the assimilation results. 31. Page 17, line 12, and elsewhere. "Pluri-annual" is not common English usage. Please reword. 32. Page 21, line 12. What is meant by the 48 kg/mˆ2 limit? This seems to appear from nowhere, and additionally represents very shallow snow. 33. Page 22-23. Recommend simply removing these sub-section headers. They are fairly clear from context, and the sections are not too long. 34. Page 22. I don't think you can claim 0.79>0.78 without doing a rigorous statistical test; they seem basically the same to me. The offset is a definitely change; I would highlight that.

35. Page 22, line 25. Recommend giving RPE here instead / in addition, since that's what is being discussed earlier in the paragraph. 36. Page 23, Table 5. Recommend redoing notation. Why are italics used in random places? Why aren't "obs" and "sim" subscript? 37. Page 24, line 8. Recommend introducing "wet snow" as an issue in the introduction. Add some text maybe on how passive microwave won't give additional information about snow once the snow is wet, but can help correct earlier biases, etc. It is only mentioned once in the methods, and is quite easy to miss. 38. Page 25, Figure 8. Can you show the SWE_{DA} posterior ensemble spread, as in the other graphs? I think we should see it get larger when liquid is present, which should enrich the discussion in this section. 39. Page 26, line 27. Do you mean "However", instead of "Nevertheless"? 40. Page 27, Figure 9. Is this for posterior or open loop?

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