

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.

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General comment of the R1

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;

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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 m⁻² to 43.1 kg m⁻² 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 openloop 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.

General comment from the author:

It is not only 12 sites, it is 43 winters which were compared (see table 1 for time period of each station), and these winters were different enough to ensure to study a large range of snowpack observed in Québec. We added the following line in the 'study area' section : "A total of 43 winters could thus be studied (Table 1). These winters were all very different, the winter 2012-2013 had the lowest snow accumulation in ten years (165 cm) whereas the winter 2013-2014 was very snowy (379 cm) compared to the average snow accumulation (217 cm). The winter 2014-2015 was unusually cold (3 ° below the average temperatures) and the winter 2015-2016 was the warmest in 60 years (statistics can be find at <http://www.mddep.gouv.qc.ca>)." The following paragraph was rewritten and moved at the beginning in section 2.2 (page 6, line 26): " Comparing data simulated at the station against model cells involves uncertainty due to spatial variations of the snowpack and land cover. This is a well-known problem for model

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validation studies and we assume here that the high number of sites (12 SWE stations, or 43 snowpack simulations) provides a useful assessment of simulations. It is also known that the spatial localization of measurements can lead to some biases (Molotch and Bales, 2005). To diversify its measurements, Hydro-Québec has installed two SWE sensors in the forest, and not in a clearing as is the usual practice for ease of maintenance. " Answer to minor Comments

R1. 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.

AC1. Done

R2. 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.

AC2. Done. We moved page 13, line 4-8 to the introduction in Page 3 line 9-10.

R 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.

AC3. Done. Page 6, line 1 : "The Crocus model updates the snowpack every 15 minutes by interpolating meteorological inputs, but in this study we used daily Crocus outputs (SWE, snow depth, density, etc.) computed at 14:00 local time (19:00 UTC), in agreement with the AMSR-2 pass (Sect. 3.1.1)."

R4. Page 5, line 17: "total of precipitable water". Remove "of"

AC4. Done

R5. 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

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to “observation” and otherwise keep the same.

AC5. Done Page 6: "The observation error was"

R6. Page 7, line 2: “Database” should be “Data”.

AC6. Done

R7. Page 8, line 14: “dense forested” should be “densely forested”.

AC7. Done

R 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.

AC8. Done. We rewrote the sentence Page 8 Line 17: " The measured TB signal can be significantly affected by the forest and the signature of the underlying snow is attenuated during the winter period in such densely forested areas. "

R9. 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.

AC9. Done. We added this new sentence, Page 9, new line 14: " In particular, the snow layers are modeled with a set of variables representing the morphological properties of snow grains (shape and size), including the specific surface area (SSA), which is one of the most sensitive variables for snowpack emission simulations. The snow microstructure evolves in time according to semi empirical laws (Vionnet et al., 2012).

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Crocus is the only model able to simulate the SSA as a prognostic variable (rather than as a diagnostic variable) by using the formulations of Carmagnola et al. (2014). "

R10. 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?

AC10. We do not integrate it directly at 4 cm because TBs observations are affected at least during 40h (according to satellite observations) by the formation of an IL at the top of the snowpack. We added the following sentence: "This is a simplified way to take into account the presence of IL, and further studies are needed to dynamically evolve these ILs in the snowpack and the impact on the neighboring layers. This work is particularly complex and no solution was found yet (D'Ambroise et al., 2017), in particular because measurements are difficult to take." This section has been clarified. Page 10, new line 17: " Hence, the IL first added at the surface of the snowpack was moved to 4 cm from the surface as soon as a snowfall was detected with GEM precipitation data or, if not, after five days to take into account the snowpack transformations (percolations, sublimations, etc.). The maximum number of detected IL was fixed at two. When a second IL was detected (IL2), IL2 was added at the surface while the first detected IL (IL1) was left at 4 cm. After the next snowfall (or after five days otherwise), IL1 was moved to 8 cm from the surface and IL2 to 4 cm."

R11. 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.

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AC11. Done, we moved the explanation of the TB, ATM in the section 3.3.

R12. Page 11, line 12. I believe that “simple” should be “single”, correct?

AC12. Done

R 13. Page 12, line 5. What does 0.1 represent? Probably better to define as a symbol, and give the value in the text.

AC13. Done

R 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.

AC14. Done. See new section 3.3.3

R 15. Page 12, line 6. What is the definition of r_H in equation 8?

AC15. Done

R 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. $\sigma_s = k \sigma$.

AC16. Done

R 17. Page 12, line 9-11. Is Γ frequency-dependent?

AC17. yes, we rewrote the sentence

R 18. Page 12, line 12-13. I think I see now that you are using ν 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 “ ν ” subscript to denote quantities that are dependent on frequency, hereafter.”

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AC18. Done

R19. 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.

AC19. Done, we adjusted the word ‘inversion’ everywhere.

R 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 (η_{ν} , β_{ν}), and two frequency-invariant parameters (ω , σ_s). We perform a sort of two-stage calibration. We permute all possible combinations of the two frequency invariant parameters. Specifically we varied ω from 0.02 to 0.16 in steps of 0.01, and varied σ_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, η and β , 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.”

AC20. Done, we rewrote the section page 13.

R21. 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

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AC21. We clarified this section Page 13. In this paper we add a new inflation technique of the covariance matrix. It was not performed in the first paper (not necessary with synthetical observations)

R 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.

AC22. Done. Page 14, new line 20: " Note that in reality it was probably larger since it represents all mismatches between observations and simulations obtained if the model was run with 'correct' inputs. This observation error cannot be easily estimated (low spatial resolution, representativeness, etc.), but it is only a sort of initial value here, since we used a covariance inflation to adjust it."

R 23. Page 14, line 22. Can you clarify that observation error covariance here is just observation standard deviation squared times the identity matrix?

AC23. the R matrix is the observation standard deviation squared times the identity matrix. We clarified it: Page 15, new line 12: " In this study, we developed a new technique to avoid a degeneracy problem, which consists in the online adjustment of the R matrix (i.e. observation standard deviation squared times the identity matrix) such that the weight of the 25-th selected particle (wekeep) is at least equal to $1/N$."

R 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.

AC24. Rott et al. 2013 detailed the objectives of the CoreH2O mission for SWE estimates, and fixed the wanted accuracy to: 3 cm for SWE < 30cm, and 10% for SWE >

30 cm.

R 25. Page 15, line 21. What do you mean by “punctual”? Please reword.

AC25. We mean data simulated at the station. This sentence was removed in page 6 line 16.

R 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?

AC26. The sentence was clarified. We have 10 SWE stations followed for 4 winters (2012 to 2016), 1 station followed for 2 winters and 1 station followed for one winter, i.e. 43 winter simulation. The winters were very different from each other (2015-2016 was the warmest from 15 years, and 2014-2015 the coldest from 6 years). The 43 winter simulations were studied together to better represent the different snowpack observed in Québec.

Note that GMON instruments are expensive and daily SWE data are often sparse. This is the first time that the snowpack in Eastern Canada are studied with so much data. 12 other GMON stations were added in 2018.

R 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. see Molotch, 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.

AC27. This part was rewritten: Page 6, line 26. " This is a well-known problem for model validation studies and we assume here that the high number of sites (12 SWE stations, or 43 snowpack simulations) provides a useful assessment of simulations. It is also known that the spatial localization of measurements can lead to some biases (Molotch and Bales, 2005). To diversify its measurements, Hydro-Québec has installed

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two SWE sensors in the forest, and not in a clearing as is the usual practice for ease of maintenance. "

R 28. Page 16, line 4. I recommend retitling the first subsection "Results of model calibration".

AC28. Done

R 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.

AC29. Done. See new section 3.3.3.

R 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.

AC30. Done—: Section 3.3 was rewritten to be clearer and to well separate method and results. We removed the column with winter RMSE data in the table.

R 31. Page 17, line 12, and elsewhere. "Pluri-annual" is not common English usage. Please reword.

AC31. We replace dit by 'multi-year'

R 32. Page 21, line 12. What is meant by the 48 kg/m² limit? This seems to appear from nowhere, and additionally represents very shallow snow.

AC32. we added the following information, but it was explicitly detailed in sect 2.3.4: "Performance is estimated for SWE up to 48 kg m⁻² (about 20 cm of snow depth,

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derived from measurements, see Sect. 3.4.2)"

R 33. Page 22-23. Recommend simply removing these sub-section headers. They are fairly clear from context, and the sections are not too long. AC33. We think that the headers help to clarify the presentation of results. Several data are presented here and it can be hard to follow. Moreover, the section is quite long (2 pages).

R 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 definite change; I would highlight that.

AC34. done. The sentence was rewritten (Page 22, line 19) " Correlation between SWEDA simulations and SWE measurements gives a similar R coefficient to the one obtained with SWECrocus simulations ($R = 0.79$ and $R = 0.78$, respectively), but the offset is significantly reduced with SWEDA compared to SWECrocus (offset = 10 kg m⁻² and 29 kg m⁻², respectively). "

R 35. Page 22, line 25. Recommend giving RPE here instead / in addition, since that's what is being discussed earlier in the paragraph.

AC35. Done: RPE GLOBSNOW = 39.5%

R 36. Page 23, Table 5. Recommend redoing notation. Why are italics used in random places? Why aren't "obs" and "sim" subscript?

AC36. Done, see new table 5.

R 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.

AC37. We added the following sentence in the introduction (page 3, line 2): " However, the assimilation of PMW must be used with care, and a good understanding of the in-

teractions between the properties and microwave emission of the snowpack is crucial to avoid degrading the SWE estimates. For instance, the assimilation of passive microwave in wet snow conditions can introduce large uncertainties since the presence of liquid water in the snowpack increases TBs, whereas increases in snow grain size decrease the brightness temperature independent of any change in SWE (Klehmet et al., 2013)."

R 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.

AC38. Done, a new Fig 8 was recomputed with the spread of ensembles.

R 39. Page 26, line 27. Do you mean "However", instead of "Nevertheless"?

AC39. Yes, we corrected this sentence.

R 40. Page 27, Figure 9. Is this for posterior or open loop?

AC40. This is before data assimilation as written in the caption.

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