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Discussions

Interactive comment on "Homogeneity criteria within IASI pixels for the preparation of an all-sky assimilation" *by* Imane Farouk et al.

Anonymous Referee #1

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This manuscript looks at an interesting area and some new results are presented but without getting into much depth of analysis. There are gaps to be filled to support the conclusions. In particular it is hard to make an informed choice between the different inhomogeneity screening options based on this work, as only one of the viable options was tested in an NWP system (the other tested option already showed clear defects even before NWP system testing). There is also no independent validation of whether the screening achieves its goal, which is homogeneous scenes.

Major points

1) P6 L9: "we plan to assimilate clear or cloudy observations that are completely covered in the IASI FOV .... discarding fractional cloud observations". This still allows the possibility of fully clear obs being assimilated in a fully cloudy model (or vice-versa). Is

that the intention? How do these screening methods treat cloudy modelled scenes if at all? The text should explain.

2) The choice of $49K^2$ departure threshold in 3.2.2 is unsupported and uninvestigated in the text. Important questions are what this threshold means in terms of retained cloudy scenes, and how do its effects differ from those of the 7K AVHRR departure check in the Martinet et al. (2013) approach? Ultimately it should be investigated why the adapted (it is not the original) Eresmaa (2014) technique provides poor cloud screening here. Maybe it is the adaptation of this departure check? Finally, it is probably a case of poor wording rather than science, but it seems incorrect to claim the departure threshold as a check on homogeneity rather than just on cloud (P10 L19).

3) The choice of the 0.8% threshold on p11 is barely supported in the text or by Figure 2. It may be the colour scale but there seem to be no highly inhomogeneous scenes according to the IASI Ne (e.g. between 0.3 and 0.7 on Fig. 2) and there certainly seems no correlation between the relative cluster standard deviation and the Ne.

4) P11 L17 "Similarly in model space the D_mean..." Since D_mean is based on observation minus background it is not in model space and neither is it a direct indication of the model cloudiness.

5) "The percentage of cloudy AVHRR pixels" - if this is a good enough indicator of fractional cloud and/or inhomogeneity to use it to validate the screening criteria, why is it not used as part of the screening criteria?

6) Section 5, the intercomparison of selection criteria, does not fully make the case for the proposed selection method. M2013 keeps 29% of data in table 2 compared to 21% in the compromise method, with only slightly higher standard deviations. That could be a good choice, but it has been rejected at this stage. The balance between a slight increase in std. dev. and gaining extra data has hence not been properly explored. It seems odd to instead test the "Obs_HOM" approach in data assimilation as already from the intecomparison it is clear it does not work well. Further, without an exploration

of its sensitivity to threshold choices, the E2014 test does not have much of a chance in this intercomparison. Finally, each of the previous techniques M2013 and E2014, as well as the newly proposed compromise technique are composed of two tests, and it would be good to know how many rejections each is responsible for and how much overlap there is between the two tests.

7) Much of the conclusions will need to be updated to reflect a more thorough comparison of the different methods but particularly problematic is the statement P19 L4-6, saying that the M2013 and E2014 techniques were unsatisfactory due to a large loss of observations. Since M2013 provides more observations than the proposed method according to table 2, this statement is wrong. Also E2014 was not well explored in terms of thresholds and other possible adaptations to make it work in the current framework, using all-sky forward radiative transfer. Any rejection of this technique needs to be carefully qualified.

Minor points

1) Introduction: an up-to-date reference on homogeneity critetria for all-sky is the following. It should be discussed:

Okamoto, K. (2017), Evaluation of IR radiance simulation for all-sky assimilation of Himawari-8/AHI in a mesoscale NWP system. Q.J.R. Meteorol. Soc., 143: 1517-1527. doi:10.1002/qj.3022

2) P3 L31 "the first level at 10m" sounds odd; surely the lowest 10m of the atmosphere is also included in the model?

3) P5 L28 "Stratus Continental and Stratus Maritime" are cloud microphysical options in RTTOV; this should be stated; also it should be made clear how the choice is made, even if it is as obvious as using the land-sea mask.

4) P10 L1 "Background brightness temperature for AVHRR" - for clarity explain if this is clear-sky or all-sky.

C3

5) P10 L7 If $f\hat{}j$ is different from C_j please explain how; otherwise use consistent notation.

6) P11 L10 Instead of "relationship" is "ratio" intended? At present the text is imprecise.

7) P12 L15 "Mean standard deviation" is always a confusing phrase and needs explanation of what samples got meaned or standard deviated and in what order.

8) Table 2 should additionally include statistics for the observations that are fractionally cloudy according to AVHRR.

9) P12 L15 to P14 L10 is hard to read as it is overloaded with numerical results and bereft of much interpretation. Many of the numerical results are already listed in table 2 and do not need exhaustive restating in the text, which needs to be rewritten with a higher level of analysis for the reader. Where numerical results given in this text are not already in tables, they should be.

10) P14 L22 "Gaussian" is overly strong here and is not backed up by any statistical tests of Gaussianity.

11) P14 L30 "we keep more observations" - which method is referred to by "we"?