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Comment on gmd-2021-37

Anonymous Referee #1

Referee comment on "Multi-sensor analyses of the skin temperature for the assimilation of satellite radiances in the European Centre for Medium-Range Weather Forecasts (ECMWF) Integrated Forecasting System (IFS, cycle 47R1)" by Sebastien Massart et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-37-RC1, 2021

Review of "Multi-sensor analyses of the skin temperature for the assimilation of satellite radiances in the European Centre for Medium-Range Weather Forecasts (ECMWF) Integrated Forecasting System (IFS, cycle 47R1)"

By Sebastien Massart, Niels Bormann, Massimo Bonavita, and Cristina Lupu

General Comments

This manuscript describes experiments to evaluate the impact of modifying how analysis increments of the surface skin temperature are represented within a 4D-Var data assimilation system for numerical weather prediction (NWP). The previous approach, used in the operational system up until now, uses a skin temperature variable for the analysis increments that is independent for each instrument and each observation location and time. That is, the background error correlation between the skin temperature at different locations/times and between different satellite instruments is assumed to be zero. In the new approach, a pair of 2D time-dependent gridded fields of skin temperature (one for microwave sensors and one for infrared sensors) is used to compute the skin temperature increments when assimilating observations from all instruments. These fields are assumed to have both spatial and temporal background-error correlations that are non-zero. Simultaneously, the specified background-error standard deviation is modified in the new approach, with one notable change being the reduction in the value over sea ice.

Overall the manuscript describes well the experiments performed and the results obtained. However, the introduction lacks a description of the international scientific context of this work. In addition, the design of the experiments makes it difficult to draw general scientific conclusions due to multiple simultaneous changes to how the skin temperature analysis increment is represented and how its background-error covariances

are specified. Finally, there is insufficient analysis of the results to back up some of the assertions. These major points, which should be fully addressed by the authors, are detailed in the following section.

Major Specific Comments

The introduction section lacks a description of the scientific context as it does not provide a description of related work that has already been performed at other NWP centers, internationally. For example, by saying "new approach" (on line 76), presumably this refers to being new at ECMWF and not within the international field of NWP. As an example, an early study by Garand et al (2004, Journal of Applied Meteorology and Climatology) described the Canadian system that already used a gridded skin temperature field for assimilating radiance observations (one of the “new” innovations described in this manuscript). They then evaluated the impact of including the background-error correlation between skin and air temperature, which in some respects goes beyond what is described in this manuscript.

The design of the experiments does not allow for a systematic evaluation of how the different modifications affect the results. As already mentioned, in some situations the specified standard deviation of skin temperature appears to have been changed in the new formulation in combination with the use of a common gridded skin temperature field for all instruments and introducing both spatial and temporal background-error correlations. This makes it difficult to draw any conclusions about the impact of the changes over sea ice, where it is noted that the standard deviation is significantly changed. This aspect should be more fully explored.

The reduced amplitude of the analysis increments with the new approach are assumed to be related to the additional constraints imposed on skin temperature increments in the new approach. These constraints result from the addition of spatial and temporal correlations and also the use of a single 2D skin temperature field for all microwave instruments and another for all infrared instruments. I would think that some additional analysis of the results could provide concrete evidence of this and possibly suggest if any of these differences are more important than the others. For example, the increments obtained with the two approaches could be compared (at observation locations) in terms of their spatial, temporal and between instrument variability. Showing a much higher variability when using the previous approach, either in space, time or between instruments, would support the assertion that one or more of the added constraints cause reduced increments. The authors could then also comment on if the higher variability is more or less physically realistic.

Other Specific Comments

Equation 9: The cost function is not shown in the incremental form, which makes the meaning of H^T less clear, since there is no corresponding tangent-linear operator. Either

the cost function and gradient should be presented in incremental form or at least the correspondence between the shown form and what is actually used in practice well explained in the text.

Equation 18: Shouldn't this be sqrt of the B matrices? If not the variable transformation should be better explained and a reference given.

Line 255: I don't think it's correct to simply refer to this approach for skin temperature as 4D-EnVar, since a major part of that approach is how the 4D ensemble covariances are implemented with spatial localization. In the present study, the following section describes how covariance parameters in a wavelet-based representation are estimated from an ensemble. Also, the Liu et al reference is probably not the best for 4D-EnVar, since the approach in that paper does not include spatial localization and uses ensemble covariances purely in observation space, making it quite different from what is currently considered the 4D-EnVar approach. It would be better to refer to Lorenc (2003) and Buehner (2005).

Line 307: This choice of wording is unclear and possibly misleading. Consider rewording to avoid the use of "replace".

Line 316: It is not clear if these diagnosed length scales are actually used as part of the B matrix specification. I thought that the (diagonal) wavelet approach directly computes the spatial correlations in wavelet space from the supplied ensemble. Please clarify if the diagnosed values referred to in the text (both spatial and temporal) are merely for diagnostic purposes or are somehow used in the B matrix specification.

Line 341: What about MHS instruments on the platforms with AMSU-A? Are they not assimilated?

Figure 3: Probably not appropriate to use a line graph when the x-axis represents distinct satellite sensors. A bar graph would be better.

Line 410: I suppose this is referring to the large values of stddev of the difference between the increments of the two experiments. This is not clear from the current text.

Line 411: Would be helpful to provide average values for each of the 3 types of radiance instrument from each experiment to support this statement.

Line 417: Please briefly mention what these "other errors" could be. Is the accuracy of

surface emissivity a concern?

Line 430: What is the implication of this statement? This needs to be better explained.

Line 436: How would inclusion of the background-error correlation between skin temperature and various atmospheric variables affect this? Is it technically possible to include such correlations with the hybrid 4d-var + "EnVar" approach used in the new approach?

Line 457: Please discuss the possible cause of the skin temperature being driven to fit the surface-sensitive channels.

Line 467-469: I do not understand this statement. Please clarify the reasoning behind it.

Line 481: This wording (i.e. "the fit is increased") is confusing. Please find a more straightforward way of expressing the change in the fit to the analysis.

First 6 paragraphs of “Conclusions”: There is too much content here from the introduction and methods sections. This should be condensed significantly for the conclusions section.

Technical Corrections

Equation 10 and in equations elsewhere: The adjoint of H should be bold non-italic as in the main text.

Figure 1 and others: “micowave” should be “microwave”.