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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Kalman filter physical retrieval of geophysical parameters from high temporal resolution geostationary infrared radiances: the case of surface emissivity and temperature" *by* G. Masiello et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper discusses a data assimilation framework based on the Kalman filter, to assimilate observations from the geostationary satellite instruments that are sensitive to surface temperature and emissivity. This study focuses on estimating the temporal evolution of these two parameters over an area approximately centred on the Strait of Gibraltar. Retrieved parameters are compared to a series of independent measurements. The main aim of the paper is to show the benefits of combining observations

of these two parameters with a model describing their temporal evolution, in the case of a well observed system. In practice this is achieved by applying a data assimilation method, in this case a Kalman filter. I believe the paper is interesting, nicely written and relevant. I have, however, a few major points that need to be looked at.

- In the introduction the authors claim that "the precise form of the evolutionary equation is not important for the estimation problem but the current state and its statistics". I believe this statement is flawed, as model error characteristics contribute to determine forecast error, which is used by the assimilation system. If the model does not provide sufficient information, the assimilation results will be essentially the same as those obtained from a standard retrieval procedure (i.e. without the use of a dynamical model). I believe this is the reason why the differences between the results with the persistence model used in the paper (SEVIRI KF) and those from a retrieval method (SEVIRI LSA SAF) are almost identical (see Fig. 17). A much more interesting exercise would have been to use the KF with a more sophisticated model, for instance capable of capturing the daily cycle. As recognized by the authors, this would be particularly important for surface temperature over land, as shown in Fig. 4 where a model for the daily cycle of temperature is actually shown.

- As far as I understand, in the paper the model variables at different grid points are considered to be independent, i.e., the forecast error covariance matrix at different spatial locations is considered diagonal. This is another major simplification of the problem, which for example neglects spatial temperature forecast error correlations induced by realistic initial condition errors. As a consequence, the assimilation results at each grid point are only affected by the data at the same grid point, again making the assimilation procedure very similar if not indistinguishable from a collection of retrievals at different spatial locations. This limitation should be discussed in the paper.

- Another simplification, recognized by the authors in section 4.1, results from neglecting model error correlations between temperature and emissivity errors. A physically realistic model of both quantities should be able to account for those correlations that are actually occurring in nature: as discussed in section 4.1, Figure 10 shows that emissivity also presents a daily cycle. Correctly accounting for cross-variable correlations would make an observation of temperature to improve the emissivity field, despite spurious correlations induced by the observation operator. Again, a physically consistent model and model error term is essential for the solution to be realistic. This should be pointed out.

- The reason for choosing a Kalman filter instead of a smoother that provides, under certain circumstances, the optimal analysis for a fixed time interval, is usually due to near-real time constraints. For the applications presented in this work, the use of a Kalman smoother would have allowed future observation to affect the estimate in the present and past (within the interval). Again, this would have increased the amount of information extracted from the data with respect to a standard retrieval strategy. This should be pointed out in the paper.

In summary, I recommend publication subject to revisions that should address the above as well as the following points.

OTHER COMMENTS

P6876, L2-3: It is not clear what “current state” means here. The estimate depends on the best estimate of the current state conditioned on all previous observations and on present observations.

P6884 L19: replace “error forecast covariance” with “forecast error covariance”.

P6885 Eq. 11 and later: It is advisable to use a notation that is not too different from usual practice: please replace H with M as the tangent linear the dynamic model.

P6886 L6-10: The analysis actually depends on all previous history, so implicitly depends on all previous observations as well as the dynamical model. When the system is Markovian, it is the analysis update, not the analysis that depends only on the previous state and current observations. Also, even in the Markov case, the model features

in the analysis as (Eq 13) as Xa is the forecast from the previous analysis. Please correct.

P6887, L15-end of page. I am confused, as I thought each grid point was treated independently of all other points. Also, the last sentence (“we stress..”) is rather obscure to me. Please clarify.

P6888, Eqs 20 and 21. I found the implementation of a transform that ensures positiveness of the retrieval quite appropriate and useful. However, for the analysis to be optimal, the transform should also ensure Gaussianity of the prior distribution. I would like the authors to comment on that and perhaps to test the background error distribution resulting from the logit transform

P6896 L21: I believe Sa should be replaced by Saˆ-{1}

P6897 L10: Is the model time step also 15 mins?

P6904 L10-until end: I believe the assimilation method described in this paper is meaningful because it aims to estimate parameters that are well observed and results show that in this case the predictive model can be very simple (although the results are in this case very similar to those derived with simple retrievals, as noted above). But when the aim is to estimate atmospheric quantities, the proper way to do this is to assimilate (satellite) data with a numerical weather prediction model. I would then recommend the authors to make that very clear in the final pa

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