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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.

G. Masiello et al.

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We thank the referee for the appreciation of this study and for the in depth review.

Reply to major points:

-In a data assimilation context we have the analysis and the forecast. What we are saying here is that the update only depends on the current observation and does not

depend on the dynamical model directly. This is formally expressed later in the paper with Eq. (13), which does not contain the operator $\mathbf{H}$. As far as the specific (T_s, ε) problem is concerned, we also add that for the analysis the model is not important as shown by the study, but the model error is (e.g. see Figs. 5 to 7): without the time constraint (i.e. low error in the persistence model) we would not successfully retrieve both parameters (T_s, ε). For the forecast, yes the referee is right, the model is important. We have modified in the text, where needed, to clarify what we mean.

To properly compare SEVIRI LSA SAF to SEVIRI KF, the referee should take into account that SEVIRI KF retrieves emissivity as well, therefore the SEVIRI KF retrieval is much more informative than the static split-window algorithm SEVIRI LSA SAF which retrieves temperature alone and for the case at hand has been optimized with in situ observations at EVORA station, including cover type of the surface. The SEVIRI KF is capable to show that the emissivity at 8.7 μm is larger than that at the other two channels, which is in agreement with in situ observations. Therefore we think that Fig. 17 (considering both panels a) and b), and not a) alone) shows that SEVIRI KF retrieves much more information than SEVIRI LSA SAF. However, the point remains, and here we agree with the referee, that our system is mostly driven by the data. Actually, this was purposely the objective of this paper. Maybe the referee here want to say that he expect that a method based on Optimal Estimation (OE) should show no difference with the present KF approach. This is true, although with Optimal Estimation we need to accumulate the data on a given time slot, which has the effect of increasing the dimensionality of the retrieval system. An in depth comparison between OE and KF has been performed in Serio et al 2013 (see the reference list) and the results shows that there is not so much difference between the two approaches, although KF is much more efficient to keep the dimensionality of the data space lower. We have now commented on this in the conclusion section, where we have made also a proper reference, where needed, to Serio et al 2013.

In fact, this exercise has been performed in Serio et al 2013, where the temperature

daily cycle was modeled with a second order autoregressive process. However, no improvement was found with respect to a simple persistence model. The fact is (please look at Fig. 4b and 4c) that the daily cycle is reproduced in its very fine details by the data: the daily cycle is in the data. Therefore, there is no need to include this information with an external model. We have commented on this in section 3.3 and made proper reference to Serio et al 2013.

-The referee understood well. We have commented on this in the conclusion section. We do not think this is a severe limitation for the (T_s, ε) problem, in view of the strong dependence of the SEVIRI window channels on these two parameters. Spatial temperature forecast error correlation would be for sure important in case we include within the KF system the retrieval of atmospheric parameters. Again, we have stressed this point in the conclusion section. We also point out that in our study spatial constraints are only considered in a theoretical context not in the application to the (T_s, ε) problem where we apply a strictly temporal only method. We have stressed this at the top of section 3.3.

-Short time scale emissivity variations are governed by atmospheric parameters rather than directly from surface temperature. Actually, atmospheric parameters are modeled within the scheme though ECMWF model analysis, which is a strong, informative, constraint. A more comprehensive scheme should also retrieve atmospheric parameters, but this is left to further work. We also note that we are not sure what does the referee mean when he says *model error correlations between temperature and emissivity errors..*? If he means we should be able to model the correlation between ε and T_s in the update step (i.e. model the off-diagonals $\mathbf{S}_\eta$) then he is asking a lot at this stage of the understanding of surface emissivity. Also we do not fully agree with the statement *"..a physically consistent model and model error term is essential for the solution to be realistic"*. Essential is way too strong, the results are demonstrably realistic using an uncorrelated assumption.

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-We have commented on this in the conclusion section. The Kalman smoother (KS) is a particular application of KF: just run KF forward and backward. The KF error analysis shown in the paper (e.g. Fig. 6 and 10) demonstrates that we can achieve a precision for temperature of ± 0.2 K, whereas for emissivity we have ± 0.005. A KS could well further improve the results, but the study was done with regard to an operational implementation where the additional logistics would be prohibitive.

OTHER COMMENTS:

1. We mean the analysis update. We have rephrased in the text.

2. We have corrected in the revised version of the paper.

3. We have used $\mathbf{M}$ instead of $\mathbf{H}$ in the revised paper. The use of $\mathbf{H}$ makes reference to Wikle and Berliner paper (2007), however we agree with that Rodgers notation is much more common and we have replaced $\mathbf{H}$ with $\mathbf{M}$.

4. We have corrected, again we mean the analysis update. It is true that in a dynamical settings, the analysis depends on all the previous outcomes of the process.

5. This sentence applies to the general formulation of the KF approach. Simplification are derived later when applying the methodology to the particular case of (T_s, ε). We have clarified this at the beginning of section 3.2.

6. For sure emissivity is not Gaussian since the parameter takes values which are naturally restricted in the range 0 to 1. In this case, the logit transform gives a much more appropriate representation since it removes the problem of boundaries. For the referee to look at an example is shown in the Figure 1 below.

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7. Yes, the referee is right. We have corrected in the revised version of the paper

8. Yes it is 15 min, we have clarified in the revision.

9. We have clarified this part in the conclusion section, although we do not totally agree with the referee. The referee point of vies is to assimilate observations (that is data) in a system which is driven by the model. As we stressed throughout the paper we are in a somewhat reversed position: we want to assimilate the model in a system driven by the data. The model we assimilate could well be, e.g., the ECMWF model forecast itself. In this respect, we think even with atmospheric variables, when observed every 15 minutes and with 3 km resolution, the data-driven approach with persistence model and suitable stochastic error level is probably quite capable of reproducing a good 4D field (remember - as we keep saying, a 'forecast' is not required).
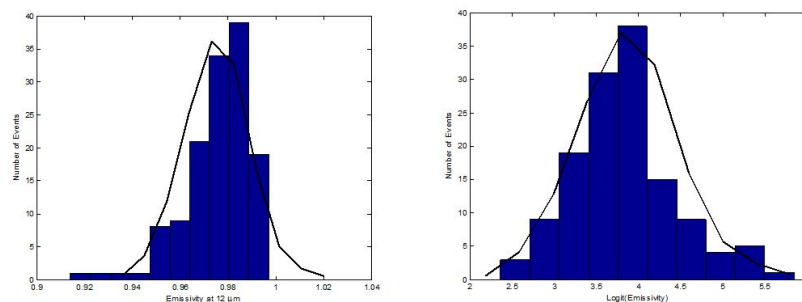

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Figure 1 (not shown in the paper). Left: histogram of an emissivity sample of size 137 representing SEVIRI channel emissivity at 12 micron. Right: Logit transform of the values on the left. The solid curve is the Gaussian curve with same mean and standard deviation as those of the sample. The sample has been derived from the ASTER (Advanced Spaceborne Thermal Emission Reflection Radiometer) Spectral Library, version 2.0 and the MODIS (Moderate Resolution Imaging Spectrometer) UCSB (University of California, Santa Barbara) Emissivity Library (http://www.icess.ucsb.edu/modis/EMIS/html/em.html).

Fig. 1.