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Atmospheric
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# *Interactive comment on* “Technical note: a new day- and night-time Meteosat Second Generation Cirrus Detection Algorithm MeCiDA” *by* W. Krebs et al.

**W. Krebs et al.**

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Both reviewers made very valuable comments for which we like to thank them! In fact, the main points of both were very similar for which reason we could even use identical replies for some points. In the following, the comments of reviewer 1 are printed in italics:

*... although the authors should also refer their work to the paper from Derrien and Le Gleau : MSG/SEVIRI cloud mask and type from SAFNWC (International Journal of Remote Sensing, 26, 21, 4707-4732), on the same topic. These authors have developed an algorithm which is different between daytime and nighttime, which is not the objective of the present paper, but relies on a similar approach for nighttime.*

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Thanks - reference added!

*The algorithm relies on a threshold analysis using a classical approach based on the analysis of the brightness temperature difference between two IR channels, and the structure of clouds as obtained from differentiation after a masking of the lower layer contribution by water vapor absorption. It is not clear how these two approaches complement each-other and especially how much information is brought by the different tests at the different wavelengths. More details should be given on the performance of each test independently.*

Both reviewers requested to show the performance of the six individual tests. For that reason we replaced Figure 11 (which showed a false color composite and the corresponding cirrus mask) by a figure which shows in addition the results of the six individual tests. We also restricted the area shown in the Figure so that differences between the tests are clearly visible. The differences are discussed in the text. This is certainly a valuable addition.

*The temperature thresholds used in the analysis are defined using radiative transfer simulations. The variability of the atmospheric parameters as well as surface temperature is considered. However the surface properties are poorly considered in this paper. This is an important issue, as a large part of the clouds is viewed by SEVIRI over land. Surface emissivity is considered to be equal to one, but over desert and semiarid regions frequently observed in Africa, the emissivity is varying from 0.6 to 1, with significant differences in the infrared wavelength domain (Salisbury, J. W. and D'Aria, D. M., 1992, Emissivity of terrestrial materials in the 8-14 $\mu$m atmospheric window: Remote Sensing of Environment, v. 42, p. 83-106). A consideration of such variations should thus be accounted for in the simulations.*

Yes, and it is not only the emissivity which is variable but also the temperature. In the description of our method (pg. 10942) we claim that we "reduce the effect of surface and atmosphere" by subtracting the brightness temperature of a nearby pixel which we consider cirrus-free. As both reviewers state, this can by no means be a complete correction but only a step into the right direction. When optimizing the algorithm we found that this approach certainly improves the detection efficiency of the algorithm. An alternative would be to use model data (ECMWF or NCEP) to obtain the surface temperature and combine with MODIS emissivity maps (as suggested by reviewer 3). However, data with the required spatial resolution and accuracy are hardly available and we doubt that much better results would be obtained that way. In consequence, the algorithm certainly has problems over heterogeneous surfaces, as most remote sensing algorithms.

Our test data set was a pixel-by-pixel data set and does therefore not allow to directly address spatial inhomogeneity effects (neither surface emissivity, surface temperature, nor cloud imhomogeneity, etc). Currently we are working on generating artificial MSG/SEVIRI observations using as realistic as possible input. These will allow to test all kinds of inhomogeneity effects. Although one might consider surface variability effects on the basis of the existing data set (by adding random brightness temperature differences), we would prefer to systematically study all inhomogeneity effects once we have a reasonable data set available.

*This would impact the threshold values defined, although it may imply a regional differentiation. This has to be discussed. Also variability of the cloud environment is considered as a whole (10000 different combinations) to define threshold, and not separating latitudes.*

Yes, a consideration of surface variability would impact the thresholds. Setting a higher threshold would certainly increase the probability that a pixel classified as cirrus is actually cirrus. On the other hand, a higher fraction of cirrus would be missed by the algorithm. The thresholds defined in this paper have been carefully set in order to capture as much cirrus as possible but not too much. The comparison with MODIS illustrates that it works reasonably, although we cannot exclude that with a comprehensive tuning exercise one could reach an even better agreement with MODIS. It is stated in the paper that the algorithm was tuned for the North Atlantic and Europe, basically mid-latitudes (we added this statement also to the abstract). For the tropics, different threshold might provide better results, although our test data set includes all latitudes and MeCiDA performs well for the test data set (for completeness: we included tropical profiles and surface temperatures in the test data set but, as correctly identified by reviewer 1, a maximum cloud top altitude of 12 km is too low for the tropics).

*In the simulations, ice clouds are considered in a single layer which base is above 6 km and top below 12 km. If the base altitude seems reasonably corresponding to observations at mid-latitudes, the top one is much below observations which show altitudes reaching 16 to 18 km even for optical depths of the order of 0.1. Authors should take this into account.*

For mid- and high-latitudes 12 km should be a reasonable cloud top altitude, see e.g. Sassen and Campbell, “A midlatitude cirrus cloud climatology from the Facility for Atmospheric Remote Sensing. Part I: Macrophysical and Synoptic Properties”, JAS 58, 481-496, 2001. The reviewer is certainly right that in the tropics cirrus might reach up to far higher altitudes, but again, the focus of the study was not tropics but mid-latitudes.

*Simulations are made referring to an ideal instrument. Noise in the measurements should be discussed with respect to the threshold values chosen.*

The instrumental NeDT of the SEVIRI IR channels is (except from the 13.4 $\mu$m channel) below 0.1 K. The estimated biases for the window channels are quite similar (0.8 and 0.9 K for the 10.8 and the 12.0 $\mu$m channel). Of course both have an impact on the cirrus detection. Thus we cannot use the model for a direct validation of the test performance. Nevertheless the model was useful for the development of the brightness temperature difference (BTD) tests. The great advantage of the combination of the two types of thin cirrus tests (BTD and morphological) is, that we can use quite low and thus sensitive thresholds in the BTD tests, as false alarms due to noise and other effects are usually excluded by the morphological test.

*Detailed comments*
*In the introduction*
*- it is written "The four solar channels as well as the mixed solar/thermal channel at 3.9 $\mu$m were not used, ... they are obviously only available during daytime." This sentence should be modified with respect to availability of mid IR channel, and the objective of coherence between day and night analyses, as written further.*

ok, corrected.

*In section 2.2:*
*- reference is made to the use of parametrisations of hexagonal crystals from Fu et al., (1998). More recent calculations exist (Yang et al., 2005, Applied Optics, 44, 26, 5512-5523), and due to crystal shape variability and a short discussion of the impact of shape needs to be included.*

Yes, and that was actually a small inaccuracy in the manuscript: For the “systematic calculation” (number 1 on page 10939) we used the parameterization by Fu et al. (1998) because that was the only one available for the thermal IR back then. For the “test data set” (number 2 on page 10939) we actually used a parameterization based on the optical properties which we obtained from Ping Yang. The ice crystal habit was also varied randomly and the effect of shape is included in Figure 3, 6, and 9. We changed the text accordingly.

If we were to redo the calculations today, we would certainly use the newer parameterization by Baum et al., “Bulk scattering models for the remote sensing of ice clouds. Part 1: Microphysical data and models”, JAM 44, 1885-1895, 2005.

*- surface emissivity assumed to be 1 in all cases : see the comments above.*

See comments above.

*In section 2.3*
*- take into account emissivity in the equations*

ok, done.

*- a different formulation of the "corrected BTD" taking into account cloud transmission would allow to obtain curves which would be zero at low and high optical depths. This would be simpler to use.*

One could do that for the model data but we are not sure how to do that for the observed brightness temperatures. The probability to find cirrus-free pixels in the surrounding of cirrus clouds is much higher then to find opaque cirrus in order to get a reasonable estimate for the temperatures in the cirrus level. We did not include the ’high optical depth’ because optically thick cirrus is easily detected by single channel thresholds, as long as polar (and other) regions with very low surface temperature can be neglected.

*- BTDs are analyzed to define a threshold (for example 0.6 K). Consider instrumental noise impact.*

See above.

*- it is the horizontal distribution of the integrated water vapor path which can be considered as smooth with respect to the cirrus cloud radiative properties.*

ok, corrected.

*In section 3*
*- give more information on what is improved using the two combined tests with respect to the use of a single one in test 1*

We have chosen to combine the BTD tests with morphological tests in the absorption channels by a logical AND, as this combination makes it possible to tune the BTD tests for high sensitivity. Both types of test complement each other to reach a low false alarm rate. We added a statement on this in the ’cirrus detection basics’ and included images that show all three parts into figure 2.

*- give more information on what is improved using the different combined tests (1 to 6)*

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*with respect to the use of a single one*

See above. A new figure has been included which shows the result of each of the 6 tests.

*- why not combine more the different tests?*

One could certainly do that. The way the tests are defined includes some “history”. Each test was developed separately and is sensitive to a certain class of cirrus, see new Figure 11. And as such, it is reasonable to present the tests as they are instead of combining all to one huge formula.

*In section 4*
*- it may be also useful to discuss the differences in the number of pixels detected for MODIS and SEVIRI cirrus fields in terms of radiance or brightness temperature of the scene with respect to the background.*

Yes, it would be interesting, but a detailed study of the differences would certainly be beyond the scope of this paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10933, 2007.