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Atmospheric Measurement Techniques Discussions

Interactive comment on "HelioFTH: combining cloud index principles and aggregated rating for cloud masking using infrared observations from geostationary satellites" *by* B. Dürr et al.

Anonymous Referee #1

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The authors present a method for cloud masking from geostationary infrared observations which follows the idea of the Heliosat method. While Heliosat has a long history and is well established, I have some questions about the general approach in the presented method as applied to infrared observations. For example it is not clear to me what exactly is the major advantage of using imager counts instead of brightness temperatures in this approach. While there may be good reasons for using counts, those could be better explained to the reader in order to reduce confusion. Especially as section 3.5 deals with a daily update of the minimum count in order to account for sensor degradation, the same approach could easily be followed with brightness temperature and then cloud information would be retrieved from physical parameters instead of im-

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ager counts. Moreover the fact that for e.g. sensor changes steep jumps of imager counts occur and the authors present a method for correcting for such jumps, the use of brightness temperature would be much more homogeneous, I assume. I also think that at some places in the manuscript the physical assumptions behind the method need more clarification or have to be better elaborated. Specific comments:

p. 1860 l. 13: Please introduce abbreviations when they appear first (CM-SAF).

p. 1861 l. 18: As above (SPARC).

p. 1862 ll. 3-8: As outlined in the general comments: Isn't brightness temperature better suited when dealing with different platforms / sensors?

p. 1862 l. 10: What is SDL?

p. 1862 l. 11: Why is cloud base temperature important (especially for opaque clouds there is no need to know cloud base temperature for spaceborne IR cloud detection).

p. 1862 ll. 14-19: Is this really the case for all kinds of clouds? What about Cirrus or altocumulus? I have the feeling that coud top temperature and sensor brightness temperature are mixed up here (which essentially are not the same for cloud types mentioned above).

eq. 1: It is a bit unusual to present equations directly connected to the method development in the introductions (here more general relationships may be introduced). Moreover the appearance of Cmin and Cmax has to be explained in more detail following the equation. Therefore I suggest to shift eq. 1 to section 3.

p. 1863 ll. 7-9: SEVIRI has different instrument characteristics compared to MVIRI. Does the difference impact on validation results (must be shown)? By the way, which SEVIRI channel is used? Or is a braodband IR signal emulated by combination of more than one SEVIRI channel?

p. 1868 ll. 11-16: The strong contrast in brightness temeparure is not present in case

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of cirrus and also not for stratocumulus. How are these clouds detected? Otherwise the authors have to discuss the underestimation of cloudiness due to the missing signal by cirrus and stratocumulus.

eq. 12: What is "s"?

Section 3.5: As mentioned in the general comments I assume that the approach could easily be followed with brightness temperatures as well. Moreover I do not agree that Cmin determined from tropical convection this way is any well suited to express CTT of deep convection in midlatitudes due to atmospheric path length and its impact on brightness temperature (limb darkening effect). How is viewing geometry and limb darkening accounted for in the method?

eq. 23: Where does the threshold of 0.1 come from? Is it heuristic? The authors should explain the origin of this threshold.

p. 1873 l. 6: I agree that CTT and CTP are somehow linked (in most cases). But CTP is not a steady and monotonous function of CTT and vice versa (e.g. think about temperature inversions like the trade wind inversion above the Atlantic Ocean). Consequently the relationship should be formulated with greater caution.

p. 1873 ll. 7-21: This formulation of CTP is not at all suited for good representation of physical cloud characterisation. If using the US Standard Atmosphere profile, CTT can be used directly to extract an assumption about CTP. I do not agree with eq. 24. In the end the authors do not at all retrieve CTT while they assume an simple exponential relationship between normalized LCI (and thus CTT) with CTP. The whole approach of cloud height (i.e. pressure or temperature) estimation has to be generally revised and expanded or should be removed.

p. 1874 ll. 13-15: 700hPa respective 3km is not at all high cloud. In mid latitude summer this is still boundary layer cumulus!

eq. 28: This equation is not really necessary as it is easy to understand how CFC is

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normalized.

p. 1876 ll. 7-12: The authors could comment a bit on the question how reliable SYNOP observations are at night.

p. 1876 ll. 21-26: Snow detection respective discrimination between cloud and snow has not at all been introduced and also has not been discussed in the formulation of the method.

p. 1878 ll. 10-16: At Sede Boker MVIRI / SEVIRI viewing angles are very high, impacting on validation results. It would be interesting to see he validation results at Sede Boker from SEVIRI (0° Service) compared to those of Meteosat-6 MVIRI at 63° (Indian Ocean Coverage Service), which has a better viewing geometry for that site.

p. 1879 ll. 8-9: What is a possible explanation for the lower altitudes of the ISCCP dataset?

p. 1879 ll. 16-21: What is the implication of these results?

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 1859, 2013.