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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of cirrus cloud optical thickness and top altitude from geostationary remote sensing" *by* S. Kox et al.

Anonymous Referee #2

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The authors provide a precise and well-formulated description of a novel approach for cirrus detection with SEVIRI. I strongly recommend publication in AMT after some minor revisions (see comments below).

l. 24 "net radiative forcing" instead of "net forcing" (also I am missing a reference)

Introduction: The authors state that the Mecida algorith has a lower optical depth limit of about 0.5. It would be interesting to see the detection limits for the other methods listed (e.g. ISCCP, HIRS, TOVS) in order to understand if part of the differences arises from different detection limits.

ll. 100ff.: The near-infrared radiation in the Nakajima and King method also contains reflected sunlight. In the algorithm the authors refer to, which NIR channel is used

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(1.6μm, 2.2μm, 3.9μm)? If the information is from the 3.9μm channel, how is the separation between the reflected part and the thermally emitted fraction of the radiation done?

l. 108: It should be mentioned that retrieval of cloud bottom height is only possible for optically thin clouds as the CALIOP signal otherwise saturates.

l. 172: I would suggest the term "quasi simultaneous" (as they stem from different satellites, the observations are not really simultaneous in a physical sense).

Section 3.2: If I correctly understood the description, all clouds with ice particles at their top were called "cirrus" in the CALIOP dataset (l.334ff). Then I do not understand Fig.2, where the cumulative distribution function (evaluated from the histogram) approximates 1.0 at an optical depth of about 5-6. Does this mean that the number of "real cirrus" (optical thin) observations is so much larger then the observation of deep convective clouds, that the CDF would asymptotically approximate 1 at these optical depths, even if the x-axis would be expanded to higher values (e.g. 50 or 100)? Or does this mean that only CALIOP observations with optical depth lower than 6 have been evaluated, as the signals (both of CALIOP and of the SEVIRI TIR channels approach saturation for this optical depth? If so, a clear description of the saturation criterion and the selection rule is missing. I strongly suggest to describe the procedure how the training dataset was derived and which assumptions and constraints have been used in more detail, as this is very important information for the understanding and correct usage of the output product.

ll. 365ff.: What is the idea behing using the 13.4μm CO2 absorption band cannel of SEVIRI as input brightness temperature, but not as input for brightness temperature difference? As the 13.4μm channel has quite heigh relatove weight (Fig. 4) and the CO2 concentration has a strong annual cycle, does the CO2 signal influence on the cirus detection capabilities of COCS?

l. 385: should be changed to "minumum cirrus top altitude" in order to avoid confusion

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ll. 395ff.: How large is the quantitative eror introduces by the change of radiance definition if the wrong method would be used to calculate brightness temperatures?

ll. 552f.: I would be careful with this interpretation. The period 1 is much longer (about 2 years, when I got the numbers right) than period 2, so the different sample size also could impafct on the resulting accuracy. I would suggest to rephrase the sentence and at least mention the different sizes of the samples.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 4123, 2014.