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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Discriminating raining from non-raining clouds at mid-latitudes using Meteosat Second Generation daytime data" *by* B. Thies et al.

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The authors describe a modified approach for discriminating rainfall occurrence using multi-spectral observations from MSG data. The work essentially involves the development and evaluation of an improved empirical transfer function designed to export high quality rainfall information from ground-based radars presumably to areas where no such observation area available. The advantages of adding effective radius and cloud top phase information from visible, near-infrared, and infrared radiance measurements to more traditional infrared-based measures of cloud top temperature is clearly demonstrated. The technique is clearly outlined, the paper is well-written, and the subject matter is appropriate for Atmospheric Chemistry and Physics.

There are, however, several aspects of the paper in need of further clarification beginning with the lack of tangible motivation for putting effort into refining VIS/IR-based rainfall detection techniques. While it is undeniable that VIS/IR observations provide unique spatial and temporal coverage compared with sensors that are more directly sensitive to precipitation such as passive microwave (PMW) radiometers and radars, the indirect nature of the relationship between rainfall and VIS/IR radiances can introduce enormous uncertainties in the delineation of rainfall from such measurements. The need for rainfall observations to characterize the global water cycle is not in question but the paper leaves me wondering just how we will benefit from high temporal resolution information about rainfall occurrence without any associated intensity measurements (as this work does not begin to suggest how intensity might be predicted from the technique). The authors should spend more time describing the potential applications that are expected to benefit from such a product and demonstrate that the accuracies implied by Table 1 and Figure 3 are sufficient to achieve the goals of these applications as opposed to the less accurate ECST technique. In other words, the results of the paper need to be put into the context of a tangible application.

The discussion of the proposed technique is well laid-out but the manuscript also lacks any discussion of the uncertainties associated with the use of VIS/IR radiance measurements for inferring effective radius and liquid water path. While, the underlying physical basis for the algorithm is sound, there is no mention of the large uncertainties associated with retrieving effective radius from visible and near-IR radiance measurements in the presence for ice (eg. due to crystal habit). It is also unclear that the retrieved effective radii, which generally represent the cloud properties at the top of the system, are really representative of liquid drops that are large enough to fall as raindrops since these drops typically reside in the middle and lower portions of the cloud. Masunaga et al. (2002), for example, use the fact that effective radii retrieved from visible radiances do not accurately represent the precipitating portions of clouds as a means to detect drizzle when coincident PMW LWP estimates are available and they didn't even have the added uncertainties due to the presence of ice to contend

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with. It is also unclear how one should even interpret the effective radius in a cloud where both liquid and ice are present. The section describing the retrieval algorithms leaves the reader with a false impression of the accuracy to which cloud microphysical properties can be assessed in these scenes. What's more, no case is really made for the ultimate goal of using effective radius and CWP for detecting rainfall. Given the large uncertainties involved in retrieving and interpreting the retrieved microphysical properties, the use of a confidence function that relates the raw radiance signatures themselves to rainfall occurrence (as was actually done in the paper anyway) is likely to perform as well if not better than the proposed idea of using output from SLALOM for this purpose. The retrievals just add an additional step where potential biases can be introduced through the physical assumptions required to determine cloud properties.

One of the main weaknesses associated with many satellite-based remote sensing applications is a lack of any means for quantitatively assessing the uncertainty of the technique for any given scene. Unfortunately the approach presented here falls into the same category. While the authors present a statistical evaluation of the performance of the technique using an independent set of radar measurements, they do not present a method for quantifying the uncertainty in their product for any given scene. It seems to me that such an estimate should be possible given the confidence shown in Figure 1 by perhaps retrieving rainfall probability rather than simply delineating the rainfall edge but no effort is made to develop such an approach here.

As a final minor note, the introductory comments and exclusive comparison of the proposed algorithm to just the ECST may also leave readers with the false impression of the superiority of this technique to all existing high temporal resolution rainfall products. Some mention should be made in the introduction of the advantages and disadvantages of approaches that attempt to merge passive microwave rainfall information with higher temporal resolution IR observations (eg. the morphing technique of Joyce et al, 2004).

References:

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