Atmos. Meas. Tech. Discuss., 7, C3745–C3747, 2014
www.atmos-meas-tech-discuss.net/7/C3745/2014/

Atmospheric Measurement Techniques Discussions

Interactive comment on "A novel algorithm for detection of precipitation in tropical regions using PMW radiometers" *by* D. Casella et al.

S. J. Munchak (Editor)

s.j.munchak@nasa.gov

Received and published: 16 November 2014

General comments:

I found the method described in this paper quite simple yet robust in its ability to detect precipitation over a variety of surfaces and applicability to a variety of instruments. The focus of the analysis section is on a statistical discussion of results and comparison to other methods. This section is difficult to read and could benefit from some improved organization starting with division into three subsections (discussion of skill score beginning on page 9250, line13; minimum detectable rate beginning on page 0252, line 26,;and impact on total rainfall estimate beginning on page 9254, line 28). Also, the very long paragraphs on 9251 and 9253 should be shortened; as they stand,

they simple serve to describe Figures 4-6 but with only a few features meriting further discussion. Here, I found the high level of detail interrupted the narrative, so to make the section more readable I suggest that some of these details can be omitted from the text (but still present in the figures themselves) without affecting the discussion.

I also found the discussion on the affect of detection on total rainfall unclear. It seems to be based on the use of the algorithm only as a screen without deriving precipitation rates (although this should be readily available from the CCA procedure). I understand the concept of the "perfect algorithm" as described, but what is the meaning when FPH+FPF is much greater (or less) than 1? Does that imply that the algorithm actually overestimates (or underestimates) rainfall?

As mentioned above, the focus seemed to be on the statistical interpretation of the results; it would be helpful to have a more physically based analysis as well (although perhaps this would be the subject of a companion manuscript). In particular I would be interested in a map of the false alarms and misses over each surface type to see if they are clustered geographically or associated with a particular type of precipitation (ie, shallow warm rain) or environmental variable (e.g., surface temperature or water vapor). This may be beyond the scope of the present work, however.

Specific corrections:

9239 line 3: DPR stands for Dual-frequency Precipitation Radar

9239 line 11: AMSR2 is onboard GCOM-W1, not Megha-Tropiques

9245 line 15: PR minimum sensitivity is 18 dBZ (not 0 dBZ), which is the value that should be used to define echo top. However, since the radiance is an integral function, it does not come exclusively from the cloud top but is weighted through the precipitation column. This weighting function varies by frequency and precipitation intensity. The result is that the parallax effect should be somewhere between zero and that implied by the ds=h*tanθ. To account for this effect properly the authors should perform

the weighting differently for each pixel, but at the very least it would be informative to perform a sensitivity test generating the database with no parallax and ds=0.5*h*tanθ.

9247, line 2: Was the CCAR procedure performed on rain rate in linear or log space? The response of Tbs is generally more linear with respect to log(R) than R, so this may make a difference in the results.

9256 line 20: The pseudo-GMI channel set also does not include dual polarization at 166 GHz which may also be critical for the detection of light precipitation.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 9237, 2014.