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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Field-of-view characteristics and resolution matching for the Global Precipitation Measurement (GPM) Microwave Imager (GMI)" *by* Grant W. Petty and Ralf Bennartz

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*General Comments*

*1. What is the source of information for the IFOV beamwidth? The beamwidths provided in the Hou et al., 2014 are slightly different than those published by Draper et al., 2015 (which, I believe, used actual measurements during calibration maneuvers after launch).*

We have added a remark that the measured antenna pattern data were supplied to us by Kummerow (pers. comm.) and that these are consistent with those used by Draper



et al.

*2. The process of determining optimal weights seeks to maximize the correlation between channels, which implies that the radiances themselves are more dependent on resolution than on the channel. Of course, if this was the case, there would be no need for the low-resolution channels, yet we utilize them precisely because of the additional information content they present relative to the higher frequencies despite the lower resolution. One can imagine a case where the 23.8 Ghz channel is deconvolved to the 89 GHz FOV. Since the 23.8 channel is more sensitive to the water vapor, which varies smoothly, whereas the 89 GHz radiances are much more sensitive to liquid clouds, which have sharper boundaries, the incorrect weights might be selected that overamplify noise in an attempt to reproduce the cloud response at 89 GHz with the water vapor response at 23 GHz. While this is admittedly an extreme case (and there are many other situations, such as a coastline under clear conditions, where the radiances are quite similar at different frequencies), I think a little more discussion is merited of the strengths and weaknesses of using inter-channel correlation as a proxy for optimal resolution enhancement. Why not simply use a high-resolution model where simulated radiances may be obtained at any frequency and resolution, and attempt to maximize the correlation between deconvolved and actual high-resolution radiances at a given frequency?*

The reviewer's comment is apparently based on a misunderstanding of the method we used. As described in Eq. (3), the correlation we maximized is strictly spatial and doesn't involve brightness temperatures at all; the method does not depend on the geophysical information content of any channel. We are in effect minimizing the RMS difference between the SHAPE of the target EFOV and the SHAPE of the synthetic EFOV produced.

*3. The 166 and 183 GHz channels were not treated in this paper due to the inconsistent scan geometry relative to the 10-89 GHz channels, but I am curious if it is possible to determine their weights as a function of the (variable) cross-scan and along-scan* *distance from the center of the high-frequency FOV to the center of the target low-frequency FOV. Would such averaging be superior to the current method used in the 1C-R product, which (as I understand) is simply a nearest-neighbor match to the target low-frequency FOV?*

Yes, such a method is possible, and it would probably yield superior results to the current method. Whether the improvement would be significant enough in the context of current applications of those channels to warrant the effort of storing and applying a family of different sets of coefficients is an open question. We have added a comment acknowledging this question.

*Specific Comments*

*Page 1, Line 12: F19 was actually launched more recently than GMI (although no longer operational).*

We have modified the text in question to avoid any misunderstanding.

*Page 1, Line 15: The 1/(d*freq) relationship for beam width only holds for the 10-18-23 GHz channels on GMI - at higher frequencies, the antenna is "under-illuminated" resulting in larger beamwidths than the minimum possible, presumably in the interest of minimizing under-sampling the scene at high frequencies (although, as the authors note, this is an issue at 89 GHz and above for GMI). Although this doesn't affect the result in any way it should probably be noted in the introduction.*

So noted, thanks for this information.

*Page 5, Line 27: This sentence seems incomplete, as it doesn't follow directly from the preceding paragraph. What exactly is being derived in Appendix A?*

Fixed.

*Figure 4: This figure displays correlation and gamma as a function of noise amplification factor, but the prescribed variable is actually gamma and I think it would be more*



*logical to view both correlation and noise amplification as a function of gamma.*

The decision how to present the results is of course subjective. From our perspective, gamma is just a "knob" to turn when seeking to optimize the tradeoff between noise amplification and correlation, plotted in the upper panel of each subplot; gamma has no intrinsic significance beyond that function. By presenting the results as we did, we can focus first on the essential tradeoff (top panel), choose where we want to be on the curve, and then drop straight down to intersect the curve of gamma vs. noise amplification and determine the corresponding value of gamma from the left axis.

*Figure 6: A color bar would be useful to interpret the magnitude the of the ringing artifacts.*

We have added color bars.

*Typos*

*Page 1, Line 7: should be "Modest improvement in resolution is achieved for the 10.65 GHz channels, . . . "*

Fixed.

*Page 4, Line 12: should be "Advanced Microwave Scanning Radiometer"*

Fixed.

*Page 8, line 15: equation should end with a period.*

Fixed.

*Table 2, last row: extraneous "km" after 52.78 degrees.*

Fixed.

*Table 3: According to Draper et al., 2015, the Ka-band GMI frequency is 36.64 GHz.*

All references to the 36.50 GHz channels have been changed to 36.64. Thank you for pointing out the adjustment.

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