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Atmospheric Measurement Techniques Discussions

Interactive comment on "Performance assessment of a triple-frequency spaceborne cloud–precipitation radar concept using a global cloud-resolving model" *by* J. Leinonen et al.

J. Leinonen et al.

jussi.s.leinonen@jpl.nasa.gov

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We thank reviewer #1 for the helpful and extensive comments. Our point-by-point responses can be found below each comment in italics.

Very interesting paper and utterly important for shaping the next multi-frequency radar missions targeting clouds and precipitation! The paper is generally very well written, with very high quality figures. With the following comments I want to foster the authors to do an extra effort (this is why I ticked the major revisions box) in order to make it even more valuable. I have three major comments

1. The authors are proposing two ACE-mission configurations and they are producing maps of high-quality detection in clouds and precipitation. In order to strengthen the message and sell the mission it would be nice to put their results in the context of the current missions deploying atmospheric radars (i.e. GPM and CloudSat). In other words I would like to see included in Fig.4 the results for such systems (you can keep your 100% normalization factor to ACE 450 km or maybe use CloudSat as a benchmark). On the other hand Fig7-9-11,. . .. look quite repetitive: I would eliminate them and include their results in a Table (where it is actual easier to compare results).

Adding the comparisons to Fig. 4 might make it more confusing, and in any case many comparisons could not be made unambiguously because of the different number of frequencies on GPM, CloudSat and our proposed radar. However, the single-frequency detection rates can be compared. We have added a paragraph at the end of Sect. 5.1. describing the improvements in single-frequency detection over GPM and CloudSat.

After discussing the issue with the bar plots of Figs 7,9,11... we decided to keep the plots as we felt that they give a quick visual comparison of the different detection classes. The bar plots have been reorganized to make them clearer by using common limits in all plots throughout the paper and by improving the readability of the error percentages.

2. Validation: I very much appreciated the effort done for producing Fig.2. It would be nice to produce an equivalent figure based on GPM-DPR data to see how well the NICAM model can capture the reflectivity variability in rain. Given the fact that it is just a question of changing configuration it should not be a lot of work. However, it is clear that for this kind of study it is not only about capturing the right pdf of your reflectivity. For what you are doing it is crucial to properly capture also the horizontal variability of clouds (because you are looking at NUBF). Therefore you should also look at that aspect (the paper Evaluation of EarthCARE Cloud Profiling Radar Doppler Velocity Measurements in Particle Sedimentation Regimes, JTech, 2014, Kollias et al., could help).

We have now added GPM data to the comparison in Fig. 2. The article text was updated to cover also the NICAM-GPM comparison.

We have also added a brief evaluation of the horizontal structure, similar to the reflectivity difference CDFs of the paper mentioned by the reviewer. We used CloudSat data as a reference product for this purpose, as its horizontal resolution is better than that of GPM. This is now found in Fig. 2b and explained in the last paragraph of Sect. 3.

3. Error definition: there is quite some degree of freedom in defining the classes for erroneous error. First, you should write a line to explain why in eq 26 you are using this combination for evaluating NUBF effect (you want both to check the variability of the effective reflectivity and that of the integrated path attenuation, if I am not mistaken). Second, it is not clear to me why the 2 dB threshold has been selected. You may want to pinpoint at specific references there (if any) or articulate a little bit your reasoning.

Eq 26 tries to capture in one simple functional whether either of the sources of error affects the observations by making them depart from the standard 1-D single-scattering radar equation. The first term captures NUBF and the second captures MS. Within the first term, the two factors account for the effect of NUBF on reflectivity at that range (Ze is not affected by attenuation) and for the effect of NUBF on the attenuation above it (Zss is affected by attenuation).

2 dB was chosen as representative of the overall accuracy (accounting for calibration and precision) of the CloudSat CPR and the GPM DPR.

These points have now been clarified in the paper.

Minor comments 1) Line 20 page 4152: effets

Typo corrected.

2) Line 8 page 4153: I guess you are integrating along track to achieve the sensitivity you have in Table 1. Thus I am not sure you can do this. You are underestimating NUBF.

The effect of along-track integration is already accounted for in the footprint size. The paper has been updated with a mention of this.

3) Eq.26: You should write a line here to explain why you are using this combination for evaluating NUBF effect (you want both to check the variability of the effective reflectivity and that of the integrated path attenuation if I am not mistaken).

Please see our response to major comment #3.

4) Line 23 page 4155: add "for all three bands" after not satisfied (right?)

The reviewer's interpretation is correct; this has been clarified in the text.

5) Line 3 and 10 Page 4159: is it attenuated or is it multiple scattering? I guess both

Both effects are certainly present, but since multiple scattering generally results in a stronger signal than a purely attenuated signal would be, the attenuation is responsible for bringing the signal below the detection limit. Thus line 3 was not changed, but multiple scattering is now mentioned on line 10.

6) Line 22-24 Page 4159 : I can't understand how this is possible.

A conceptual drawing of this effect is given in the attached plot. There, it can be seen that the wider beam results in the signal being detected over a larger area. In the case discussed in Sect. 5.2, the magnitude of this effect is larger than the area-decreasing effect of the loss of sensitivity. More detail was added to the sentence in order make it clearer to the reader why this effect arises.

7) Line 23 Page 4160: the only change to only

This typo was corrected.

8) Tab1: This is nominal sensitivity at what integration distance? There is a 5.2 dB difference due to the altitude. You are just approximating it to 5, right? A word about the rationale of not having beam-matched, i.e. for using the same antenna size at all

freqs.

Yes, the 5.2 dB difference is rounded to 5 because all numbers in Table 1 have been rounded to integer values. The same antenna is used for all sizes as a cost-effective design compromise. Approximate beam matching can be achieved during post-processing by spatially averaging the data from the bands with smaller footprints. This is now stated Sect. 2.4.

9) The pdf of 94 GHz is not “outstanding”; you are clearly underestimating MS because you are missing a lot of the high reflectivity (dense ice aloft?). That’s why I think it would be good to see if you capture the pdf at Ku and Ka by comparing with GPM-DPR.

Please see our response to major comment #1.

10) Fig4: the sum of the 450 km in the top left panel is not 100%! Check all of them.

As far as we can see it does sum up 100%, as do the other panels in this figure (except for rounding errors of the order of 0.01%).

11) I am a little bit surprised by this result! As a rule of thumb for CloudSat I remember from some of the plots I produced you have a drop of 10-15% coverage once you drop by 5 dB in sensitivity. It is strange now that you get only 5% reduction. This maybe due to the Z distribution problem

This is an interesting remark. If the reviewer is able to provide a reference for this “rule of thumb”, a short discussion of this could be added to the article.

12) Fig 12: this profile makes not much sense. It should be all clutter, shouldn’t it? I think going down to 400 m as you assumed is also quite challenging. You may mention how you are going to achieve such performances (keeping sensitivity high!)

We thank the reviewer for pointing out that Fig. 12 did not specify the impact of ground clutter in this scene. This has now been corrected. All figures and analyses have also been updated, where necessary, to properly account for the effect of ground clutter.

As for achieving the 400 m limit, the numbers adopted in this paper are in line with the estimates of the ACE Science Working Group (Tanelli, S., Durden, S. L., Im, E., Heymsfield, G. M., Racette, P., and Starr, D. O. (2009, May). Next-generation space-borne cloud profiling radars. In 2009 IEEE Radar Conference, 2009 IEEE (pp. 1-4). IEEE). Furthermore, this is a paper addressing a notional radar, and providing all the details of the system that would result in that particular sensitivity would require studies well beyond the scope of this paper, would be detrimental to the generality of this assessment, and could violate intellectual property, competition sensitive data and export control rules.

13) Fig.14: Vertical cut is missing

This is found in subfigure 14d; this is now clarified in the caption.

14) Page 4163: Line 6 There is also a paper recently published by the same first author in JGR ‘Multiple scattering in observations of the GPM dual-frequency precipitation radar: evidence and impact on retrievals’ which gives evidence of multiple scattering in GPM Ku-Ka observations in presence of dense ice aloft.

Not the same first author, but this reference was added now that the study is citable.

15) Page 4164: “dual-polarization techniques” It would be good to explicitly mention what the authors have in mind here (polarization diversity? ref?).

While the benefit of dual-polarization is limited in nadir-pointing radars, at least the linear depolarization ratio (LDR) could be used for melting layer detection. This example is now given in this chapter.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4137, 2015.

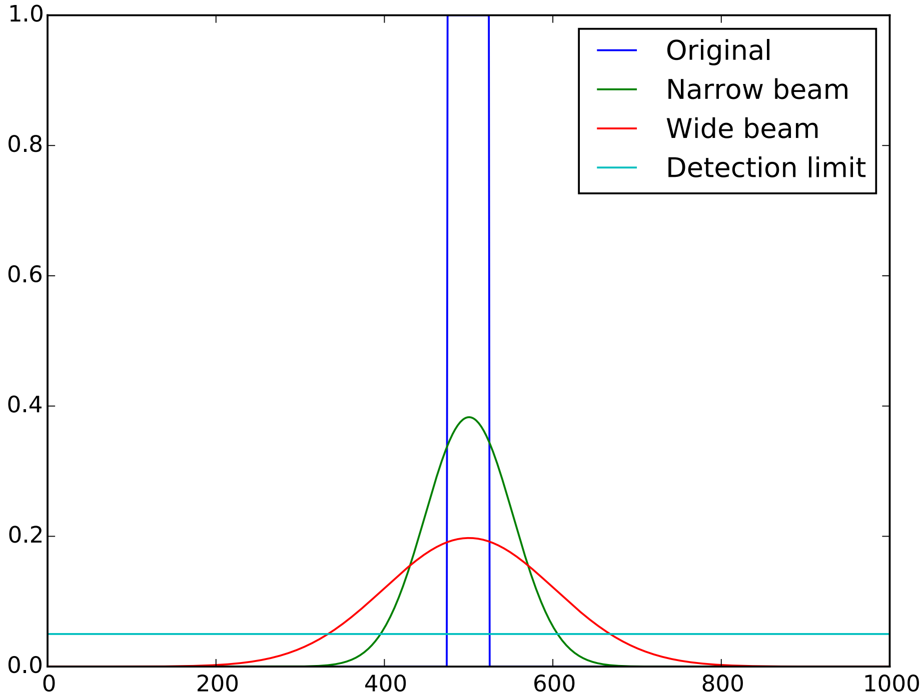

Fig. 1. A conceptual example of nonuniform beam filling leading to a larger number of detected points.