

## ***Interactive comment on “What millimeter-wavelength radar reflectivity reveals about snowfall: An information-centric analysis” by Norman B. Wood and Tristan S. L’Ecuyer***

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The authors present a snowfall retrieval based on radar reflectivity and temperature. While similar retrievals have been developed before, the focus of using only a single radar reflectivity and the extraordinary detailed error analysis makes it nevertheless an important contribution. The paper is well written, shows attention to detail, and the figures are clear. I have quite a few comments, but they are all of minor importance and I recommend the paper to be published subject to the following comments:

General comments:

C1

- Does this paper describe how CloudSat’s 2C-SNOW-PROFILE works? If yes, I would recommend to say so. If not, I would recommend to mention the differences
- I wonder how does this retrieval compares to traditional Ze-P relations? Clearly the sophisticated error estimates are an advantage, but what about the absolute P values? For several fixed temperature values, can the authors plot P as a function of Ze? This would allow to see 1) where the retrieval deviates from a power law form, 2) the impact of temperature on P, and 3) how it compares to published Ze-P relations

Specific comments

- L120: I assume the authors refer to the a posteriori covariance of  $x$ ?
- L124: Is this the test shown in chapter 12.3.2 of Rodgers, 2000?
- L169: Do the authors underestimate  $D_{max}$  when they use a measurement by an optical instrument? Isn’t it quite unlikely that an individual particle is rotated such that the true  $D_{max}$  can be observed?
- L174: does the  $\log(N_0)$  distribution have a Gaussian shape?
- L204: Do the authors think that their results are also applicable to high latitude locations?
- Figure 1) Why is the aircraft based  $N_0$  higher than the ground-based? Has the C3VP dataset been corrected for in situ probe shattering effects?
- L228: Typo in SF

C2

- L233: I would say that Optimal Estimation cannot handle biases at all. I think it is perfectly acceptable that the authors assume that the CPR does not have any bias, but I would recommend to remove 'uncertainty in the absolute radiometric calibration'
- L247: Defining  $K_b$  is a very important step, I would recommend to spend 2-3 sentences on it instead of referring only to previous work.
- Figure 3: Add to the caption that measurement uncertainty is shown.
- L266: I appreciate that the authors do handle the errors sources conservatively and do not oversell the retrieval's uncertainty, but I wonder whether they are a little bit too pessimistic here: A radar always observes thousands of particles, isn't it quite unlikely that they are all of the same kind? Maybe a more recent bulk scattering method such as SSRGA would work better?
- L281: Why didn't the authors use the follow up paper by Heymsfield and Westbrook (2010)?
- L286: I would recommend to provide some details about  $S_b$ , I guess it contains the uncertainties of the  $m(D)$  relation?
- Figure 8: A more convincing evaluation example would be to use a different data set, e.g. from the high Arctic
- L345: I wonder whether the discussion about accumulation errors is relevant for CloudSat since it can provide only a snapshot of the current measurements?
- L376: Why is the number of states 0.9 higher than  $H$ ?
- L378: A couple of years ago, I had the same problem and, after thinking about it a long time and checking my code many times, came to the same conclusion, i.e.

C3

it is related to high correlations. However, I looked into the same issue recently and found that the negative values on the diagonal of  $A$  disappear after I added checks making sure that my covariance matrices are not singular: Python's (and I guess this applies to other languages, too) built-in inversion routine is quite forgiving and also inverts matrices that are 'slightly' singular. However, these instabilities can add up and many matrix inversions later lead to a negative entry on the diagonal of  $A$ . And it turned out I had created the singular matrix by myself by applying the authors' eq. 16 which added some numerical noise making my  $S_{\text{Epsilon}}$  singular and non-symmetric. After making sure that my  $S_{\text{Epsilon}}$  is really symmetric and nonsingular (i.e. doing a rank test), negative values on the diagonal of  $A$  disappeared. I admit I never investigated this systematically, so it could be a coincidence, but I would be curious to see whether the authors' negative values are also related to numerical instabilities. In the end, this appears to be a cosmetic issue and not very important: the total degrees of freedom and all other results stayed the same in my sample retrieval.

- Figure 12: I'm surprised that the  $d_s$  values are not higher. A couple years ago I developed an ice cloud retrieval where I, because it was an information content study, simply added everything to the state vector: 3 PSD parameters, 2  $m(D)$  parameters, and 2  $A(D)$  parameters. With such a large state vector, I always got two  $d_s$  when using  $Z_e$  and mean Doppler (I never tried only  $Z_e$ ). The fact that  $d_s$  is not 1 in the authors' study might mean that a little bit of information is unused. I wonder whether this information could be used and  $d_s$  would be 1 if the  $m(D)$  parameters were moved from the  $b$  vector to the  $x$  vector. This might lead to lower  $P$  uncertainties. This would also help with the issue raised in L419. Of course, this includes the challenge to make sure that the retrieval doesn't put all the information into  $m(D)$  instead of the PSD parameters which probably would make the  $P$  uncertainty even larger.
- L450: Add the DOI

C4

Heymsfield, A. J., and C. D. Westbrook, 2010: Advances in the Estimation of Ice Particle Fall Speeds Using Laboratory and Field Measurements. *J. Atmos. Sci.*, 67, 2469–2482, doi:10.1175/2010JAS3379.1.

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