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Interactive comment

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

Maximilian Maahn (Referee)

maximilian.maahn@uni-leipzig.de

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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:

- 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 Dmax when they use a measurement by an optical instrument? Isn't is quite unlikely that an individual particle is rotated such that the true Dmax can be observed?

- L174: does the log(N0) 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 N0 higher than the ground-based? Has the C3VP dataset been corrected for in situ probe shattering effects?

- L228: Typo in SF

- 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 Kb 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.

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_Epsilon singular and non-symmetric. After making sure that my S_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 Ze and mean Doppler (I never tried only Ze). 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

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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