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Atmospheric Measurement Techniques Discussions

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Interactive comment

# *Interactive comment on* "Thin ice clouds in the Arctic: Cloud optical depth and particle size retrieved from ground-based thermal infrared radiometry" *by* Yann Blanchard et al.

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This paper describes a novel method to determine (thin) ice cloud optical depth as well as some limited mean particle size information using a combination of lidar data, IR radiometry and atmospheric thermodynamic state information. The technique appears robust and appears reasonably easy to implement. It could also be a candidate for network deployment.

The paper is, in the main, clear and well-written and worthy of publication. Another reviewer has already noted a number of issues. I have noted a further few aspects that should be improved before final acceptance.



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Discussion paper

P1: line 1: What type of profile information ? Please be specific here.

P11: Lines 25-28: The authors should expand the lidar multiple-scattering discussion. It is not quite satisfying/convincing to me. I agree that the application of Eloranta's formalism is appropriate, (it would be useful if they specified the equation number of the formula they used) however, they must follow through to discuss the errors in terms of optical depth and not leave the discussion solely in terms of Pt/P1. Also, the discussion must be made much clearer.

For example, (looking at Eq. 4) it is clear that the important factor in determining the COD is the ratio of the signals at cloud base and cloud top. Thus, the relevant quantity for determining the effect of MS is the Pt/P1 ratio at cloud top (since at cloud-base Pt=0). However, curiously, the worst-case cloud-base height is defined as 5.5 km but the cloud top altitude is not specified.

Assuming that the worst-case Pt/P1 ratio value of 60% quoted by the authors is indeed the value at cloud-top. The error in COD induced by MS effects for the worst-case scenario can be easily calculated. Using Eq.4 it is easy to show that the effect of MS will be to lower the retrieved extinction by an amount given by

dCOD_ms = -0.5 log (1.0+Pt/P1)

and Pt=0.6 P1 implies that dCOD_ms= 0.23 which is about a -10% bias.

Moving on, it is not clear what the authors mean by the "overall average value for the Pt/P1 upper limit". Is that the altitude averaged value for the just described "worst-case" scenario or the average ratio at cloud-top over the investiged cases ? If the former, then it is not a useful quantity. If the latter then that would correspond to a COD retrieval bias of -0.05 which could be significant for many of the results presented in this paper (e.g. see Fig .6 before about 19:00). Indeed accounting for the MS effects may bring the IR radiometer and lidar results more into line with each other values.

The authors have enough information to define the COD, cloud geometry and particle size ranges they are dealing with. Using this information and Eloranta's model I think with not too much effort they could define and apply a mean eta factor as was done in the paper by Platt which they reference.

Page 20: Line 5-10: Can the authors please comment on how realistic it is to assume that all the measurement errors on the IR BTs are indeed independent ?

Page 20: Line 14: sigma/sqrt(1000). What is the significance of the sqrt term ?

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