Atmos. Chem. Phys. Discuss., 14, C4269–C4272, 2014
www.atmos-chem-phys-discuss.net/14/C4269/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol indirect effects on continental low-level clouds over Sweden and Finland" *by* M. K. Sporre et al.

Anonymous Referee #1

Received and published: 30 June 2014

Review of the manuscript by Sporre et al. submitted to ACPD.

Sporre et al. have studied aerosol indirect effects on low-level clouds using ground based aerosol measurements and remote sensing data. The research topic is very interesting but, unfortunately, this manuscript does not bring anything new to it. All the conclusions of the manuscript have been published previously in other papers, some of them by the same authors. Furthermore, the methods used are not completely sound. For example,the authors base a number of their conclusions on correlations that are almost non-existent. Therefore, I suggests that the editor rejects this manuscript.

Here are the detailed comments:

1) Significant correlation. This phrase is used a lot in the paper as a basis for the

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conclusions. However, when you look at the correlation coefficients they are in many cases below 0.35 which basically means that there is no correlation. Even though the p-values indicate significance, it doesn't mean that there is correlation between the parameters. The p-value just tells you how unlikely a given correlation coefficient will occur given no relationship in the population. The authors seem to have forgotten this. Futhermore, I'm not even sure if correlation is the best metric for this kind study. Maybe more advanced statistical methods should be used.

2) The data are from two sites (Vavihill and Hyytiälä) but at the end of several sections it is mentioned that the number of measurements from Vavihill are so small that you shouldn't trust those results. This raises the question, that why are those results presented in the first place?

3) The particle number concentrations were averaged for 5 hours and satellite data for 90x65 km2. Why did you choose to average so much data? Typically, when comparing ground based aerosol measurements with satellite retrievals you use 1 h and 50x50 km2 averages,respectively (see Ichoku et al. (2002) for more details). The basic idea is that the different instruments should have measured the same air masses. I'm not sure if that is the case in this study.

4) The results are compared to other studies but, usually, the comparison limits to a statement that others have got different kind of results. It would more useful for the readers if you could explain why the results are different.

Technical comments:

page 12933, line 5: Do "natural" aerosols have indirect effects?

p 12935, l 15: You mention in the inroduction that low-level clouds are affected by anthropogenic pollution but then you use background stations to study it. That sounds contradictory.

p 12936, l 11: What was the temperature limit for the cloud screening?

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p 12936, l13: Distortion of what?

p 12936, l 17: What are the spatial resolutions of the used products?

p 12937, l 4: If Re (3.7) is 10 μm larger than Re(2.1), why does it mean that Re(2.1) is overestimated?

p 12937, l 5: What is the size and maximum amount of pixels in the scene?

p 12937, l 27: What is the maximum amount of pixels in the scene?

p 12939, l 10: "only the ACI is calculated" –> the ACI is calculated only

p 12940, l 30: What could cause the difference between these studies?

p 12941, l 17: I wouldn't say that Re and RH are positively correlated if the correlation coefficient in only 0.25.

p 12941, l 19: Are the Re values also logarithmic in the figure?

p 12941, l 24: "The r2 values obtained ... are low due to large scatter.." Isn't this quite self evident?

p 12942, l 10/p12943, l 27: How did Jansen et al. (2011) measure Re and COT? Could different methods explain the differencies in the results?

p 12943, l 1: The correlation coefficients between COT and Re vary between 0.24-0.34. The numbers are positive but I wouldn't call it correlation.

p 12943, l 3/p 12944, l 3: The correlation coefficients between COT and RH vary between 0.18-0.31. These parameters are not "significantly correlated", they are not correlated at all.

p 12943, l 24: The correlation coefficients between LWP and N130 vary between -0.1 and -0.17. That does not indicate weak correlation, it indicates no correlation at all.

p 12944/Fig. 6: It would be easier to compare to the results by Lihavainen et al., if you

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would also have a line that is not binned by LWP.

p 12944, l 27: Shouldn't the number of data points affect the signicance of the correlation? In the Hyytiälä plot, the correlation is significant for the dark blue line (LWP:155-200) but not for the green (65-110) and red (20-65) lines when looking at the smaller cut-off diameters. Despite that you indicate that the red and green lines are more trustworthy than the dark blue line. Seems a bit contradictory.

p 12945, l 9: Why doesn't the low number of measurements affect the significance in Hyytiälä?

p 12945, l 13: What could explain the different results?

p 12945, l 21: The highest correlation coefficient for dbcz is 0.28 which basically means that it is not correlated with other parameters.

p 12945, l 23: Please give the correlation values in the text.

p 12947, p 1: How is this a surprise if you have already mentioned that Twohy et al. (2005) and Constantino and Breon (2013) found the same thing?

References:

Ichoku, C., D. A. Chu, S. Mattoo, et al., A spatio-temporal approach for global validation and analysis of MODIS aerosol products, Geophys. Res. Lett., 29 (12), 2002

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 12931, 2014.