

Review of “Climatology of aerosol optical properties in Northern Norway and Svalbard” by Y.-C. Chen et al.

This article compares Aerosol Optical Thickness (AOT) and Ångström exponent in two **A**erosol **R**obotic **N**ETwork (AERONET) sites (Andenes and Hornsund) using a three years dataset. The paper gives a very detailed description about instrument, sites and dataset. My main concern with the paper is that it is too descriptive, with little science conveyed. I suggest the paper be resubmitted after more detailed explanations can be included.

Specific comments:

1. I am very curious why only three years dataset were used in the paper. The data record for Andenes is from 2002 to 2012 and for Hornsund is 2005-2012. Also, why only these two sites were chosen for comparing? The paper refers to one literature (Rodríguez et al., 2011), Rodríguez et al. (2011) focus on the campaign during international polar year 2007, with a very comprehensive analysis using the relationship between AOT and Ångström exponent, aerosol type and HYSPLIT tool. My question is that what makes this paper special for analyzing the (sub) Arctic ground-based measurements compared with Rodríguez et al. (2011)?
2. A more complete literature review for Arctic aerosol research is needed in the introduction part to describe progress for Arctic aerosol measurement and analysis. Also, basic concepts of aerosol, aerosol classification and aerosol effect in the introduction part should be reduced; otherwise, it seems to be more of a report that could be available on some website rather than a scientific paper.
3. The paper compares both AOT and Ångström exponent for Arctic, there is no problem for the accuracy of Arctic AOT, how about Arctic Ångström exponent? Are there any literatures about validation of Ångström exponent over Arctic?
4. Some figures are not informative enough, for instance, figure 1, 3 and 7. I may suggest delete figure 1 and leave the location information in the text. As to figure 3 and figure 7, I suppose that maybe tables are better than figures.
5. To my view of points, the main problem in the paper is that the authors described some data or phenomenon, unfortunately, without further explanation or support for previous researches. For instance,
 - P7622, Line 22, “This phenomenon has not been observed in sub-arctic areas”, this sentence should be clarified.
 - P7625, Line 14 where “the singular large value” comes from? Dataset problem causing by cloud screening or this is a very serious pollution event?
 - P7627, Line 7 “which might be due to the influence of smoke or some other kind of anthropogenic pollution”, this is too descriptive, I guess maybe this sentence can be used in most cases with relatively high AOT.
 - P7628, Line 6 “Figure 6 shows the fine-mode and coarse-mode contributions to τ at the two sites...”, there is no any further details for why fine mode dominates.
 - P7628, Line 19, how this “seasonal aerosol size distribution” was calculated?

Minor revision:

- P 7620, Line 3, Full name for AERONET should be included.

- P7620, Line 4, I may suggest to remain two digits for the location information of AERONET, especially for Hornsund, the accurate longitude for Hornsund is 15.560278° E.
- P7621, Line15, which wavelength for α ?