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Review of ‘Contrasting Trends of Mass and Optical Properties of Aerosols over the Northern Hemisphere from 1992 to 2011’ by K. Wang, R.E. Dickinson, L. Su and K.E. Trenberth

Introductory Remarks

The authors use regional databases for the Northern Hemisphere to compare PM10 and PM2.5 mass concentration data with daily mean visibility observations which are then converted to optical extinction coefficient values. The work is carried out for 4 regions over the Northern Hemisphere – that of Canada, China, Europe and the US. The result of a decreasing PM10 over the past decade or more is already fairly well established from sources such as National and Regional Environmental Agencies. The findings regarding PM2.5 variations since the mid to late 1990’s are more interesting, as is the decrease in visibility for 3 of the 4 regions examined.

Comments

Since PM2.5 is a measure of the mass of aerosol particles of size which are the dominant influence on optical extinction coefficient, a serious omission from the paper is an investigation of annual anomalies for PM2.5 – which is presented for PM10 in Figures 9 and 10. This might have provided evidence in support of the statement made in the paper [Page 17923, Line 16-21], which infers an increase in number concentration of fine smoke particles (and a consequential increase in PM2.5 mass concentration) due to increased number of wild fires in the US. However having said that, while Figure 6 shows fairly constant PM2.5 levels over the period from about 2002 to 2007 (with the exception of 2005), an overall decrease in PM2.5 levels occurs over the period from 1998 to 2010. In summary, the explanation given is not entirely convincing.

It is also not clear why monthly anomaly plots analogous to the Figure 9 and 10 annual anomaly type plots were not presented or discussed.

Auto-correlation analyses might have been undertaken to assess how other co-variates influence optical extinction. For example, the authors could have investigated how mean dew point influenced optical extinction, and thus assess the role of hygroscopicity on extinction for the different regions.

Other Comments/Queries on material in order of appearance in the paper:

Page (P) 17916 Line (L) 21-23 It is not evident how the correction of the impact of relative humidity on visibility was made, nor is it described in detail in the paper

P17916 L27 and sentences that follow ‘These observations’ – should ‘spell out clearly’ what are ‘these observations’. It would seem that ‘these observations’ refer to ‘manual’ observations; yet the follow up sentences seem to infer otherwise

P17919 L23-25 The number of urban, suburban and rural Canadian sites are not given for PM10 and PM2.5 – they should be specified, as it is not straightforward trying to infer numbers from Figures 1, 2

P17921 L15 The measurement method used for the three regions should be described

Lack of knowledge of the method(s) of measurement of PM10 for Chinese sites is deemed to be a weakness of the paper

Table 1: ‘Total’ is not explained and should be

Figure 6: There is no explanation given as to the increase in PM2.5 in Europe in 2005 – and which should be given

P17923 L3 ‘finer particles, estimated from visibility measurements’ This requires clarification. Firstly, what property of fine particles is estimated ?, and secondly, on what basis is this property estimated?

P17923 L3-4 ‘finer particles have an opposite long-term trend to that of PM10’ This statement needs both clarification and explanation – Figures 4 and 6 show both PM10 and finer particles (aka PM2.5) having declining or negative trends over the longer term

P17923 L12-13 It is not made clear why ‘the largest seasonal variations’ of both PM10 and optical extinction necessarily leads to the ‘strongest’ correlations

P17924 L1-3 Extinction coefficient derived from satellite inferred aerosol optical depth (AOD) is a column integrated value over the whole atmospheric vertical column. The inference of aerosol extinction very close to ground level from AOD clearly requires an assumption of vertical path homogeneity – which may not indeed be the case and so its comparison with surface based aerosol optical extinction is not in general directly comparable

and may well be in error. In addition, the satellite signal is over a much shorter time scale than a 24 hourly averaged surface extinction value
P17924 Section 3.4 The data shows relatively high correlation coefficients between extinction coefficient and dry day occurrences for China and Europe but much poorer correlation for the US and Canada. Why? Indeed Figure 9 for the US, shows for years 1999 and 2000, no discernible effect on extinction or PM10, despite a relatively large increase in number of occurrences of dry days.
As mentioned above, similar type analyses for PM2.5 should be carried out with respect to extinction and dry day occurrences, as presented in Figures 9 and 10

<u>Other comments</u>

A discussion of limitations and consequences of the use of a daily average of visibility, as pointed out by Husar et al (2000), are lacking in the paper, and at least should be commented upon.

Attempting to seek a correlation between mass (proportional to aerosol particle diameter cubed) and optical extinction coefficient (proportional to diameter squared and dependent on particle size distribution and refractive index) is problematic. One may of course obtain a good correlation between mass and extinction for a given location for a particular airmass in which the aerosol particle characteristics do not vary greatly with time. However, there is no sound physical basis to expect a strong correlation between particle mass and particle extinction coefficient and correlations found in the paper are arguably fortuitous.