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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global trends in visibility: implications for dust sources" *by* N. M. Mahowald et al.

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This manuscript describes the use of global visibility data to investigate trends in dust emissions for the past 30 years. A major portion of the paper is dedicated to assess how well the visibility derived dust proxies data agree with measured optical thicknesses, to evaluate to which extent the visibility data actually represent atmospheric dust. Difficulties in using this type of data are pointed out. The results remain in part ambiguous due to the complexity of the problem. While the use of these visibility data as proxy for dust sources has inherent problems, their potential as indicator for decadal trends in dustiness should certainly be explored. I recommend the publication of this paper in ACP after addressing the following points for clarification:

(Page and line numbers refer to those in the ACPD online and print versions)

1. One of the problems using visibility observations from weather stations to assess atmospheric dust emissions is the fact that the network of weather stations is rather sparse in desert regions, such that soil dust variability may not be realistically captured. Also, the location of weather stations may be biased towards relatively densely populated areas. I suggest addressing these drawbacks already in the introduction section.

2. Page 3016, line 13: Holbren=Holben

3. Page 3017, line 3: Reference is missing

4. Page 3018: A (very) brief description of the AERONET AOD measurement method should be added, as there is a strong emphasis on those data in the evaluation of the visibility data.

5. Page 3018: Comparison of AOD and visibility: What is the reason for comparing monthly data opposed to daily data (which are available in the datasets)? For the relative location of AOD and visibility stations it would be interesting to show them in one single map, as the comparison of Figures 1 and 4 does not reveal which weather stations are actually used for the comparison. A possible solution may be to add the relevant visibility stations in Figure 1.

6. A reference to the recent works of Moulin and Chiapello should be added (Moulin and Chiapello, 2004, GRL; Chiapello et al., 2005, JGR). The author discuss the dependency of Saharan dust transport on NAO and the Sahelian rainfall deficit on the basis of an extended time period compared to the Moulin et al. 1997 publication.

7. Page 3020: The authors use simple correlation, stating that the distributions may not be Gaussian, but that rank correlations show similar results. Why not show the rank correlation results instead then?

8. Page 3022, line 8: It is unclear what is meant by 'dust variability' in this context, please replace by a more precise formulation.

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9. Page 3023, paragraph 1: The better agreement of the model surface extinction and AOD compared to the observed correlations might be due to model deficiencies in formation of a boundary layer in the model. Also there may be a cloud bias in the comparison of model results and observations, as to be really comparable only cloud-free model days should be evaluated.

10. Page 3023, line 20: I do not understand the statement that the correlations may be strong because of the dry conditions, please explain. Does this relate to hygroscopic growth of soluble aerosols under humid conditions? This would affect both AOD and visibility, however.

11. The main explanation for the lack of correlation between observed surface extinction and column AOD would be the existence of confined elevated dust layers (which are frequently observed downwind of the source regions), with relatively clear air near the surface. This should be explained more clearly in the text.

12. Page 3023, line 26: There is a limited number of Aeronet stations and meteorological stations used to obtained the results in Table 2, therefore not only the location of the Aeronet stations should be given (which is done in Table 1), but also the name and locations of the nearby meteorological stations and the distance between Aeronet and meteorological stations used should be given. Also, what is the distance of the Aeronet stations used for the comparison to the nearest dust source region? A greater distance would increase the likelihood of dust transport at elevated layers, which leads to low correlations between AOD and visibility. For example it is well known that at Cape Verde during NH summer the Saharan dust is transported at several km height, undercut at the surface by relatively clean marine air.

13. Page 3024: The authors claim that the reason for the better agreement of the 5km visibility data and the Aeronet AODs may be caused by highly localized events such as dirt roads near meteorological stations. As it is written, the text appears to imply that 1km visibility data are a worse indicator for dust sources compared to the 5km data.

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This statement is not supported by the data presented here. The number of Aeronet stations used for this study is very low, and in particular as e.g. in the case of the Cape Verde station, the distance to the source region is considerable. At those stations the AOD data are no indication for nearby dust sources. Instead, the agreement with the 5 km visibility data is rather an indication that the Aeronet stations do not represent dust source regions, and that both Aeronet AODs and 5km visibility data represent stations influenced by dust that has already been transported over some distance, and the dust clouds are already diluted. Localized conditions like dirt roads may indeed influence some weather stations, but it is unlikely that this is the case for a wide array of globally distributed stations. Note that the visibility threshold of 1km is commonly used to define dust storm conditions (in contrast to e.g. 'blowing dust' conditions). Using 1km visibility data in the type of analysis as done here with the 5km data would be still of high interest, if dust sources and not transported dust are evaluated.

14. Are the TOMS data also compared to the visibility data? TOMS AI and AOD like Aeronet AOD describes column aerosol (with a bias to higher layers), while visibility reduction describes near-surface aerosol. It is to be expected that these disagree for cases of far travelled dust.

15. Page 3025, Paragraph 2: The results presented in Figure 2 (and in particular later in Figure 3c) show that in the model results the dust source emissions are not necessarily well related to the surface extinction, supporting that AOD measurements are insufficient to describe dust sources. Last line: 'add the complexity...' is unclear.

16. Page 3026: The first paragraph is a bit confusing, as it is difficult to see which parameters are correlated with which others. Lines 13+14: '... does as well ...' what does this refer do - does as well in what?

17. Page 3027. Line 14: Are seasonal differences considered? Since in many regions the contribution of dust to AOD or surface extinction varies strongly by season it is conceivable that the results would differ when looking at annual numbers or seasons

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with maximum dust loads.

18. Page 3027, Figure 5: The results showing the maps of visibility reduction ($<$5km) frequencies and surface extinction look quite different from the results showing the dust storm frequencies defined with visibility reduction $<$1km presented by Engestaedter et al. , GRL 2003 (Figure 1a in that publication). The Engelstaedter results do not show the pronounced maximum in dust indicators in the India/Pakistan region and higher values in the Saharan region, compared to the values shown Figure 5 in this publication, although both studies based on a compilation of weather station observations. What could be a possible explanation for this discrepancy?

19. Page 3027, Line 20: '... as some have claimed ...' Please change this wording - as written it implies that there is only very weak evidence that the Bodele is a very strong dust source, but there are in fact many publications supporting this claim. What is not yet clear is the extent to which the Bodele in fact contributes to long-range dust transport, which may be not so important. A statement that the Bodele is a weaker, less frequently active dust source than previously assumed would need to be supported by showing that the meteorological stations in the vicinity of the Bodele depression are really located downwind of the actual dust source location, and close enough to it so that the dust has not yet been mixed up to higher layers, and the surface extinction there represents the columns dust. How is this relationship between dust source and surface extinction at the Bodele-near station locations in the model?

20. Figure 6 (and similar figures)- I suggest to mark the pre-1973 period by dashed lines, as the number of measurements is lower and the results are mostly not used in the analyses.

21. Page 3029, Line 9: Which El Nino/NAO indicators were used? Were the monthly values compared? Also, did you compare the average of (wind speeds cubed) or the (average of wind speeds) cubed? Based on monthly, daily, or sub-daily values? (Best would be to compute wind speed cubed at each available measurement time, and then

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average the values.)

22. Page 3029, bottom: The low correlation between surface extinction and NAO and ENSO show that those climate modes mostly influence transport direction, rather than dust emission. Moulin and Chiapello (2004, GRL) found that the correlation between AOD and NAO was not as significant at a longer time period when compared to the Moulin et al., 1997 publication

23. Page 3031, Line 7: Can the difference to the results by Kurosaki and Mikami (2003) be explained? E.g. were the data filtered differently? This difference is puzzling, as I would have expected that the results should be based on the same station data.

24. Page 3031, line 23: Is ENSO correlated to precipitation in the Australia dust source region directly? This would be easy to test.

25. Page 3032: Can any indication of the dust bowl years be found in the data for the US? In Figure 18 those years do not really show up.

26. Page 3032, Line 14: The fact that in North American dust source regions the surface extinction is not positively correlated with wind speeds my also be an indication that in those locations dust devils (which occur at low wind speeds) may play a major role for dust deflation.

27. Page 3034: The strong correlation of dust indicators with cultivation (Table 4) may be controlled by the India and Pakistan region. How are the results for the rest of the world if this region is left out? Is this relationship typical for only that region? In India and Pakistan the visibility is also reduced by anthropogenic pollution. How are the correlations if only the season with maximum dust is considered? If the spatial correlation of cultivation and visibility reduction were really due to dust emissions, why is this not seen in the temporal trends?

28. Page 3035: As stated above, it remains unclear why a better agreement of Visibility $<$5km with Aeronet AODs compared to Visibility $<$1km is used as argument for

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using the Visibility <5km in the analyses presented here - Aeronet measurements not really represent dust sources, and in particular as the model results show that AODs are not necessarily closely related to emissions. The authors state that Visibility<5km is a better measure of 'dustiness' than Visibility<1km which may be correct (if 'dustiness' is interpreted as dust emission fluxes), but to my understanding the focus of this publications is on dust sources or controls of dust emissions.

29. Page 3036, Lines 12-16: There appears to be a contradiction in the statements - the authors write first that 'surface extinction should be much better related to source surface fluxes Ě than columns amount' and in the next sentence 'Ě it is unclear whether ..optical depth or visibility derived variability gives best information about variability in dust sources'. Please clarify.

30. Page 3036: I agree with the statement that 'more work is needed to better determine the location of dust source regions is vital'!

31. Page 3037, Line 8: Please replace 'shows' with 'appears to show'

32. Page 3037, Last line: A references to Moulin and Chiapello (2004) should be added.

33. Page 3038-3039: When discussing the differences between the surface extinction and TOMS AAI retrievals the differences of what these data can show should be pointed out more clearly. It is not that we should 'believe' one dataset more than the other, rather both dataset record different things and have different drawbacks. While surface extinction may be a better measure of source fluxes than AI or AODs, the number of stations in dust source regions (desert) is still comparatively sparse, therefore the results may not be representative for large scales.

34. Page 3039, Line 7: Only if the relationship between dust emissions and the different land form types is known.

35. Page 3039, Line 14: As already stated above, I do not follow the argument why

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the bad comparability of the visibility data with AODs is a sign that those data are bad descriptors of dust sources, as AODs themselves don't o that job so greatly.

36. Page 3039, last sentence: This appears only to be true for the India/Pakistan region.

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