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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.

N. M. Mahowald et al.

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The reviewers comments were very helpful in improving the text, and were followed, except were noted below.

I. Tegen (Referee)

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

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trends in dustiness should certainly be explored. I recommend the publication of this paper in ACP after addressing the following points for clarification:

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.

Good point-we add to the text.

2. Page 3016, line 13: Holbren=Holben Thanks, fixed.

3. Page 3017, line 3: Reference is missing Oops–added.

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. Good point, added.

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.

Because we are looking at long term trends, we evaluate the monthly means for stations with more than 3 years of data. If we were looking at the ability of the visibility stations to capture specific events, we would evaluate the daily averaged values That is a separate piece of work, which is quite worthwhile, but beyond the scope of this paper. We modify Figure 1 to show both aeronet and the nearby visibility station used for the analysis, as suggested by the reviewer. Notice that the long term visibility stations are

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not required for the comparison to the aeronet data, so the visibility stations shown in Figure 4 and Figure 1 are not always the same. We make this point in the text, since it is possibly confusing.

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. We add references to this work in the discussion of the NAO and precipitation impacts on page 3029, since this makes the most sense to us.

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? We find that rank correlations are not very intuitive to ourselves or most readers, so we prefer to present the material using regular correlations. We include more information on this point in the text.

8. Page 3022, line 8: It is unclear what is meant by 'dust variability' in this context, please replace by a more precise formulation. We include more information on what we mean in the text.

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 cloudfree model days should be evaluated. Yes, this is true! I wish other modelers would look at this same field and see if it were true in their model (hint!). But we include more description of this in the text.

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

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visibility, however. Yes ,we are speculating here. But water may impact visibility more heavily, because most of the moisture is confined to the boundary layer. We try to make this statement more clear.

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. This is one explanation, which we do not agree that this is the main explanation, but it is a very optimistic explanation! It may be that moisture affects are too important (clouds, precipitation). Or that the data itself is not of high quality (because the person making the observations is not careful). It may also be that there is a local source of dust impacting the visibility right at the airport (on a dirt road in front of the observer), but not regionally. We add all these explanations.

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. We add the number of the meteorological station to this table. In this table already is the modeled correlation coefficient, which indicates the inversion seen at Capo. We include a reference to this in the text, to make this more clear. We show the locations of the visibility stations used for the AERONET analysis in Figure 1, and think this is the best way to compare locations, instead of including in the table. For most of the AERONET stations, the visibility station

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is very very close. For a few, it is farther away. We include this information in the text.

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.

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.

The reviewer is assuming that the number of $<$1km visibility events is an interpretable observation of high quality, and is something that should be used in the literature. We do not make this assumption. We think the quality of the visibility data is so possibly so poor that this data should not be used at all. There is no documentation in the literature of the quality assurance of visibility data, comparison of different stations to make sure we are comparing apples to apples, etc. What little we think it can be used for, is examined explicitly in this paper. If we cannot correlate the visibility data to something we think it is true, we assume that the data is garbage. There may be other ways to check the visibility data, but we have not seen in this in the literature, and thus we will be conservative and assume that the part of the data which does not correlate to a

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known, well tested measurement is not something we want to interpret as a measure of dustiness.

In other words, the <1km visibility MAY be representative of SOMETHING. But until we know what it is (and how good it is at measuring this quantity), we think it should not be used as a proxy for dustiness.

Here we examine what data can be correlated with something. The <5km visibility frequency appears to correlate with aerosol optical depth of known quality, so we are willing to use it.

We make these assumptions more explicit in the text, and contrast this with the reviewers approach. We also point out that the vis <1km could be quite important, but we chose not to interpret it without verification of its value.

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. We think the highest quality aerosol optical depth data is the AERONET data, and therefore use it to evaluate the visibility data. We also want to point out that TOMS AAI has many biases, and so do the same comparison for the TOMS AAI and AOD products. There is no reason to compare the TOMS AAI to the visibility data–neither is of particularly good quality as a gauge of aerosol optical depth, as shown here.

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. We rephrase this sentence to try to make it more clear.

16. Page 3026: The first paragraph is a bit confusing, as it is difficult to see which

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parameters are correlated with which others. Lines 13+14: '... does as well ...' what does this refer do - does as well in what? We add in more information about specifically which correlations we are talking about.

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 with maximum dust loads. We looked at monthly means and annual means. They looked fairly similar, so we just show annual means. It is possible that other ways of looking at the data would get different results, so we reiterate what we looked at here in this section (it is also in the methodology section) to be more clear.

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? Excellent point. We add in the Engelstaedter data for this comparison and in figure 5. In doing so, and in discussion with Ina and Sebastian Engelstaedter, we realized that the dust storm data used in Engelstaedter was not number of days with visibility $<$1km, but rather number of days with blowing dust or sand. So we add this also to the text. Note that the Engelstaedter data is then not a visibility-derived dataset, but rather comes out of the atmospheric phenonmenon dataset. We make changes to the text to reflect this change.

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

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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?

These are excellent points. For this model, these stations are downwind, as can be seen in the figure 2b. The last point can be seen by comparing figure 5 and figure 2b–the model assumes a strong Bodele basin source and overpredicts the EXT and Vis 5 (on a relative basis). We add these points to the text. We try to change the text to reflect the reviewers feeling that we are saying there is little support that Bodele basin is a dust source.

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. These plots are pretty small as it is. We think it is easier to read them if we keep the lines as is. Since it is a time plot, it is pretty easy for the reader to look at the data before and after 1973.

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 average the values.) This information is contained in the methodology section, except for the wind speed, which was done as suggested by the reviewer. We add this information to the methodology section. We include this information in this location as well, since the reviewer wanted to see it again at this point.

22. Page 3029, bottom: The low correlation between surface extinction and NAO and

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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 We make this change to the text.

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. They analyze very different stations, including many stations in regions dominated by non-dust aerosols. We note this in the text.

24. Page 3031, line 23: Is ENSO correlated to precipitation in the Australia dust source region directly? This would be easy to test. Dai et al., 1998 did this already, so there is no need to repeat this. They get some signals, but not over the whole region which might be a dust source..

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. Unfortunately, there was not enough data in those regions to analyze for this.

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. We add this explanation.

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? The correlations ARE controlled by India and

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Pakistan, and we make that more clear in the paper. We did not do a seasonal analysis for this paper, and it doesn't seem that it would add to the paper to do so. The big change in cultivation may have occurred prior to 1974, and thus we would not see a change since then in temporal trends. We add comments to this effect in the text.

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 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.

As we stated above, we do not believe the visibility data represents anything interpretable until it has been shown in a paper of sufficient quality. Here we show that the <5km data has some correlation with aerosol optical depth, so we are willing to interpret that part of the visibility data. We are not willing to interpret the <1km data, since we have no idea what that data represents.

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 ˇE than columns amount' and in the next sentence 'ˇE it is unclear whether ..optical depth or visibility derived variability gives best information about variability in dust sources'. Please clarify.

This is because the visibility data is not of particularly good quality, as compared against the AERONET data, which is the only comparison we have seen done in the literature. If the visibility data were of as good of quality as the TOMS data, we should use it. We clarify this in the text.

30. Page 3036: I agree with the statement that 'more work is needed to better deter-

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mine the location of dust source regions is vital'! Yes!

31. Page 3037, Line 8: Please replace 'shows' with 'appears to show' Here we are saying what the data shows, not that there is a change in dustiness. We think VIS5 or EXT show is ok. (If we interpret that as dustiness then we should stick with appears to show').

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

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. Yes, good point.

34. Page 3039, Line 7: Only if the relationship between dust emissions and the different land form types is known. Yes, but it is an approach to go down–Ballentyne has a paper starting to look at this.

35. Page 3039, Line 14: As already stated above, I do not follow the argument why 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. Without quality assurance and comparisons to other data, we simply do not believe the visibility data is of sufficient quality to interpret, as stated previously.

36. Page 3039, last sentence: This appears only to be true for the India/Pakistan region. We put in a less strong statement here (India and Pakistan are in the previous sentence) and add a sentence talking about the problems with the visibility data interpretation.

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