**Atmospheric
Chemistry
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Discussions**

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

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Response to reviewers:

The reviewers make excellent suggestions and comments that substantially improve the text. For the most part we modify the text to better reflect their comments. There are a few exceptions, where we do not do as suggested, in which case we explain why we did not.

Comments by Reviewer # 1 (Ron Miller)

"There seem to be four main conclusions. First, surface extinction, where light attenuation is measured within air at the surface, is a better indicator of local aerosol sources than column optical thickness, where attenuation is measured vertically across the entire column. This is because column extinction includes aerosols high overhead that

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may be transported from faraway sources. Visibility, which measures light attenuation along a horizontal path is related to surface extinction, because attenuation occurs within air at the surface. The article would benefit by making these points more explicit and prominent. I couldn't find a definition of surface extinction in the article. As a reader with limited experience in radiative transfer, I could only infer through context the distinction with optical thickness, so that one of the interesting points of the article was initially lost on me."

Good point. We make these goals and conclusions from the paper more clear in the introduction and define surface extinction more clearly in the text.

"The second conclusion is that temporal variations of visibility can be related to rainfall over Africa and surface wind speed over East Asia, but otherwise exhibit little correlation with other variables such as cultivation and grazing or climate indices related to El Nino and the North Atlantic Oscillation. I have some technical comments below, but these conclusions are consistent with previous studies (e.g. Prospero and Lamb 2003 for the Sahel; Sun et al JGR 2001 for East Asia), and seem robust. Thirdly, the authors conclude that spatial variations of visibility are better correlated to cultivation than other indicators of dust sources, such as topographic lows. They then conclude that either are 'equally good at inferring...dust surface fluxes' (p.22). I have reservations about this comparison. As the authors note (p.2), cultivation is often collocated with natural sources of dust, so that using spatial correlations to distinguish the influence of each upon variations in visibility is ambiguous. In addition, the network of visibility measurements, while extensive, does not include large parts of the Sahara that act as dust sources. (see Figure 5: I believe the gray shading indicates something related to the TOMS AAI, which is intended as a proxy for natural sources, although this is not stated in the caption nor could I find the words 'gray' or 'shading' in the text.) As the authors note, surface extinction and visibility are attractive measurements because they are more sensitive than column extinction to local aerosol sources. However, their corresponding drawback is that sources far from the observing network will be difficult

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to detect. Visibility measurements are taken in locations of human settlement, which are generally supported by nearby agriculture. Thus, the visibility measurements are positioned to be more sensitive to cultivated sources of dust, and will exaggerate their importance if natural sources are comparatively remote. That the usual metrics of natural sources (e.g. topographic lows) correlate with TOMS AAI variability (Prospero et al. 2002) but show no significant correlation with visibility raises the question whether the density of the visibility network is sufficient."

These problems about the collocation of humans and visibility are good points. We add these disclaimers to the text. We don't actually believe you should believe the visibility that much.

Of course, realize that TOMS AAI is biased to see sources of dust where the PBLH is high (e.g. topographic lows in the middle of deserts–Mahowald and Dufresne, 2004), so it too has problems. The analysis in the paper suggests that even if TOMS AAI wasn't biased towards thinking that places with high PBLH (topographic lows) were sources, AOD in general tells us little about dust sources. So we don't believe we should disregard all data which disagrees with a biased dataset. We just think we should look at this more closely.

"Another issue is the influence of other aerosol species on the visibility. While the correlation is limited to sites where dust contributes at least half of the annual average surface extinction, this allows a substantial contribution from other aerosol species. Anthropogenic sulfates originating from power plants or black carbon from inefficient combustion (e.g.), will reduce visibility and correlate with nearby cultivation. While I wouldn't rule out an important contribution to dust from cultivated sources, the visibility measurements presented here seem insufficient to contradict the 'hypothesis that dry lake beds are dust sources' (p.22)."

It is fine to argue that there are biases in the data–we maintain that throughout the text that one should be very skeptical when using the visibility. We do not argue in the

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text that the dry lake bed theory is wrong we argue: "Thus, this paper suggests we need to re-examine the hypothesis that topographic lows are the dominant source of dust. Using datasets that represent the vegetation, land use, and underlying soils and landforms would provide a more physical basis from which to understand dust sources" which is not a very strong statement at all.

" I also don't see any quantitative criteria that can be used to argue that the TOMS AAI and visibility network are 'equally good at inferring...dust fluxes' (p.22). "

We do not in the text ever argue that they are equally good. We argue that they 'may be' equally good. We think that the analysis indicating that AOD correlates spatially or temporally with surface fluxes at only a moderate level (and the fact that TOMS AAI will do worse than AOD at providing information close to the ground) shows that TOMS AAI is probably not very good at getting surface fluxes. Visibility may or may not be better–it has many of its own problems.

"It seems more defensible to argue that visibility measurements indicate the importance of local sources in the vicinity of cultivated areas. Since the significance of cultivated sources hasn't been established definitively in the literature, it would be useful if it could be shown that visibility is reduced in regions far from natural dust sources and where other aerosol species are negligible."

As the reviewer argues, the fact that there is a meteorological station means that there are humans nearby, so we are not sure how to do as the reviewer suggests. We suggest an alternative approaach. The comparison of the AERONET column amounts to the nearby meteorological stations was meant to address the point of whether the visibility data could be used as a regional indicator of dustiness. We argue that to the extent that is true, the correlations between AERONET and dustiness are strong. As they are only at a 0.4-0.5 level, that is the portion of the visibility that we would argue is regional, not local. We expand the text on this point to make this more clear.

"Finally, the authors conclude that while spatial variability of dust sources is related to

cultivation, temporal variations in the last few decades are related to climate variables like rain in Africa and wind speed in East Asia. Given large changes in cultivation over this period related to the near-doubling of world population, it surprises me that cultivation fails to leave an imprint on temporal trends if it is such an apparent source of spatial variations in dust. One problem may be the low temporal resolution of each land use time series, which might obscure its correlation with higher resolution data sets. That the expansion of cultivated areas to match the growing population has little influence on visibility seems to highlight the uncertainty in both data sets."

We agree completely, and try to highlight this point.

"In summary, I think there is too much uncertainty to assess the relative importance of natural and cultivated sources. I would recommend that the authors give more emphasis to using to visibility observations to show the existence of cultivated sources. The article represents a substantial analysis of the visibility measurements, a potentially important source of information about dust that has received little attention, with the exception of Engelstaedter et al 2002 and various Chinese authors. The authors deserve credit for trying to find a common picture among such a heterogeneous set of data.

We agree that there is too much uncertainty to assess the relative importance of natural and cultivation sources, and reiterate this point as the last sentence. The paper is quite full as it is, and the data is questionable, so we would argue that we should not add more text to the paper, looking more at cultivation sources, but rather leave that for a different paper. The purpose of this paper was to summarize a lot of new data.

***** Specific Comments ***** "p.1 (abstract): After the second sentence promising 'to assess the anthropogenic impact on long term trends in desert dust emissions', insert a sentence briefly describing how you are going to do this."

We make this change.

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p.1 (abstract): replace 'Ÿ0.47' with '0.47'? Done . p.3 replace 'one long time series dataset' with 'one long time series'. done

p.3 delete 'as well as the potential for other problems' or list specific problems? Done

p.3 'we try to derive a representative proxy from a global data set *to correlate with visibility observations*'? Yes, add explanation p.3 'Very little is know*n*' done

p.4 (citation)? Oops. Got citation in there.

p.5 In order to evaluate the visibility measurement...' Given that you ultimately conclude that visibility is better suited than column extinction to measure aerosol surface concentration, it seems paradoxical to use AERONET AOD to reassure yourself that visibility measurements are sensitive to aerosol variations. A poor correlation might simply result from the presence of lots of far-traveled aerosols above the boundary layer, and not the insensitivity of visibility to surface aerosols. No, we conclude that visibility is better suited than column extinction to measure aerosol surface FLUXES. But you are right, there is the possibility that we are missing this. We mention this more explicitly in the text. Except that in the model they are pretty well correlated, and frankly, what choice do we have??

p.5 citation to Mbourou et al 1997: Sharon Nicholson (a coauhtor on this paper) cites it as N'Tchayi Mbourou et al 1997. Done.

p.5 'surface extinction value through Koschmeider's formula.' Please define surface extinction. If I'm not mistaken, it represents light extinction within air at the surface and thus provides a measure of surface concentration of absorbers, including aerosols, water vapor and clouds. Please also relate surface extinction to measurements of visibility. We add a definition of surface extinction in the intro and here in the methods. This formula shows the relationship between the measured visibility and the surface extinction. We think the new text should make this point more clear.

You might also note that surface extinction is less prone than column optical thickness

to contamination by aerosols passing overhead from remote sources. Readers like me, who aren't radiatively adept, might otherwise miss the significance of your first major conclusion.

Done.

p.5 '...here we compare them (TOMS) against the AERONET optical depths.' What is the goal for doing this? Note that AERONET AOT measures extinction by all aerosols including sulfates, whereas the TOMS AAI is sensitive mainly to absorbing aerosols, so the two AOT may differ even if they have the same sensitivity to dust.

Because ultimately we have to evaluate TOMS results against visibility results, and we have to realize that TOMS has a lot of biases, one of which is that reflective aerosols so a negative signal, but there are many others. If we are looking in dusty regions, we are finding out whether there is a good correlations between them in dusty regions.

p.6 '(PDSI) incorporates antecedent precipitation...' Please provide a citation so that the reader can find the exact formula if necessary.

The citation is given the first time PDSI is used, but we add it again in this paragraph. p.7 'rank correlations, for which we know the distribution...' I thought rank correlations were attractive because you didn't have to know the distribution. You don't have to know the distribution of the original dataset, but you know the distribution of the ranked dataset. We clarify in the text.

p.7 'we arbitrarily choose >25%' Shouldn't this depend upon the number of independent observations and the number of variables? Perhaps, but this probably doesn't matter, since it occurs so rarely. Most of the time the variable is easily replaceable.

p.7 'replaceable' If explained variance is reduced by more than 25% when a variable is omitted, shouldn't it be 'irreplaceable'? Yes, we correct this the text..

p.7 MATCH: what time period corresponds to the model simulations? Added to the text.

p.7: Given that MATCH/DEAD is used to identify dusty regions, describe how sources of dust and other aerosol species are prescribed in the model. response: We add more text to the model description, talking about the assumptions for the dust and other aerosol sources.

p.8 '...elucidates the theoretical relationship between dust sources and extinction' Is there a theoretical relationship? Alternatively, I thought the value of the model was that you could relate sources and extinction in the absence of data gaps and contimination by other aerosol species. In the model there is a relationship between the dust sources and concentrations and column amounts. In the real world we have no way of obtaining this information, since we have no way to deducing the dust source fluxes directly. We try to clarify this point.

p.8 correlation of AOD to surface extinction (calculated from visibility): did you remove the seasonal cycle from each time series prior to computing the correlation? No, we keep it in to try see if the surface extinction can even capture the seasonal cycle. Removing the seasonal cycle makes the correlation go down. We add this to the text.

p.8 'we correlate the values collectively?' Did you correlate the station-averaged variables or did you average the individual correlations at each station? We correlate across all the values, no matter what station or time period they come from.

p.9 Maryland Science Center vs GSFC: AOD at both these locations is predominately influenced by urban pollution such as sulfates and carbonaceous aerosols with sources broadly distributed across the eastern seaboard. Thus, it probably isn't a uniformly good indicator of the effect of small-scale aerosol variations (e.g. urban pollution within largely rural areas). Urban pollution is normally highly hetereogeneous with different regions having quite a bit of variability, so it would seem to be a fine example. We have no other choice.

p.9 'Note that if we perform the correlation over all the *AERONET* stations...'? clarified.

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p.10 'best correlations...3-7 km' Are the correlations at 2 and 9 km significantly different to warrant exclusion from this range? No, you are right, we include them now.

p.10 correlation of TOMS and AERONET: again, is the mean seasonal cycle included in this correlation? Yes. This is indicated by saying they are monthly means, not monthly mean anomalies.

p.10-11 linearly proportional to concentration of *aerosols, water vapor, and cloud water*'? This point is clarified.

p.11 replace 'Thus, we are using...' with 'Nonetheless, we are using...'? ok.

p.11 'For this section, the results are...' Could you be specific? We add text to be more specific.

p.11 'modelled maximum in dust AOD or 'surface concentration...' Figure 2 is labelled with 'surface extinction' rather than 'surface concentration'. See also 'The surface concentrations appear...' They are linearly proportional, but for clarity we keep consistent nomenclature.

p.12 '(even at a limited number of stations)' I suggest deleting this phrase because it is qualitative and the issue is discussed in the next paragraph. Deleted.

p.12 replace 'much better job' with 'better job' unless you can quantitatively defend 'much'? done.

p.12 'Our analysis suggests that...' This seems like a comparison of apples and oranges. Your benchmark for quality is agreement with AERONET AOD. Yet, TOMS and visibility are expected to disagree with AERONET for different reasons: TOMS should disagree because it is more sensitive to absorbing than reflecting aerosols, and the visibility measurements should disagree because they measure horizontal extinction rather than the column extinction related to AOD. This is an excellent point. We include some discussion of this in the text.

Section 4: A lot of discussion is devoted to large fluctuations in spatial averages early in each record when there are significantly fewer observations and thus greater uncertainty in the spatial averages. I suggest adding error bars to each time series in Figure 6 (and similar figures) so that the reader can decide which fluctuations are robust. One point to note is that not all observations used in the spatial average are independent due to geographic proximity, so that the number of independent observing sites used to estimate the confidence interval will be less than the total. Good points. Unfortunately, we couldn't get the error bars in without making the plots too messy–they are very busy already. The criteria of a radical change in the number of obsevations, shown in the bottom panel, seems like a valid criteria for excluded certain time periods, so we make this point more clear. We also point out that there is very little data and these large fluctuations are not statistically significant.

Figure 5: I probably overlooked it, but do you explain the gray contours? I'm guessing that it is related to TOMS AI, but this is not explained in the caption. Good point. We expand this explanation in the figure caption, but is the Ginoux basin factor.

p.13 'The Bodele basin does not appear to be the largest source of dust...' I have several reservations about this statement: i) other aerosol species are allowed to contribute up to 50% of the surface extinction so that there is an imperfect (and spatially variable) relation between dust and visibility, and ii) you only have one station in the vicinity of the Bodele depression according to Figure 5. We add some discussion to this point, in the text, pointing out that in our model anyway, the Bodele basin has a downwind influence which is large enough to be felt at several stations, as suggested by the other reviewer.

p.13 'associated with Sahel drought...' I don't see large decadal variations in precipitation in Figure 6 that are associated with the Sahel drought (e.g. Prospero and Lamb Sci 2003), and I wonder whether you might find a stronger relation with visibility limiting the spatial average of precipitation to a subset of the African continent showing a high correlation between visibility and precipitation? Sharon Nicholson has noted that the

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decrease in Sahel rainfall is often associated with an increase in rainfall to the south along the Guinea coast, and this regional compensation may reduce the apparent variability in rainfall over the dust source regions (Nicholson Rev Geophy 2000). There is a definite downward trend in the plot, so we add a horizontal line to make it more clear.

p.14 'We focus next on correlations...' Are these correlations computed from annual averages? If monthly averages are used, is the seasonal cycle included in each time series? It seems more appropriate to compute the correlation with monthly averages after subtracting the seasonal cycle if you are interested in relations over interannual and decadal time scales. Otherwise, a high correlation may simply indicate that the seasonal cycle of the two variables is in phase, and not that there is a longer term relation. The correlations are annual–we repeat this in the text to make more clear.

p.14 'and boxes indicate that the correlations only exist between that variable and the visibility derived variable.' I don't understand this. This is described in the methodology. We point the reader back to the methods to explain the methodology..

p.15 'positive correlation between EXT and cropland...' The cropland and grazing time series have only about 10 or so independent values, given the low temporal resolution of the data sets. Thus, it is much easier to get a high correlation by chance compared to a data set like visibility where values from successive years are probably independent. Was this reduction in the number of degrees of freedom accounted for when computing the significance of correlations with cropland and grazing? Yes.

p.15 'China...in the 1940's' According to Figure 13, there is no visibility data for this region in the 1940's. Whoops. We change this to the 1950s.

p.16 'This makes some sense...' Is it possible that the positive correlation is because higher humidity during wet years increases haze and reduces visibility? Yes, this is an alternative explanation that would mean we shouldn't interpret this result as real from dust. So we clarify this to say that the explanation that would make sense from a dust perspective isĚ., and include this explanation as an alternative. P.18: When

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computing statistical significance of correlations, did you account for the possible dependence of certain stations? If the stations are not all independent, then the threshold for significant correlation will increase. Yes, good point, we will include this.

p.18 'These results suggest that cultivation is the best determinant of spatial variability of dustiness, ...not whether there are topographic lows nearby.' That the correlation between visibility and cultivation decreases when the cultivation resolution is increased (so that the visibility stations and cultivation are no longer precisely colocated) suggests that the visibility data are strongly influenced by local sources, which will favor cultivation compared to natural sources. Good point. We include this disclaimer again here.

p.22 'The hypothesis that dry lake beds are dust sources is not supported...' I'm skeptical of this conclusion, because the visibility measurements seem disproportionately sensitive to local sources of aerosols. Yes, but the part that correlates with the AERONET data should be 'regional' aerosol.

Figure 1: the AERONET locations are indistinct: use a higher contrast color like white and a thicker line? Good point.

Figure 6 (and similar figures): the scale for cultivation should be reduced so that temporal changes are more apparent? Good point.

Figures 7, 11, 13, 15, 17, 19 and 21 are missing minus signs on the color bar. Thanks.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3013, 2007.

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