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Interactive comment on “What controls the recent changes in African mineral dust aerosol across the Atlantic?” by D. A. Ridley et al.

D. A. Ridley et al.

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We thank the reviewers for their time and useful comments and suggestions. We have made efforts to improve the manuscript accordingly, please find comments to individual points below.

Reviewer #1

Major Concerns: 1. Organization and Paper Objective This work would benefit from a statement of objective in the introduction. There are some logical disconnects between the title and the content of the article. No objective statement or hypothesis exists in the introduction for clarification. “Attribution of African dust trends” implies the work focuses on the reasons behind observed reductions in mineral dust transported off of

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Africa. In addition to attribution the paper also includes a discussion of the accompanying changes in radiative forcing and a proposed mechanism by which surface winds may have changed in the 20th century. The proposed mechanism of surface winds alteration is a neat idea, related to their key finding that near surface winds are more important than surface conditions for explaining the trend in dust. Their radiative forcing section, although very interesting, is outside of the scope of the paper based on the title and the topic of surrounding sections. I suggest the authors remove this section from the results section, reduce the length, and move it to the discussion section. (Alternatively, broaden the scope of the title and objectives in the introduction). The conclusions section should be revised, and re-focused about the paper's objective. For example, the current layout leads with a discussion of the radiative forcing which is a tertiary finding of this work. This manuscript contains a lot of great results, but could be strengthened with some re-organization.

After consideration we agree with the reviewer's insight here and have made substantial changes to the manuscript organization based upon the suggestions. We have:

-Edited the final paragraph in the introduction to better reflect the flow and objective of the paper.

-Moved the sections discussing the evaluation of the model dust scheme against observations into supplementary materials for those interested in the specifics.

-Reduced the discussion of DRE and included the discussion of the magnitude of the DRE in the Model Description and Evaluation section and the discussion of the trends in DRE in the section on AOD trends and variability to complement this analysis and to prevent the break in flow of main theme of the paper.

-Reframed the conclusions around the main aim of the paper rather than the auxiliary radiative effect results.

2. Statistics The authors argue, based on Figure 8 and Figure 9 that the role of sur-

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face vegetation is minimal and that 10 m or near surface winds are key drivers of the observed reduction in mineral dust load over the Atlantic. Later the authors present convincing evidence of this in the form of Figures 10 and 11; that in general the reduction of near surface winds coincide with regions of reduced dustload and that the same cannot be said for vegetation, which is out of phase with regions of reduced dustload. The abstract reflects this, with a strongly worded statement about vegetation playing little role in decadal dust reductions in their model runs. While the sum of the evidence supports their conclusion, I feel that the authors over-state the results of Figure 8 and 9 in Section 4.3 of the text. From the figures alone, statements such as “We have shown that changes in vegetation are unlikely to directly influence dust emission via changes in source regions” are not supported. For example, it is not clear to me that trends in Figure 9 in the North Atlantic, South Atlantic or Caribbean that the DAOD trend labeled ‘10 m winds’ and ‘vegetation’ are statistically different from each other, or from the baseline run itself. Accompanying statistical tests demonstrating differences between vegetation and 10m winds; and moreover between each component at the model baseline are necessary to support such statements at this point in the manuscript. Perhaps a table showing significance would be helpful to prove the argument?

It may not have been clear that statistical significance of the trends had been assessed. Therefore, the following statement has been added into the discussion of the attribution of the trends: "In all locations except North America the trend with no interannual variability in 10-m surface winds is significantly different to the baseline run (>95% confidence), whereas the trend with no interannual variability in vegetation is indistinguishable from the baseline." We believe that this explicit statement, combined with the uncertainty of each of the trends included on Figure 6 (formerly Fig. 9), reinforces the conclusion that vegetation cover appears to have very little impact upon the trends and variability in DAOD.

3. Variance vs. Mean-State In section 3.4 the manuscript would benefit from a more careful separation of treatment of and comparison between variability and mean state.

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The authors argue that if say precipitation or vegetation is does not contribute much compared to total variability; that it is not important for dust emission (e.g., “Removing the inter-annual variability of vegetation has a negligible impact on the variability in DAOD suggesting that the changes in dust source region resulting from vegetation cover changes are unimportant.”). It is possible that vegetation cover changes may not contribute significantly to year to year variability, but may be important to decade to decade variability – especially since vegetation changes on significantly longer timescales than precipitation or wind. In this way changing the above sentence to “Removing the inter-annual variability of vegetation has a negligible impact on the variability in DAOD suggesting that the changes resulting from vegetation cover changes are unimportant for inter-annual or intra-annual variability in dust.” It would be fairer to make a strong statement (as is in the current version of the manuscript) after examining both variability and mean state (long term trend), rather than just the variability alone.

The effect of vegetation changes on the interannual variability in DAOD is found to be negligible (absence of vegetation influence in the pie charts in Fig 5, formerly Fig 7) and the effect of vegetation on the long term trend in DAOD is also found to be weak during this period (based upon the statistically insignificant differences between the baseline run and the model run with no interannual variability in vegetation – green and grey lines on Fig 6, formerly Fig 9). We agree with the reviewer that vegetation changes on longer timescales may have a significant impact on emissions and so have included a caveat that the vegetation changes are only negligible for this time period. When the ‘interannual variability’ has been removed by using only one year of vegetation cover repeatedly this removes both the interannual variability and any continuous trend. This fact is now stated explicitly in the text (pg13 ln 282), and we have made clarifying statements in this section to ensure that the justification for this conclusion is now clearer.

Minor Concerns and Comments: 4. NAO: Some discussion of the NAO is found scat-

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tered about Section 4. I would recommend moving all NAO related conversation to the discussion; to frame your work within the context of the literature. The work here focuses on the direct mechanisms that result in mineral dust emission, not climate proxies such as the NAO. I think in the discussion section you can relate your findings to previous work on the NAO, but it is not necessary to devote as much space as you do presently. Furthermore I think when you discuss Figures 8 and 9 (Section 3.4) your arguments are broken up and weakened through the asides relating indirectly to the NAO. The authors state the NAO has a weak correlation with dust in the most recent decades and recent publications (Riemer 2006, Doherty 2008, Nakamae and Shiotani, 2013) all show that the NAO is of secondary importance to other climate proxies, and certainly the more direct mechanisms you present here.

We agree with the reviewer that this potentially detracts from the discussion within the results. These references to the NAO have been removed. We have kept the discussion in the introduction as this clarifies why we have chosen to frame the research around physical parameters rather than the NAO climate index.

5. Page 4, line 1: Chin 2013 reference is missing from bibliography.

This is included

6. Page 10, lines 15+: “Biomass burning aerosol below approximately 12_ N during the winter and sea salt aerosol in coastal regions may both influence the agreement with MODIS and AERONET, but we expect these effects to be small relative to the dust aerosol that accounts for over 70% of the annual AOD between 10_ N and 36_ N in the model.” This is very close to what Formenti 2008 found in observation, they found 72% of aerosols mass in aged plumes containing both dust and biomass burning, was dust particles.

We have included the Formenti et al. reference to show agreement with the model dust fraction.

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7. Figure 4 might be improved by applying your color scheme (blue for winter, and red for summer) to the markers as well.

Figure 4 is now included in supplementary material (Fig S3) and has been altered to show markers in color and the 2:1 region altered to improve contrast.

8. Page 11, lines 1 – 4: “While the total improvement relative to the observations is small, the new dust emission scheme is considered to be more realistic as it represents both sub-grid winds and the modulation of dust emissions from vegetation changes.” If the changes to your model do not result in statistically significant improvements with respect to observations (not discussed), I am not sure if it’s fair to say that the new dust emission scheme is more realistic.

Altered to simply say we should be capturing more of the processes with the new scheme (supplementary material pg4 line 5)

9. Page 11, line 7: This is an interesting section. “In the Sahel, there is a tendency for the model to overestimate the AOD during high aerosol loading (predominantly in winter).” Next you argue that in summer, it’s underestimated because of local convection driven winds not in MERRA. It’s possible that your assumption is correct. Could it also be possible that the applied distribution of winds under-represents gustiness and in turn the emission model is then tuned upwards so that low-frequency synoptic flow contributes too much dust emission (like what is seen in winter)?

Yes, this is also a valid interpretation. We have included this as well as the underrepresentation of Haboobs with the following statement: “...or from poor representation of wind gustiness and therefore a bias towards emission from synoptic air flow in the wintertime” (Supp. mat. In12 pg 4)

10. Figure 6. Two suggestions. First, I would recommend sticking with DAOD in the figure caption to be consistent with the text. Second, I would recommend going with red instead of grey as the color for the model, because the grey is harder to see. Later

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figures refer to this color scheme – however Figure 6 remains difficult to discriminate between black and grey.

The model caption has been altered to match the y-axis and the model changes to red (and colors in Fig 6 – previously Fig 9 – altered to avoid confusion

11. Page 13, line 30: The phrase “a period responsible for significant transport of dust to South America” – is repeated in back to back sentences.

This paragraph has been restructured

12. Page 14, line 6: “Figure 7 shows the anomaly in monthly dust concentration measured at Barbados alongside the modeled surface concentration anomaly.” The caption to Figure 7 does not refer to an anomaly, rather the model concentration.

The y-axis and caption have been altered to refer to concentration anomalies.

13. Page 15, line 13-15: “We find that precipitation primarily affects the variability in dust loading over the Atlantic via wet scavenging rather than by increasing soil wetness and suppressing emission.” Is this from work not shown, or is this taken from the increasing importance of precipitation as distance from source increases? I suggest the authors clarify the rationale for their conclusion.

We have now explicitly stated that this is based upon the model, i.e. the deposition and emission diagnostics.

14. Page 15, lines 16-18: “Removing the inter-annual variability of vegetation has a negligible impact on the variability in DAOD suggesting that the changes in dust source region resulting from vegetation cover changes are unimportant.” Perhaps this is true, that vegetative state is not important to inter-annual variability. However this statement is dis-proven in Figure 9, where it is shown that variability in vegetative cover are related to changes in dust load in all regions except perhaps Coastal Africa. (Please see major concern #2).

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We believe this has been addressed as part of Major Concern #2

15. Page 16: Please see the major revision section of this review; but the discussion here would be greatly augmented by inclusion of comparative statistics. It is not clear to me that in Figure 9 in the North Atlantic, South Atlantic or Caribbean that the DAOD trend labeled '10 m winds' and 'vegetation' are statistically different from each other, or from the baseline run itself. As such I don't think that the work presented justifies the statement "We have shown that changes in vegetation are unlikely to directly influence dust emission via changes in source regions" or the more general statements in the abstract.

Statistics are now mentioned in the text. Please see response to Major Concern #3.

16. Page 17: An examination of radiative effects of the observe trend in mineral dust is a natural progression, and is of interest to the readers. Based on the objectives of the paper, or the title, would Section 4.5 be better served as a shorter discussion section? Furthermore in terms of the radiative uncertainties associated with the refractive index and model size distributions, perhaps this is outside the scope of this article? At the least I would suggest reducing the length of this section as much as possible to keep the focus of the work on attribution of trends.

Thank you for this observation. We agree that this section is not required in such detail for the paper and have reduced and incorporated into the results on trends and variability (please see responses to Major Concern #1).

17. Page 21: There is no discussion section, although the authors do a good job interspersing comparisons to previous work throughout their results section. I would recommend collecting the various NAO discussions into a single section, which would probably allow for less text to be spent on the subject.

We have reduced the discussion of NAO to the single paragraph in the introduction and the comment on the correlation during the results (Sect. 4.2).

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18. Page 22: Conclusion points unrelated to the title. (Please see major concern #1). Again here its not clear what the focus of this work is, I can't tell what the major take home results of this work are. Improvements to GEOS-CHEM? Vegetation is not important to inter-annual dust variability? Radiative changes as results of decadal trends?

We have now restructured the conclusions based on this reviewers criticisms. We now focus on the magnitude and attribution of trends and variability as suggested by the title (with a brief mention of the radiative impact for context), the lack of evidence that vegetation plays a significant role in these, and conclude by summarizing the hypothesis that anthropogenic aerosol indirect effect may be influencing the trends in dustiness observed.

Reviewer #2

1. p 3585 l. 15-17 positively or negatively correlated?

Altered to include 'negatively'

2. p. 3596 Could you describe in more detail the wind aspect of the experiment? The lowermost winds have two functions: they define the surface wind stress used to drive saltation and dust emission, but also play a minor role in the transport of emitted dust. Did you hold fixed both aspects of the surface wind, or only the surface wind stress component?

We have clarified this by adding: "this only affects the 10-m winds used for calculation of dust emission flux, not for other processes in the model" (pg 14, ln 285)

3. The mechanism outlined in section 5 would in principle seem to be a feedback, in that decreased aerosol loading leads to reduced wind stresses and therefore further reduced aerosol loading. Would the opposite also occur, whereby increased aerosol leads to increased dust generation, or is the aerosol type (sulfate vs dust) and/or geographic region sufficiently different that this would not be likely to occur? Are there

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simple scaling arguments for how strong such a feedback might be, if present?

At this stage it is not possible to determine whether simple scaling is applicable to this feedback, the relationship would require testing in a climate model. Furthermore, the nature of the relationship via the non-linear aerosol indirect effect upon clouds, the related change in inter-hemispheric energy balance, and the impact on winds and hence dust emission, we believe it is unlikely that a simple scaling will be possible.

4. Why do you assume that the AIE is required for the aerosol-induced stilling; shouldn't the direct aerosol radiative effect also lead to changes (albeit weaker) in the response?

The requirement of the aerosol indirect effect comes from Booth et al. (2012) where they find that aerosol-cloud microphysics accounts for 80% of the aerosol forcing in the North Atlantic.

5. If so, why consider the models with and without AIE as a categorical distinction rather than one of overall strength of the total aerosol direct + indirect effect?

Please see answer related to Booth et al. (2012) above.

6. The two main ideas explored in the paper (revising the dust source function vs. understanding the decline in dust emissions) could be tied better together in the manuscript. Is there a mechanistic link, for example is the first part required to get reasonable results in the second?

The change in dust source function gives only a small improvement over the original dust scheme when compared with observations and simply allows the model to consider changes in dust emission related to vegetation. Following the reviewers' comments we have decided to move the modifications and evaluation of the model dust scheme to supplementary material so that this section does not detract from the main findings of the paper.

7. Fig. 9: I can't actually see the difference between grey and green lines here for most of the panels, are they completely overlapping? If so, would be useful to point that out

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in the caption. Also, I find it interesting that the far S. America, the transport does play an important role. Does this imply that changes to the interhemispheric transport are occurring?

Yes, the lines are overlapping for the most part. The baseline model is now shown in red and the fact that they overlap is included in the caption. The change in the South American DAOD trend resulting from transport and precipitation are not quite significant at the 95% confidence level so they are not discussed in the text. However, it does appear that an increase in wet removal in the outflow during winter and a slowing of trans-Atlantic winds are responsible for reducing the interhemispheric transport of dust.

Comments from M. Mishchenko

1. The downward AOT trend discussed in this manuscript was first identified using AVHRR retrievals in the paper Mishchenko, M. I., and I. V. Geogdzhayev, 2007: Satellite remote sensing reveals regional tropospheric aerosol trends, *Opt. Express* 15, 7423-7438.

This reference is now included along with Zhao et al. (pg 11, ln 240)

2. In that OE paper, we also noticed a significant decrease of the regional Angstrom exponent, meaning that the dust particles became larger. Although the Angstrom exponent trend is less reliable given the poor quality of the AVHRR data, it would be interesting to discuss it in the context of other findings in this manuscript.

This is an interesting point. However, we have chosen to reduce the section on the radiative effect based on comments from the reviewers and now broadly acknowledge the uncertainties involved in determining the DRE and AOT retrievals for dust aerosols.

3. AOT retrievals for dust aerosols are affected strongly by dust-particle nonsphericity: Mishchenko, M. I., I. V. Geogdzhayev, L. Liu, J. A. Ogren, A. A. Lacis, W. B. Rossow, J. W. Hovenier, H. Volten, and O. Munoz, 2003: Aerosol retrievals from AVHRR ra-

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diances: effects of particle nonsphericity and absorption and an updated long-term global climatology of aerosol properties, J. Quant. Spectrosc. Radiat. Transfer 79/80, 953-972. AOT errors can exceed a factor of 3 if nonsphericity is not accounted for. However, the AOT trend can still be rather accurate if the observation geometries remain stable on average over the period of observations. The authors should discuss this important issue.

Again, based on the reviewers' comments this section has been reduced so an extended discussion is not possible; however, the fact that shape is a cause of uncertainty is now mentioned explicitly rather than just 'size' and a reference to Mischenko et al. (2003) has been added so readers can explore this important issue further if they wish.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3583, 2014.

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