Atmos. Chem. Phys. Discuss., 14, C300–C306, 2014
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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

Anonymous Referee #1

Received and published: 5 March 2014

Overview: The authors implement changes to the GOES-CHEM dust emission scheme and show some slight improvement in model performance. Next, the authors demonstrate a large decline in dust transport over the North Atlantic, and attempt to understand if changes in vegetation, surface winds, precipitation or large-scale atmospheric flow is responsible for the decline. Results suggest near surface winds are critical and that vegetation plays a small role in the year-to-year variability of mineral dust. The authors provide some evidence that vegetation is not important for the long-term decline of mineral dust in their model, and back this up with subsequent reanalysis products supporting their claim. The authors then discuss the radiative forcing changes resulting from decreasing dust load; and conclude with a potential explanation for why near surface winds have declined, in turn driving the decline in mineral dust emission. The large number of topics covered by the authors represents a weakness of the paper, to a certain extent the motivation and focus of this paper is not clear amongst the myriad results included in this manuscript. This is reflected in the conclusion section of the manuscript, which seem to focus on secondary and tertiary results, rather than the topic as suggested by the title – or even the order of which the results are presented.

This work is comprehensive in terms of observations, with model results compared to surface based measurements (both AERONET and station data) and satellite observations. The model framework the authors use to test the four components, which may contribute to the decline of dustload, is novel, and appropriate for the problem at hand. The subsequent analysis (in particular Figures 10 and 13) support the author's key findings, and moreover are suggestive of interesting future work that can emerge from this study.

Given the importance of dust to regional and global climate, and the general inability of GCMs to recreate dust emissions, dust load, inter-annual and inter-seasonal variability this work represents and important contribution to the field, with useful insight about dust emissions in a modeling framework. The topic and scope of the article make it appropriate for publication in ACP.

Suggestion to editor: Accepted with minor revisions. This research presented is of high quality, references included are more than sufficient, the writing and figures presented exceed general publication quality. I present below three major concerns regarding the organization of the paper and the statistics included in the analysis that I would like to share with the authors in the hopes of improving the final manuscript.

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.

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

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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?

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. 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.

Minor Concerns and Comments:

NAO: Some discussion of the NAO is found scattered 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

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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 week 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.

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

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.

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

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.

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 pos-

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sible 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)?

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

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

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.

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 rational for their conclusion.

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).

Page 16: Please see the major revision section of this review; but the discussion here

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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.

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.

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.

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?

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