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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Saharan dust transport and deposition towards the Tropical Northern Atlantic" *by* K. Schepanski et al.

**K. Schepanski et al.**

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*Before answering on the referee's comments, the authors would like to thank for the detailed and helpful comments. The response to reviewers comments are indicated by italics.*

This article consists of two parts. The first is a description of output from a regional model and a comparison to MODIS and AERONET retrievals of aerosol optical thickness (AOT). The description is standard and has been done with other models, and I suspect that this part of the article will be of interest mainly to people who need documentation of this particular model. The second part of the article might be of interest to a much broader group of readers. Here the authors use model output to examine some of the assumptions made to retrieve deposition from satellite radiances.

Deposition can be calculated from the divergence of column transport, which is the product of concentration times wind velocity summed over the entire depth of the column. Satellite instruments retrieve column AOT instead of the vertical dependence of concentration, so they have to assume a (spatially uniform) vertical profile and estimate column mass using AOT. This raises the question whether the profile has been chosen correctly and whether it is really spatially and temporally uniform. It is straightforward to test these assumptions using model output, which is what the authors do in their final section. Before I recommend the article for publication, I would like the authors to take into account some of my comments below.

(If the authors have any questions, they are welcome to contact me at rmiller-giss.nasa.gov.)

1) Section 3.6: This is probably the section of the article that will be of interest to the broadest group of readers, and there are interesting calculations here. Unfortunately, the description is not always clear, and strangely, the results are not illustrated by any plots or tables. The authors need to systematically describe the assumptions made by Kaufman et al to calculate the flux and divergence (which is assumed to equal the deposition) and summarize the results of calculations that are used to test each assumption with a figure or table. First, Kaufman et al they assume that deposition is indicated by a downwind reduction in the zonal flux. This neglects the possibility that the zonal flux decreases because mass has diverged in a meridional direction. Given that the atmosphere is horizontally non-divergent to lowest order in Rossby number, this assumption is not obviously valid and needs to be tested. The authors point out this assumption (16078/26-27), but they should test it, which should be easy given the model output. (For example, they should make a scatterplot of deposition versus the change in the zonal flux along each latitude over the tropical Atlantic, including a range of latitudes. A high correlation would indicate that meridional divergence is unimportant, validating Kaufman et al8217;s assumption.) (Technical point: over



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a large enough latitude band, the divergence may be small, and Kaufman8217;s assumption may be valid. The authors could test whether the correlation increases as the latitude band used to calculate the zonal flux divergence increases.)
The flux equals the product of the dust mass times the wind, integrated over the depth of the column. Because Kaufman et al don8217;t have any information about aerosol variations with height, they make the simplifying assumption that the height dependence of both the aerosols and the wind are the same everywhere. Then, the flux is proportional to the product of the wind at a single height and the total column mass. In this case, the mass can be derived from the retrieved AOT. (This assumes that the AOT accurately reflects the mass contribution from all size categories. This is true for the model, but may be only approximately true for the real atmosphere, because of limited precision of the retrieved AOT and the fact that AOT is less sensitive to larger particles compared to smaller ones. Note that larger particles disproportionately contribute to deposited mass.) The authors appear to assess this assumption by comparing their cases 1 (the flux computed by integrating the product of mass and zonal velocity over the entire column) and 2 (the flux computed by using AOT and wind at a single level.) The authors report (16078/18-19) that these two cases result in "large differences" in the calculated flux, but should offer a scatterplot or table to support this. This would also be a good place to cite work by Mahowald et al JGR 2003, who find that monthly anomalies in surface concentration account for only about two-thirds of the variability of column mass. To be sure, Kaufman et al infer column amount from concentration at a higher level, so the Mahowald result is not strictly comparable. But Mahowald do demonstrate that the vertical distribution of dust is changing within the column from month to month, calling into question the use of a single vertical distribution implied by eq. 8.

*The reference to Mahowald et al. (2003) is added. Furthermore, the idea of the reviewer to perform scatterplots and to redraw parts of this section is implemented and strengthen the argumentation. Thanks for this helpful comment!*



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2) The authors write (16070/20): "Both, AOT computed from modelling result and MODIS measurements, show similar seasonal pattern", but Figure 2 shows obvious differences that aren't discussed. For example, the model dust plume is very diffuse compared to spatial variations in the MODIS retrievals. (This may be due to saturation of the color scale within the figure, and the authors should add colors for values of AOT above 1.0) Nonetheless, taking Figure 2 at face value raises the question of why there are more regional variations in the retrievals compared to the spatial uniformity of AOT at the center of the model dust plume in Figures 2a abd b. Is this because of excessive horizontal mixing in the model? Are the model dust sources too extensive? Also, while the model shows a large decrease in dust loading from the winter to summer, this decrease cannot be assessed in the MODIS retrievals, given their absence over bright desert surfaces that are major sources of dust. The authors need to evaluate their model dust load using a product like the TOMS (or OMI) AI or Deep Blue that is available over the entire Sahara. To be sure, the TOMS AI has a spatial dependence upon aerosol height, and Deep Blue has to assume this height (which may vary from the actual plume height). Nonetheless, Northern Hemisphere Africa is an important region and a comparison needs to be made over its entirety even if the retrieval product is imperfect.
Similarly, descriptions of the deposition (Figure 3) and vertical dependence of aerosol concentration (Figure 5) are limited in value by the absence of observations for comparison. To be sure, deposition measurements exist only in limited locations and have large uncertainties, but LIDAR retrievals are increasingly available. Users of MUSCAT would find the descriptions of model vertical profiles more useful if these were accompanied by profiles typically observed during these months.

*The discussion on modelled and observed AOT distribution is redrawn. Deep-Blue AOT and OMI AI monthly mean fields are added. The scaling is changed and now points towards regional differences in the simulated AOT field, which are now*



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*discussed in comparison to the satellite retrievals.*
*As the AOT fields implicitly include the treatment of dust emission, transport and deposition processes, the comparison of modelled and observed AOT in its spatial distribution (now Section 3.2) and temporal evolution (now Section 3.4) is discussed in the revised version of the manuscript. Furthermore, a comparison of modelled dust size distribution and aerosol size distributions retrieved from observations (AERONET) is shown to account for the reliability of the model.*

3) Below, I have made suggestions to improve the grammar and make the text more comprehensible. However, what should have been corrected before submission is the mislabeling of wind directions. "Westerly" is mistaken for "westward", and similar errors appear in a few places.

Technical comments:
16063/18: The authors use the word "exemplarily" often in the article to describe the months they have chosen for analysis. While "exemplary" is an actual word, the adverbial form used here is unusual and doesn't always mean what the authors intend. I would suggest instead using words like characteristic/characteristically or typical/ typifies. For example, replace "This paper aims to show exemplarily for three single case studies..." with "This paper shows for three case studies typifying the seasonal cycle of dust activity..."
*changed*
16063/23: replace "refereed" with "referred to"
*done*
16064/10: "Local wind systems...depend on topography." Why does this need to be said?
*It is mentioned because the topography is smoothed for meso-scale modelling and so e.g. channeling effects and precipitation due to mountains show different effect on different grid resolutions (e.g. Reinfreid et al 2008, revised).*



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16064/17: "soil dependent threshold" Please state how the threshold depends upon the soil.
*done*
16064/24: "In detail, the parameterisation considers..." Please provide a reference that describes the parameterization precisely.
*The reference Heinold et al., 2007 is added.*
16065/8: "correct in continuum" If the particle is smaller than the mean free path of the air molecules, is the continuum approximation valid?
*The mean free path of air molecules under atmospheric standard conditions is around 0.068 $\mu$m. This is smaller than the smallest dust size considered.*
16065/13: "Resistance" is based on an analogy to electrical circuits. What physical process related to deposition is represented by resistance?
*The resistance length accounts for the turbulent mix-out, which occurs independently of the gravitational settling.*
16065/16: replace "an increasing" with "a strong"
*done*
16066/6: replace $\rho$ with "$\rho_p$" to be consistent with eq. 6?
*done* 16066/eq. 6: should the numerator be multiplied by the air density?
*The formula has been compared to formulas used in other publication, e.g. Tegen and Lacis, 1996*
16066/12: "up to 12 km" This is a very low model top that is often exceeded by the depth of summertime tropical convection. Does the LM model Have a higher top? Does this low lid artificially limit the vertical extent of dust transport?
*The meteorology is simulated up to 24 km, which covers the entire summertime troposphere and simulates also deep moist convection. Only dust is treated by the MUSCAT part up to heights of 12 km. Dust transported to heights above the top of the MUSCAT model is lost. The information on the LM model top height is added to the text.*
16066/22: please define "endoheric"



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*done*

16067/1: "Each dust source area is characterised by seasonal and annual changes in frequency of dust source activation." How are your source regions prescribed? Are you using the paleolake preferred source of Tegen et al 2002? Are there prescribed seasonal variations in the sources?

*Potential dust source areas are prescribed using the mask presented by Schepanski et al., 2007. Areas which have been identified as dust source area for at least two times during 2006/03-2007/02 are set to potential dust source areas. If a potential dust source emits dust, its strength depends on the meteorological condition provided by the LM part. So also seasonal changes in location and strength of dust sources depends on the correct reproduction of seasonal characteristics of the meteorology. Neither strength of dust fluxes nor seasonal changes are prescribed.*

16067/7: replace "exemplary" with "typical"?

*done*

16067/10: replace "preformed" with "performed"

*done*

16067/17: the "buoyancy" of what?

*'buoyancy' is replaced by 'atmospheric stability'*

16067/18: replace "gravitationally" with "gravitational"

*done*

16067/20: "or washed out by rain events" or removed by dry deposition?

*rephrased*

16068/3: replace "remain stationary" with "does not change its height"

*done*

16068/6: insert "and" before "a"

*This sentence is rephrased.*

16068/13: replace "source areas" with "dust near its source"

*done*

16068/17: replace "seasonal" with "seasonally"



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*done*

16068/20: "the BL is deeper during summer" This is not "due to northward shift of the ITD" but to greater solar heating of the surface during this season.

*The aspect of changing solar heating is now considered.*

16068/23: replace "fast" with "rapidly"

*done*

16069/10: replace "exemplarily for" with "typifying"

*This section is revised.*

Figure 2: Please mark the regions in each map where no MODIS retrievals are available. (At the same time, please compare model output to a retrieval like TOMS or OMI or Deep Blue which extends across the entire Sahara.)

*Monthly means of DeepBlue AOT and OMI AI are considered now.*

16069/13: what is the geographic extent of the "Savanna" and how does it differ from the Sahel?

*'Savanna' is removed*

16069/19: "the mean (spring) values are higher (than winter values)" This is not apparent from Fig 2a and b, which show comparable maximum values.

*This section is revised.*

16070/6: replace "southerly" with "southward" or "northerly"

*This section is revised.*

16070/9: "MSG IR dust index" Please provide a reference that demonstrates this.

*The observations have been done by the authors. As complete discussion on the MSG IR dust index and its observations concerning stationary dust plumes will be beyond the scope of the present paper, this part is removed.*

16070/22: "The AOT differences may be partly caused by the lower temporal resolution of measurements compared to hourly extracted modelled AOT values." The use of monthly averages in Figure 2 would seem to minimize this effect. Nonetheless, if the authors want to make this claim, they need to subsample the model output to every 6 hours and see if it looks more like MODIS.



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*Figure 2 shows monthly averages, whereby the way of averaging accounts for missing values (e.g. due to cloud masking). To make observations and simulations most comparable, only AOT fields around noon time are used for the computation of monthly means.*

Figure 3a: Why does dry deposition decrease dramatically as the plume crosses the coast? I realize that the continental BL air rides up over the marine BL and becomes the SAL (whose elevation reduces the efficiency of dry deposition), but this transition in is very abrupt. Is the small spatial extent of this transition seen in LIDAR retrievals of the SAL?

*This is a good point and will be investigated in further sensitivity studies.*

16072/2: replace "are a main characteristic of" with "characterize"

*done*

16072/5: replace "offshore" with "of"

*This section is revised.*

16072/21: delete "especially"

*This section is revised.*

16072/24: "the magnitude of AOT is well reproduced" Figure 4c shows that there are several weeks in July where the model underestimates AOT.

*The discussion on AOT comparison to the model results is revised. Furthermore, additional stations are shown to draw a more completely image of the model-measurement comparison.*

16072/26: "Comparison with deposition fluxes" What were the deposition fluxes compared with?

*This section is revised.*

16074/11: delete "tracking"

*done*

16074/28: replace "Additionally to" with "In addition to"

*This section is revised.*

Table 1 and 2: I believe that the results in these tables would be much easier to



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understand if each table was replaced by a pair of plots. The zonal flux could be plotted as a function of latitude with each of the three longitudes given by a single curve. One panel could be for January and one could be for July.
*Table 1 and 2 are replaced by plots as suggested by the reviewer.*
16075/12: replace "south to southwesterly" with "northerly to northeasterly"
*This part is revised.*
16075/14: replace "westerly" with "westward" or "easterly"
*This part is revised.*
16076/6: Is "most active" defined in terms of emitted mass or the number of emission events?
*It is defined in terms of number of emission events. This information is added to the text.*
16076/9: replace "The present modelling study show a part of Bodele from up to 50% over the Cape Verde Archipelago." with "The model calculates that up to 50% of dust over the Cape Verde Archipelago originates from the Bodele source."?
*The sentence is rephrased.*
16076/17: replace "dependences" with "dependence"
*done*
16076/21: Please replace "lower tropospheric heights" with the actual height range.
*done*
16076/29: "be in disagreement" Please define the disagreement.
*This section is revised.*
16077/1: "can be larger" Please give an example of where this is shown in Figures 6 and 7.
*This part of the discussion is revised.*
16077/9: replace "southwest" with "southwestward or "northeasterly"
*done*
16077/eq. 8: The authors should note that this is essentially the same formula as eq. 6, subject to a few assumptions, and then they should state these assumptions.



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This might allow them to identify why the MUSCAT model shows a coefficient of proportionality around 1 compared to the value of 2.7 used by Kaufman.
*See answer to comment (4) by reviewer 1.*
16078/9: Kaufman et al. (2005) assume "that the gradient of AOT between two points is related to the atmospheric removal of dust." This is not quite true. Kaufman et al assume that the gradient of the *zonal flux* (which they assume is proportional to AOT) is related to the removal of dust.
*This point is rephrased.*
Figure 9 shows that the relation between column mass and AOT is roughly of order 1 g/m$^2$ but varies like a Gaussian. This means that using the average relation will give errors at many locations where the local relation varies from the mean. These errors will result in spurious flux divergence (and its implied deposition) given a spatially uniform wind and uniform dust concentration. This should be noted. (Again, this is the most interesting section of the article from my perspective, and I think the authors can attract more readers by making their discussion and assessment of the Kaufman et al assumptions more detailed and systematic.)
*This argument is now considered.*
Figures 6 and 7: Please use pressure as a vertical coordinate instead of model level.
*The figures are redrawn and now showing altitudes (km) instead of model levels.*

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16061, 2008.



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