Reply to reviewer's comments

We thank all reviewers for their useful comments. The questions and comments were taken into account for the revised version. The responses to the reviewer's comments below are also marked in italics.

Referee 1

General comments

The areas where dust emission is allowed in the model correspond to dust source activation found in the MSG observation. Moreover, the threshold used to identify the dust events in the model was based on the MSG observation. However, no dust emission can be detected from MSG observations in presence of thick cloud cover or high water vapor loading. These strong limitations in the dust source activation obtained from MSG observations lead to constraints in the model that should be discussed. The dust source activation occurs almost exclusively between 06:00 and 12:00 UTC according to the MSG analysis (Fig. 1, top row). However there is a significant literature discussing about dust source activation during other hours because of Sharav cyclones and mesoscale convective systems (as cited page 27675). As the later are associated with cloud cover, their associated emission should lack in the observations and in the model. This point should be discussed.

We agree that the MSG product may contain some bias due to the fact that dust emissions may be underestimated because of cloud cover. This may in particular lead to a bias in the time of day when dust emissions are counted. Dust emissions that occur in connection with moist convective events would occur mainly in the afternoon and evening hours. Due to the development of clouds these events may be undetected by the satellite. We should note, however, that dust emission events occurring below cloud cover are in fact counted if the dust event is large enough that it emerges from below the cloud and the source is clear– in those cases the actual source area cannot be located exactly, however. This possible source of error in the dataset is noted e.g in Schepanski et al. (2007). We strengthened the emphasis on this in response to the specific comments below at various parts of the manuscript, and in the conclusion section.

Specific comments

Abstract

- Page 27668, line 7. "Considerable number", please give a number

Numbers of dust source activation that occur in connection with low-level jet formation in the model or in the morning hours in the MSG observations are given in the text. There are caveats with those numbers that are discussed (e.g. possible underestimates of modeled dust activation due to insufficient mixing in the model), therefor we did not provide those numbers in the abstract, since an appropriate discussion is not possible there, but that is done later in the text. The sentence in the *abstract is reformulated.*

- Page 27668, lines 11-17. Observations at AERONET stations are not necessarily representative of dust variability. So an agreement between the model and AERONET observation does not imply that the model reproduces the aerosol optical depth well everywhere. Thus the deficiency of the model to reproduce the interannual variability might be of importance.

  Yes – this agrees with the argument we want to make. While not necessarily representative for the full spatial extent of the Saharan dust optical thickness, the Aeronet AOT provides at least some quantitative comparison at specific locations. However, while the MSG dust emission counts show a clear difference between the two years. This could be due to (1) the Aeronet stations being really not representative; (2) the difference in the MSG data are due to strongly different cloud cover or humidity conditions in the Sahara the two years (however there is no indication for that), or (3) the dust emission counts are not representative for emission fluxes and are thus only a minor influence for AOT (i.e., the many emission events caused by low level jet breakdown that are indicated by the observations contribute only to a minor amount to the dust AOT, while dust AOT is strongly influenced by less frequent major dust events like frontal passages). Explaining interannual variability of dust remains an important challenge for a full understanding of the controls of the atmospheric dust load. This part of the abstract was re-formulated to make it clearer.

Section 3.1 Dust source activation frequencies

- Page 27674, line 8. "Cloud cover higher than 50%", please justify the choice of such an arbitrary threshold. The total water vapor content that masks the dust signal in the MSG observation should be taken into account as well. Why do you not consider it?

  The 50%-threshold is a simple, conservative criterion to roughly mimic the effect of cloud cover on satellite dust retrievals. Sensitivity tests, however, show that the results are actually not very sensitive to the choice of cloud cover fraction at which the model grid cells are excluded as dust sources.
  The influence of water vapor on the dust on the thermal infrared signal is described e.g. by Brindley et al (2012). This limits the usefulness of the MSG data in the Sahel, where the dust signal is reduced at high humidity conditions. For quantitative dust retrievals from the MSG data the influence of the humidity on the IR signal would need to be considered explicitly, together with the surface emissivity and height of the dust layer. However, for the retrieval of dust events, the dust plume (from IR brightness temperature differences at 8.7, 10.8 and 12.0 μm) is detected visually, as described e.g. in Schepanski et al., 2007 (GRL) and 2009 (JGR). This method considers the spatial development of the dust cloud, i.e. the displacement of the dust signal between retrieval time steps. This method is not sensitive to changes in color shades due to changes in meteorology and surface emission. Also, weak signals can be detected. Schepanski et al. (2007) compared this MSG dust frequency product with dust storm data from weather stations and found good agreement. Certainly, for quantitative retrievals of dust optical thicknesses (respective dust loads) the impact of water vapor must be taken into account, together with other

dependencies of the dust signal.

- Page 27674, line 25. Dust emission due to LLJ is just an important (not dominant) mechanism as it accounts for 40% of events only (Table 1 and page 27675, line 19).

  The sentence was changed according to the reviewers suggestion.

- Page 27674, line 27. Please indicate some reasons explaining the DSA observations are biased towards the morning. The quasi-totality of emission in the morning is too surprising to be right.

  We added the argument that dust emissions in the afternoon that occur in connection with wet convective events may be obscured by cloud cover and can thus not be detected. The strong morning peak in dust emission numbers derived from geostationary MSG data was already described by Schepanski et al (2009). Other remote sensing products retrieve information once daily and thus cannot provide information on the sub-daily dust emission frequency. The important role of the break-down of nocturnal low level jets which leads to morning dust emissions in the Sahara has been noted e.g. as important mechanism in the Bodele depression, the most important dust source worldwide (Washington and Todd, 2005). We must note that the high frequencies of morning emissions do not necessarily mean that the dust emission fluxes are highest dust emission fluxes from these mechanism, since the many small, short-term dust emission events may not lead to strong dust events, and the dust that is emitted from such sources may not contribute considerably to far-range transport if it is not mixed up high in the atmosphere.

- Page 27676, line 7. You stated that "the passages of Sharav cyclones are the cause of major dust emission events in Northern Sahara". Because the Sharav cyclones do not shown any diurnal cycle and DSA occurs exclusively in the morning hours in October to January, this more than suggests that the MSG observations underestimate DSA.

  The actual sentence in the text is: "This may suggest that here the MSG observations underestimate DSA, as during these large scale events dust sources may become activated during the passage of Sharav cyclones below the passing dust cloud. These events would not be detected by the MSG imagery as new DSA, leading to an underestimation of DSA in the observations." This actually agrees with the statement of the reviewer. To be more clear, we added: This underestimate may increase a bias towards the maximum number of dust emission events in the morning hours.

- Page 27676, line 8. Similarly to the Sharav cyclones, mesoscale convective systems would mask dust emissions leading to another underestimation of DSA in the observations.

  This is mentioned elsewhere in the text (e.g. page 27677, line 9 in the submitted *version), but we added a statement here as well.*

- Page 27676, line 13. "The agreement between modeled and observed dust emission events is very good". This statement is subjective. Few lines below, you write "missed emissions are most evident for the annual average in the Sudan and the mountainous region along the border between Algeria, Mali and Niger". So the agreement is not so good. Please prefer numbers to subjective statements.

  This sentence as written in the manuscript only refers to the Bodele region. (Accurate citation: "In both years the agreement between modeled and observed dust emission events is very good in the Bodele region in Chad, which is the most active dust source in the Sahara desert."). That this is not the case for other regions is mentioned in the following. For the Bodele region, here taken as the area between 15°E and 20°E, 15°N and 19°N, the sum of emission events for the 2 years are 4095 for the model simulation compared to 4476 events in the observations based on 1°×1° datasets. These numbers are added in the text.

- Page 27677, line 1. Why is an overprediction of the morning wind peaks due to the LLJ breakdown unlikely?

  This refers to the arguments given above (page 27675, lines 1-7) which refer to earlier results of the model. We added the appropriate references again here.

- Page 27676, line 8. "Potential deficits in the model capability to reproduce moist convection may be also obscured in favor of the model". It would be worth mentioning here that the model does show some emissions in the afternoon while MSG does not. This discrepancy in hours of emission has to be discussed.

  That the MSG data may be biased by obscuring the dust signal connected to cloud cover occurring during moist convective events is already noted in the following sentences (lines 8-10 in the submitted version)

- Page 27677, line 9. "...to cloud-free conditions" and high water vapor loading as well. This should be noted.

  A note was added in the text according to the reviewers suggestion.

- Page 27677, line 25. "...a close agreement is achieved between the model and observations". As the number of DSA in the model was constrained to equal the number of DSA in the observation, adding number of days with LLJ occurrences but without dust emissions increases by 30% the number of DSA in the model. In that case, there is no more a close agreement between the model and observations. It would be worth to remark it. Furthermore, the location of these additional numbers of days should be shown in a new figure to verify the suggested close agreement.

  That is correct – The fact that the limit for dust emission fluxes to be counted as 'dust emission event' (excluding small emission fluxes) would of course need to be increased if all low-level jet events would lead to dust emissions in those areas where the model underpredicts dust emissions compared to the observations, if an *agreement between total number of DSA in model and observations should be achieved. If the threshold would be increased, accordingly, then also the number of overpredictions would decrease. However, this is just hypothetical and given here as a possible explanation for the underprediction of dust emission events by the model in mountainous regions. The choice of the threshold used in this work was based on the actual model results, even if the model has some deficiencies. Some discussion was added to the text.*

Section 3.2 Dust optical thickness

- Figure 4. For an easier comparison, the model outputs should be shown using the same projection than the one used for the observation. Also, it would be preferable to plot the observation in the top row (as in Figures 1 and 3). Why do you use OMI index and not MODIS Deep Blue aerosol optical depth? This would render the evaluation more quantitative. Apparently, the aerosol optical depths from the model from 2007 and 2008 are much lower than those retrieved from MODIS Deep Blue (see Fig. 1 of this review).

As suggested by the reviewer, Figure 4 was changed so that the same projection is shown for model results and satellite data.
For the comparison of the year-to year Saharan dust (Figure 4) we used the AI data as qualitative indicator as here we wanted to focus on the differences between the two years rather than performing a quantitative validatiion study for the modeled regional AOT distribution. The MODIS Deep Blue product is certainly interesting and has great potential for identifying dust over land surface. One needs to be aware, however, that quantitative retrieval over land surfaces remains difficult. Carboni et al., (2012) notes discrepancies of the MODIS DB data with Aeronet AOD as well as differences between different AOD retrievals from different satellite instruments, particularly over land surfaces . According to the authors, Deep Blue AOD showed a bias of 0.18 for all and 0.39 for dusty conditions compared to Aeronet AODs. Ginoux et al (2012) investigated dust sources by means of the Deep Blue product where they performed a careful analysis of ''Dust Optical Thickness" compared to "Aerosol Optical Thickness" in for the satellite data. Their Deep Blue dust OT is clearly lower than the DB AOT product, that is readily available. Such an analysis to derive the dust optical thickness from the total aerosol optical thickness product is beyond the scope of this investigation. Since quantitative evaluation of the regional dust model has already been performed for various case studies and for the different aspects of the model (dust AOD, in-situ size-resolved dust concentration from surface and aircraft measurements, deposition fluxes, vertical extinction profiles: (Tegen et al. (2006), Heinold et al. (2007), Helmert et al., (2007), Laurent et al. (2008), Heinold et al. (2009), Wagner et al. (2009), Müller et al. (2009), Heinold et al (2011), Niedermeier et al. (2012)) the aspect of quantitative model validation was limited in this study to comparison with Aeronet AOT. That comparison, however, was extended to include more stations (Figure 5). The focus of this paper was to study the usefulness of the dust emission frequency data derived from the MSG satellite instrument. The comparison with the satellite map for the atmospheric dust content with annual averaged dust optical thickness by the model was done to test to which extent the difference in the numbers of dust emission that is evident in the MSG product is reflected by similar differences the atmospheric dust load.

- Page 27678, line 23. Please justify the choice to compare optical thickness observations at 440nm with model results of dust optical thickness at 500nm wavelength. An accurate comparison would use optical thickness observations at 500nm.

  This was actually a mistake in the paper. Since Aeronet sunphotometers measure optical thicknesses as standard wavelengths of 440 nm, 670 nm, 870 nm, and 1020 nm but not 500 nm, we actually calculated the modeled dust aerosol optical thickness from the mass size distribution for 440 nm wavelengths instead of 500 by using the appropriate extinction efficiencies for each size class at this wavelengths. The description quoting 500 nm was in error (however, the actual differences of the results at these two wavelengths are small). Thank you for pointing out this error

- Page 27679, line 4. Dakar and Agoufou are not in the Saharan desert.

  We corrected this in the text.

- Page 27679, line 14. Please quantify "the good agreement at the station Saada" and "the notable discrepancies" at the other stations (e.g., bias, rmse, correlation). It appears that the AOD is strongly underpredicted in the simulation at Dakar.

  Correlation, Bias and RSME were added in the new Table 2. The underprediction in Dakar is noted.

- Page 27679, line 19. Agoufou is in the Sahel (not close to the Sahel).
- *We corrected this in the text.*
-
- Figure 5, the black dots are too small to be read

  The black dots in Figure 5 were enlarged.

Conclusions

- Page 27681, line 8. "The model results agree generally well with the AERONET optical thickness data". This sentence is not correct as many discrepancies were noted.

  This sentence was reformulated to also note the discrepancies

Typos

- "Ehile" (27679/14), "the their" (27680/20), "overpredicing" (27686)

  All corrected, thank you for pointing out the typos.

Referee 2, Slobodan Nickovic

In the phase of the quick access review, I already made a full paper review (see listed below). I invite the authors to address to those of my questions that they did not yet consider.

The manuscript "Comparing two years of Saharan dust source activation obtained by regional modeling and satellite observations" compares simulation of a regional dust model with observations. It analyzes capabilities and weaknesses of the model simulations, representing so an important guidance for model validations of a similar kind. A particular intension is paid to model treatment of dust emissions and the influence of mesoscale atmospheric systems such as low-level jets and cyclone passages. The paper is well-written and clear and the model experiments and observation evidence well documented. Before this work is published I still want to address to the authors a few comments and advices as listed below:

Thank you for the nice review!

- Model results are compared against four AERONET stations mainly distributed over Western Sahara. I suggest that the validation is extended by including more AERONET stations belonging to the model domain.

  There are only few AERONET stations providing data for the two years that were directly in the vicinity of the Sahara desert and thus being exclusively impacted by desert dust. .We did not include downwind stations in Europe of the island locations since these would also strongly influenced by other aerosol types (anthropogenic, smoke). In the Sahel region there is the additional location Banizoumbou providing observations for the two years. However, we found that the data from this station compare similarly to the model results as the data from Agoufou, which is influenced by the same large-scale meteorological features. The similarly for the station Blida, which compares favorably to model results similarly to the station Saada. In this revised version we also added those additional stations. We also added the stations Cabo Verde and Izana, at island locations downwind of the Sahara deserts that are strongly influenced by desert dust aerosol. In addition we would like to point out that the model has already been validated with various other observations for individual case studies , including lidar observations of extinction profiles, and size resolved concentrations measurements at the surface and from aircraft at different altitudes (see our reponse to Revier 1 on Section 3.2 and the references cited there). Here the focus is mainly on the comparison of the modelled emission frequencies with the MSG observations.

- It is somehow unexpected that the model behaves much better against one observation type and underperforms against the other one. More elaboration on that issue could be appropriate.

  The discrepancy is indeed interesting, it shows that different observations have different sensitivities or are not all equally suitable for use in model evaluation. It could also point toward problems in the representation of the dust in the model, which may need to be modified. See also reply to the 2nd specific comment of reviewer 1.

- Pg 11 The authors state: "This strong underestimate is likely due to failures in the model to reproduce the meteorological conditions correctly that lead to dust emission upwind of this location at this time of the year. It could be either due to dust emitted by wet convective events, which the model does not reproduce, or due to insufficiently resolved topography resulting in incorrect wind fields in this mountainous area." I should add that among suspected reasons could be the emission scheme as well. In fact, it is difficult to hypothesize a possible cause unless a sensitivity experiments are evaluated for a selected dust storm cases in the region. The same argumentation could be applied to other cases when the model fail to well reproduce the observed conditions. A need for additional close-up case studies could be a recommendation for future modelling studies.

  We agree and had already added a sentence about this in response to the earlier technical review.

- The manuscript title mentions satellite observations only, although AERONET data was also used for comparisons. An appropriate correction is suggested.

  As stated earlier, this paper specifically focuses on the usefulness of the comparison of satellite and model DSAF. The Aeronet data are used for additional validation at individual locations. Because of this focus we only mentionin the satellite data in the title.

Referee 3

Specific Comments:

- To validate the horizontal dust load distribution, the authors compare the simulations to the AI from OMI. This is a qualitative analysis and does not allow to conclude on intensity of the load. I suggest the authors include another quantitative analysis besides the one done with three AERONET stations. MODIS Deep Blue delivers information over the Saharan desert that can be used for a quantitative validation.

  We refer to our answer to Referee 1, first point Section 3.2.

- The authors clearly illustrate the importance of LLJs for dust emission in terms of frequency but they do not provide much information of these dust events in terms of emitted quantity. How much of the 1870 and 2330 Mt for 2007 and 2008, respectively, are emitted by LLJs? How much does the amount of emitted dust vary from one event to the other? The authors should extend the analysis in terms of the amount of emitted dust.

  Good suggestion. The dust emissions that occur in the presence of low level jets in the model contribute to 36% of the emission fluxes in 2007 and 38% in 2008. It should also be noted that the small dust emissions that were lower than the threshold to be counted as 'emission events' still contributed 18% in 2007 and 14% in 2008 to the total emission fluxes in the Sahara in the model. These numbers were added to the text.

- It is not clear from the manuscript whether simulated DSA events need to coincide with observed ones. It seems from sections 3.1. that the only requisites that need to be reached in order to have a dust event is that the daily emission flux in the model grid cell be larger than 0.6x10-4 kg m-2 s-1 and that the simulated cloud cover be lower than 50%. Could it be that a dust event was selected even if no event was observed that day? Please clarify and if so please include in the analysis.

  Yes, that is the case. Those events are included in Table 1 as "overpredictions". We added another explanatory sentence on the possible reasons for such 'overpredictions' in section 3.1.

- The simulated cloud cover is used to include in the analysis the fact that satellite cannot detect dust when cloud cover is present. Why not sample the model data according to the availability of observations? If cloud cover data are used the authors should include in the analysis the model performance to simulate cloud cover. What is the impact on the results.

  *We agree that the possible disagreement of the model results of cloud cover with actually observed cloud cover is a source of uncertainty in the results. However a thorough model evaluation of cloud cover simulated with the COSMO model would be beyond the scope of the investigation. To be consistent the observed cloud cover would need to be derived from the MSG data, since the time-of-day of dust emission is of importance for this study. Which then also leaves the question for the different spatial resolution of model and observations which value should be used as a limit of cloud cover. The results of this study are actually not highly sensitive to the choice of cloud cover fraction at which the model gridcells are excluded as dust source*s.

- Pg. 27674, line 24: Authors state that 65% percent of the dust in the model is simulated before noontime. Is here the same definition of dust event applied than for the definition of DSA (i.e. emission flux during the day exceeds 0.6x10-4 kg m-

2 s-1)? Please clarify if throughout the paper whenever referring to dust event it will always be using the above criteria.

The explanation was unclear in the manuscript and was changed. The modeled dust events are counted when emission exceeds 0.6x10-4 kg m^{-2} in the 3-hr time interval preceding the time of the day when the modeled DSA are noted. It was added for clarification also later in the text according to the reviewer's suggestion.

Technical Comments:

- Pg. 27674, line 6: "regions of in the Saharan desert" choose one.

  Corrected

- Pg. 27680, line 14: replace "Ehile" with "While".

  Corrected.

- Page 27691, Figure 5: Time series for Dakar is presented but not addressed in the text. I suggest the authors include Dakar in the analysis of pages 27679 and 26680.

  Done as suggested.

- Pg. 27691, Figure 5: change time axis from Julian to calendar day and black dots are hardly seen, maybe changing colour would make them easier to be seen.

  The black dots in Figure 5 were enhanced to make them more visible. However, we kept the Day of the year notation on the time axis rather than replacing with day of month, as that would make the plot more "cluttered".

- I recommend to include the location of the AERONET stations in one of the maps

  The locations of the Aeronet stations were included in the additional Figure 6.

Referee 4

- p. 3, l. 19: „Temporal integration is carried out by an implicit-explicit method." What is an "implicitexplicit method"? Please clarify.

  In the implicit-explicit scheme used in the model the horizontal advection is computed by an explicit second order Runge–Kutta method. The chemistry and vertical transport processes are integrated with an implicit method. This clarification was added to the text.

- I would like to see an intercomparison of modeled and measured (e.g. from radio soundings) low-level jets.

A comparison with radio sonde wind speed data has been carried out for several Saharan locations to validate the development of low level jet in the model. However, the available data lacked the information at the altitudes where the low level jets developed or are too infrequent ((See Figure below). It was found not to be helpful for this manuscript and therefore not included in this manuscript.

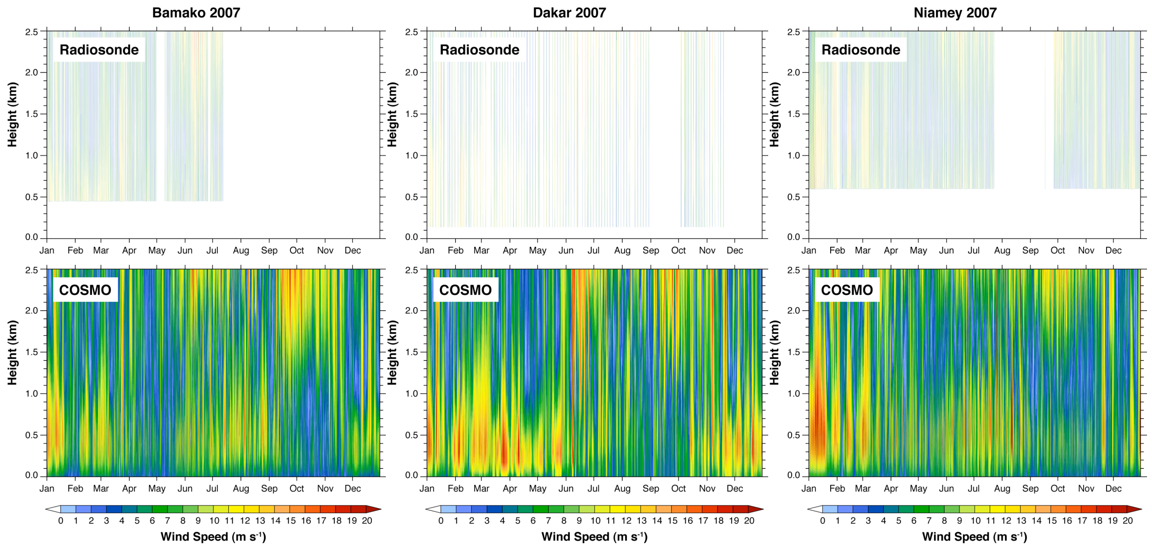

Figure: Comparison of wind speed time series for 2007 from radio sonde data compared to model results for three Saharan stations.

- Figure 1: I realize that the different colors are explained in the figure caption. However, it would be much easier for the reader if a legend explaining the different colors would be added to the graph itself.

  Done

- Figure 3: The figure caption refers to “Number of days per month (…)”. In contrast, the color bar refers to „Percent (days with dust emission)”. To which time period does “percent” refer? Please use only “number of days per month”. Does Figure 3a show results for the year 2007 and Figure 3b show the results for the year 2008? Please indicate. Same for Figures 3c and 3d.

  Done. The units in the caption, it should be percent. The years are indicated in the plot.

Technical comments:

- p. 1, l. 3: “. Tegen” à I. Tegen
-

  This error refers to the manuscript version in the technical review stage and had already been corrected in the current ACPD version of the manuscript.

- p.1, l.19: Add fullstop after „2007“.

Done

- p.6, l. 30: “2009). “. à remove fullstop.

  Done

- p.7, l.1: remove blank between “65” and “%”

  Done

- p.7, l.31: “can are” à remove „can“

  This error refers to the manuscript version in the technical review stage and had already been corrected in the current ACPD version of the manuscript.

- Figure 4: “a, b, c, d” are missing. Please correct.

  Done

References (if not included in the manuscript):

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