Reply to Reviewer Comments 1

This study investigates the ability of the new NMMB/BSC-Dust model to simulate two episodes during the field campaigns SAMUM-1 and BoDEx. This is an important evaluation of the new model that goes beyond the evaluation in the companion paper by Perez et al. 2011.

After the following revisions are considered, I recommend this manuscript for publication in ACP.

The authors would like to thank the reviewer for the constructive and very helpful comments. Below we provide a point-by-point response addressing each comment in detail.

General comments

A major question is on the consequences of this study for the operational setup of the model. The authors mention the article of Zender et al. (2003a) who describe the problems of NCEP soil moisture for dust models. Only for the BoDEx period the soil conditions are initialized with GLDAS data which results in a better reproduction of different variables. Also experiments with changed vertical to horizontal flux ratio or a reduced threshold friction velocity, that "improved the skills of the model", are only presented for BoDEx. It should be discussed how these changes (soil conditions, flux ratio, threshold velocity) influence the simulations during SAMUM-1 and if they are suitable for the operational setup of the model.

The reasons why we have chosen to evaluate the effects of incorrect soil moisture initialization only for the BoDEx period are:
1. We do not have meteorological observations directly over the major sources during SAMUM-1.
2. We carried out annual simulations for the year 2006 over the same region to test the behavior of the model. In summer, we found opposite results over the main Saharan dust sources, i.e. NCEP represented drier conditions than GLDAS providing better results in the dust simulations.
3. As the soil moisture experiments were setup to evaluate the model sensitivity regarding the effect of these changes upon the dust emission flux, it was not our primary goal to assess the reliability of the initial atmospheric data. For this a separate study would be required devoted solely to the assessment of the available soil data
4. The GLDAS database is not available in operational mode and at least for the moment cannot be used for real-time forecasting.
5. Potentially, a model-generated soil moisture (after 1-2 years of spin up) could be tested, but this is beyond the scope of this contribution.

In order to address this concern and to clarify this aspect, an additional passage has been added in the conclusion section of the manuscript. We also discuss the potential effects the changing soil moisture could have upon the emissions during the SAMUM-1.

Specific comments

Abstract

Page 30275, line 2: Please mention in Sect. 2.1 what "online" means in this context.
Page 30275, line 15: Insert "horizontal" between "operational" and "resolution".
Page 30275, line 15: Insert "dust" between "vertical" and "distribution".

All three points amended in the manuscript.

Page 30275, line 21: "...may be attributed to poor soil initial conditions." This is a vague statement that is only mentioned in the abstract and the conclusions but not discussed in Sect. 4.1.3. Did the authors perform a simulation with the GLDAS soil initial conditions for SAMUM-1 as well? Such an experiment could strengthen this statement.

The statement has been taken out of the abstract since no clear evidence of a relation between mixing height in the atmospheric part of the model and initial soil moisture data can be provided.

2.1

Page 30279, line 26: Please provide some more information and/or references for the STATSGO-FAO data.
Page 30280, line 26: Please provide some more information and/or references for the NESDIS climatology.

Reference provided in the manuscript

Page 30280, lines 24-26: The question arises why the simulations with GLDAS initial conditions are only performed for BoDEx, not for SAMUM-1.

See comments above (additional paragraph in the manuscript)

Page 30281, line 6: Where does the density of 2.6 g cm-3 for dust particles come from?

It is the typical average value used for Saharan dust particles. See for example Tegen et al. 2002 (Impact of vegetation and preferential source dust aerosol: Results from a model study).

3.1

Page 30281, line 18: Please provide also the elevation of Ouarzazate.

Amended

4.1.1

Page 30286, lines 1-2: Satellite images are only shown in Fig. 2 b,c,f,g, not in a-g.

Amended

Page 30286, line 9: The strongest signal in the MSG image is placed over the northeastern part of Sudan which should be discussed. OMI shows it as well. Does the model miss, underestimate or misplace this?
Page 30286, lines 9-12: It is not clear which dust is meant in the sentence "In the course of the day, the dust was advected..." because the previous sentence closes with "Western Sudan". It should be mentioned that "the course of the day" cannot be comprehended from the one Figure for 16 May.
Page 30286, line 12-14: Do the authors trust MODIS DB more than OMI? The SeaW-IFS image shows also a dust plume off the coast of Western Sahara and Mauritania. This seems to be missed by the model which should be mentioned.

We have reconsidered all these aspects and agree that some of them were inaccurately described. The paragraph has therefore been reformulated to clarify the queries of the reviewer. Concerning our confidence in the satellite retrievals (namely MODIS Deep Blue and OMI AOD), it is difficult to assess their reliability satisfactorily since there is a complete lack of ground-based observations over source areas. OMI seems to be more reliable with regard to the identification of aerosols in the main sources regions and to its north since the near-UV technique allows for the detection of aerosol properties over a larger variety of land surfaces. Although the Deep Blue algorithm tries to make up for the higher reflectance over desert surfaces in the VIS spectrum (making use of the 412 nm channel), its percentage error in the retrieved AOD increases, due to the fact that the error in the surface reflectance is a function of AOD itself. The interpolation algorithm may hence underestimate AOD over less dusty regions, as for example Libya, Egypt, or Mauritania. Otherwise, OMI has the tendency to accentuate the biomass burning aerosols a bit too much the closer one gets to the Sahel region and it is less sensitive to low level aerosol. As mentioned in the manuscript, the mixture between mineral dust and biomass burning aerosols potentially leads to AOD overestimation at times.

Page 30286, lines 17-18: Because the SeaWIFS image does only show half of the Iberian peninsula and due to the gaps in the satellite overflights, I think you can not conclude about this region from this image. I disagree with the statement that modeled AOD and the MSG image qualitatively match over the Iberian peninsula.

It is partly changed, however, south of the Iberian Peninsula and east of the overflight gap, SeaWiFS clearly shows dust, which is in good agreement with the modeled dust plume.

Page 30286, lines 20-21: The signal in the OMI image lies over eastern, not over western Libya.

Amended

Page 30286, lines 26-27: There is one small scale spot over the northern part of the Adriatic Sea in MODIS compared to a much larger signal over the region around Corsica. In my opinion it is not possible to draw any conclusions about the dust plume from the available images.

We reformulated this sentence such that the characterization of the dust plume can only be inferred from the model results.

Page 30287, line 6: Do the authors mean the border region Niger/Chad instead of "Eastern Chad"? Otherwise I disagree with this sentence.

Amended

Page 30287, line 9: Clouds obscure the region of interest to a large extent in the SeaWIFS image which should not be considered for this region at this time.

We added Sicily when it is referred to SeaWiFS in order to constrain the region of validity.

Page 30287, line 16: Delete "(Fig. 5)". Figure 5 does not show the "late afternoon hours".

Amended

Page 30287, line 26: Is the modeled 10m wind speed "rather smoothly distributed" all the time from 19-20 May or does this statement bear on Figure 6e? As this is the only point where the 10m wind speed is discussed, the authors could think about deleting the 10m wind speed panels from all the Figures 2-7. In turn, they could enlarge the other panels for better readability.

We agree with the reviewer and take out the respective figures with 10m wind speeds. Instead we have enlarged the SeaWiFS and the MSG images. The discussion has been adjusted accordingly.

Page 30288, lines 8-9: In my opinion it is more accurate to write "The model slightly overestimates the MODIS..." instead of "...matches also...".

Reformulated with regard to the source south of the Bodele

4.1.2

Page 30288, lines 15-16: Please clarify why values higher 0.6 indicate anthropogenic aerosols.

For dust model evaluation the Angstrom exponent is used to discriminate AOD measurements affected by fine anthropogenic particles. The Angstrom exponent follows the relationship between fine and coarse modes. Since coarse-mode particles is a feature that differentiates dust from fine-mode anthropogenic aerosols such as urban-industrial particles, an increase (decrease) in the Angstrom exponent involves an increase (decrease) of the ratio fine/coarse particles and in our case denotes the low (high) influence of the dust plume during an episode. As analyzed in Basart et al. 2009 (Aerosol characterization in Northern Africa, Northeastern Atlantic, Mediterranean Basin and Middle East from direct-sun AERONET observations.), a value of 0.6 represents an appropriate threshold value for use in dust-affected areas with influence of other aerosols. It is reformulated in the manuscript and two references are given as well.

Page 30288, lines 24-26: What is the reason for the overestimation at Banizoumbou? Is it due to overestimated emissions in the source regions or maybe due to wrong transport ways of the dust?

As shown in the companion paper Perez et al. 2011 (Atmospheric dust modeling from meso to global scales with the online NMMB/BSC-Dust model – Part 1: Model description, annual simulations and evaluation), the model has the tendency to overestimate the dust in the Sahel. It is due to its location downstream of the

major sources, whose emissions are slightly overestimated. As can be seen in seasonal comparison of the model AOD with the satellite imagery (Fig. 4 in Perez et al. 2011), the Bodele is overestimated in the model in spring and early summer. Hence dust from the Bodele is persistently transported to Banizoumbou (as it ought to be), leading to overestimation of AOD in a few cases in May, June, and July as can be seen in Fig. 5 in Perez et al. 2011. However, the remaining months and seasons show very good agreement at Banizoumbou. An additional remark has been introduced in the manuscript.

4.1.3

Page 30291, line 15: Please leave out "moderately". The model overestimates the dust by a factor of 3.

Amended

4.2

Page 30293, lines 23-25: Is this description based on the sun photometer measurements in Fig. 15a or on other data that are not shown? If it is only based on the shown data, you can say nothing about 10 March because there are no measurements shown for this day.

We took out the reference to the image, since we only describe the dust conditions during this episode.

4.2.1

Page 30295, line 9: The statement "... NCEP-GLDAS does a good job ..." is vague.

Partly deleted

Page 30297, line 7: I think it should read "Western Sudan". But how do you know that this dust is "freshly emitted".

Correct. It should read eastern or central Sudan. In fact, it is a line of dust emissions which is related to a southward progressing synoptic front or density current. Visible emissions start at 8AM UTC that day as inferred from MSG RGB images (not shown). It is clarified in the manuscript.

4.2.2

Only the simulation with the GLDAS soil conditions is discussed here. How does the model perform with the NCEP-FNL conditions?

The influence of the soil moisture on the 0-2km vertical winds is negligible. While small changes in the modelled 10m wind speed are visible (see Fig.15), the higher model levels are barely affected. Both model experiments are virtually identical.

5 Conclusions

Page 30300, lines 17-19: The possible explanations for the “insufficient mixing” are too vague.

It is taken out and replaced by a brief explanation of the general uncertainties related to the initial meteorology data used in the model.

Technical corrections:

Throughout Paper: Replace “Bodele depression” by “Bodele Depression”.
Throughout Paper: Replace “Tibesti mountains” by “Tibesti Mountains”.
Page 30280, line 19: Fig. 1 shows 0 -60 N, 20 W-60 E, while you say 0 -65 N, 25 W55 E in the text. Which of these coordinates define the model domain?

The actual regional model domain reaches that far (0-65N; 25W-60E) while Fig.1 is a rather schematic illustration. It is changed in the manuscript accordingly.

Page 30280, line 21: Leave out “in the vertical”.
Page 30290, lines 18-19: This sentence should be rephrased. Not the comparison but the profiles are similar. The comparison of profiles in Figs. 10 and 11 with the ones in Fig. 12 is pretty complicated. I recommend “meters above surface” for the vertical axis in Figs. 10 and 11 as in Fig. 12.

Sentence changed and Fig. 12 revised such that both show the standard height above sea level.

Page 30297, line 20: Should read “zone” not “zones”.
Page 30297, line 22: Should read “These...” not “This...”.
Figs. 8, 9: Note in the caption that Angström exponents are shown as black circles.
Figs. 10, 11: Please correct the labels of the stations in the caption: Fig. 10: Quarzazate (e), Fig. 11: Athens (b,d), Naples (e), Thessaloniki (a,c)
Fig. 14 j,k: In the title it should read “30.33N”.

All other errors are amended