Reply to reviewer 1 by Bernhard Vogel, Heike Vogel, Tanja Stanelle, and Christoph Kottmeier

We thank the reviewer for his valuable comments and suggestions. We followed each of them in our revised version of the manuscript. We restructured our paper to some extend.

The paper presents another just as good simulation of the March 2006 dust storm over West Africa. The case was already studied by Tulet et al. (2008), Mallet et al. (2009) and Cavazos et al. (2009), which all show a change in the atmospheric stability due to the radiative impact of dust. Following the aforementioned studies, the present paper describes the radiative impact of dust on temperature and reaches similar conclusions.

Several papers appeared in recent literature dealing with the radiative feedback of mineral dust particles and the state of the atmosphere. The ones mentioned by the reviewer studied the same dust event as we did. Modelling the radiative impact depends on the surface data, the emission parameterization, the modelled size distributions, the refractive index, the parameterization of the radiative properties needed for the calculation of the radiative fluxes, and finally in the method to quantify the impact of the dust particles. All these factors are connected with partially unknown uncertainties. To give an example, the studies mentioned by the reviewer neglected the impact of mineral dust on the longwave radiation or treated it in a simplified manner. Although, the papers including our may lead qualitatively to similar conclusions, they differ at least quantitatively. For that reason simulations of a specific situation with different tools and different methods are necessary to asses the current knowledge on the radiative impact of mineral dust particles on the state of the atmosphere.

It however differs from the previous studies by running their model over a shorter period (until 00 UTC 10 March versus 11 or 13 March) while the dust outbreak severely affected Djougou, Benin on 10 March (AOD around 2.5, see Figure 5 of Tulet et al.) and Ilorin, Nigeria on 11 March (A0D around 4). The rationale for this shorter period was not given.

We run our model for a period of 5 – 10 March 2006. The reason why we limit ourself to this time period is twofold. Firstly, we run the model in a stand alone version that means we are starting the simulation with initial data taken from the IFS model at 5 Match 00:00 UTC and are driving the model for the following with boundary conditions from the IFS model. This procedure differs from an operational forecast where initialization, assimilation and nudging with observations are done more frequently. For that reason we can not expect that simulating over a longer period using our method would allow a reasonable comparison with observations. Secondly, after 10 March the dust front penetrates into the ITC where a lot of clouds are present. This would not longer allow to quantify the "pure" effect of mineral dust.

Another drawback is a short and incomplete discussion of another case in June 2007 (see the specific comment below).

We agree with the reviewer and therefore extended the description and the evaluation of the case. We want to keep it a part of our paper as we wanted to study two different synoptic cases, one with a dust storm that covers a larger area and another one with more isolated dust plumes.

A new aspect offered by the study is on the dust feedback on dust emission.However the discussion on that part was neglected. My suggestion is to expand it in a revised version of the paper.

We agree with the reviewer and therefore extended this section.

Major comments:
The case of June 2007 is not sufficiently described compared to the other case. So it would more simple just to skip it. One of the interests of the June case in the present paper is the vertical change in temperature with respect to the height of the dust layer (Figure 10). You correctly pointed out that "the location and the vertical extension of the dust plume determine the effect on 2 m temperature" (p 7574, lines 19-20). However a similar figure was shown for the 2006 case on 10 March by Tulet et al. (2008). These authors show dust at 2-km altitude with mass concentration around 2000 ug/m3 (Figure 9c) associated with an increase in temperature up to 4 K (and a decrease in temperature below the dust layer; Figure 9f). Your case of June 2007 presents similar figures in dust concentration and temperature effect. But why not showing a similar figure from your simulation of the March case? This would make the paper much more comprehensive.

As indicated by the reviewer, we found and elevated dust layer also in our simulations of the 2006 case but only in the late hours of 10 March and in areas located in the ITC. As our simulation stops a few hours later we decided to discuss this topic using the 2007 case.

AOD retrievals from OMI, MISR, and/or MODIS should be preferred to RGB products shown in Figure 3 (p 7592) for allowing a quantitative comparison of AOD. Attached you will find a figure that completed your Figure 3 with AOD retrieved from MODIS deep blue AOD retrievals (available on http://daac.gsfc.nasa.gov/giovanni/). You wrote (p 7565, l 5) that "the position of the simulated dust storm is in good agreement with the observed one given by the dust product". This contradicts the comparison with MODIS observations, which shows a delay in the simulated dust front (in agreement with your conclusion on the comparison with sun photometer data). Also the simulation overestimated the local AOD maximum with respect to satellite data. This result contrasts with the AOD underestimation found with your selection of AERONET stations (Figure 4). In conclusion, your simulation is just as good.

We thank the reviewer for this suggestion. As proposed we include the MODIS deep blue retrievals and kept the RGB products. It allows a more quantitative comparison and shows in addition with the sun photometer data the limitation of the different data products. We included a clear description of the delay of our simulated dust front in the revised version of our paper.

Specific comments:

p 7555, First paragraph is very academic and far from the focus of the study. It should be shorten. In particular, the sentences on the aerosol-cloud interaction should be skip out as the study discuss on the radiative impact of dust only.

We shortened the paragraph.

p 7556, second line, the works of Tompkins et al. (2005) and Chaboureau et al. (2007) should be mentioned as they both show that a change in the dust representation over West Africa resulted in a modification of the monsoon activity.

We added both papers in the revised version of our manuscript.

p 7564 (l 25) and p 7565 (l 7 and 19). According to the caption, Figure 3 shows the simulated AOD, not the simulated emission fluxes.

We corrected that.

p 7566 (l 20) Which station do you refer to?

We corrected that.

p 7572 (l 6), Figure 7 does not show any correlation, but the relationship between Delta F and the AOD.

We corrected that.