

Review 1 – RESPONSE

The authors present a case study of a dust storm over the Arabian Peninsula. Is is an application study, using the regional weather research forecast (WRF) model with a chemistry/aerosol module incorporated, to investigate the impact of a specific dust storm on the Arabian Peninsula and adjacent ocean regions, particularly the Arabian Sea and the Red Sea. The novelty of the study is limited to providing data on the effect of dust storms in this specific region, if the one case is taken as exemplary for dust storms generally. The paper is a solid work, though. It provides a systematic and thorough analysis of the dust storm event, which by itself is very interesting and a valuable contribution to science. The manuscript is well structured and well written. I have only a few minor comments and questions I would like to have taken into consideration before the study is published.

The Arabian Peninsula is one of the most important dust source regions, where effect of dust on all aspects of natural and societal processes is extremely large but yet not well quantified. In this study we tried to formulate and answer to some of the most important questions related to dust mass balance, and its effect on radiation transport and nutrient balance in the Red Sea.

Minor Comments

1. Introduction: The introduction of the manuscript provides a very general overview on the role of dust aerosols for Earth's weather and climate. It could be shortened quite a lot, and still give a sufficient introduction and motivation for the presented research. Remove the parts on the general role of dust on a global scale. Focus on dust storms, what research has been done so far on modeling the regional impact of dust storms, and why a study, like the one presented by the authors, on the effect of a dust storm on the Arabian Peninsula is needed.

We agree to shorten this section as proposed.

2. Page 19185, lines 12-14: The authors write: "Sokolik and Toon (1999) and Claquin et al. (1999) showed that dust mineralogy is comprised of six main minerals: illite, montmorillonite, kaolinite, quartz, calcite and hematite."

In my opinion, this is not a correct representation of the content of the two studies. Claquin et al. (1999) provide a Mean Mineralogical Table (MMT) on the average mineral composition of 25 arid soil types. Even though the eight minerals (not six, since fractions of feldspar and gypsum were provided as well) in the table are main minerals found in soils, the fact that there are only eight minerals in the MMT is rather caused by the lack of available measurement data on the mineral composition of soils, when the study was conducted. There are other important minerals in soils, which also can have relatively high fractions, at least regionally, like chlorite, palygorskite, or halite. In a recent study, a new data set with the fractions of 12 minerals in soil was provided (Journet et al., 2014).

Sokolik and Toon (1999) studied how the state of mixture of hematite with other minerals affects the absorptivity of soil dust particles, which is important for their

radiative effect. They did not study the mineralogy of dust in general, though. Having said this, the paragraph in the Introduction with the statement on the two studies belongs to the part that could be removed altogether.

We agree that it would be better to remove this part.

3. Page 19189, line 27: How were the values for the size mass fraction s_p of the accumulation and coarse mode derived? The original GOCART model by Ginoux et al. (2001) was based on a discreet bin scheme with an upper limit of 6 μm particle radius, and s_p for clay and silt were slightly different to the values chosen here. This should be explained more. The parameters for the log-normal size distributions of the modal aerosol scheme used in the present study should be provided explicitly as well, for reasons of reproducibility. These are important parameters, since they essentially determine the emitted size distribution of dust, and, in turn, fallout rates and how much dust mass is being transported to remote regions.

The original GOCART emission scheme was coupled with the eight-bin aerosol model. But in this study WRF-CHEM is configured with the Modal Aerosol Dynamics Model for Europe (MADE) and Secondary Organic Aerosol Model (SORGAM) aerosol model (Schell et al., 2001; Ackerman et al., 1998). MADE/SORGAM uses the modal approach with three log-normally distributed modes (Aitken, Accumulation, and Coarse) to represent sulfate, nitrate, ammonium, organic matters, black carbon, and sea salt. Mineral dust is assumed to have only Accumulation and Coarse modes. GOCART has been modified to couple with MADE/SORGAM. It calculates a dust mass flux from the surface (see equation (1)) assuming that, by default, $s_p=0.07$ for accumulation mode, which for emitted particles has the modal diameter $D=0.6 \mu\text{m}$, and width $\sigma=2 \mu\text{m}$; and $s_p=0.93$ - for coarse mode with $D=6 \mu\text{m}$, $\sigma=2.2 \mu\text{m}$. According to MADE/SORGAM formulation the modal diameters change in atmosphere due to microphysical processes but widths of both distributions remain fixed. The total emission flux is adjusted using constant C in equation (1) to fit AERONET observations, as discussed by Zhao et al. (2010) and Kalenderski et al. (2013). We have modified the text on page 19189 to include the above discussion.

4. Page 19194-19996 and Figure 3: The description of evolution of the meteorological features in the regions in relation to the dust storm refers to the location of those conditions in the various countries of the region. Could the authors include the borders of the countries also in the maps shown in Figure 3, like it is done for Figure 1 and 2, if the plot program allows this? This will make it easier for the reader to follow the description in the text.

Figure 3 is corrected to show the political boundaries. See below:

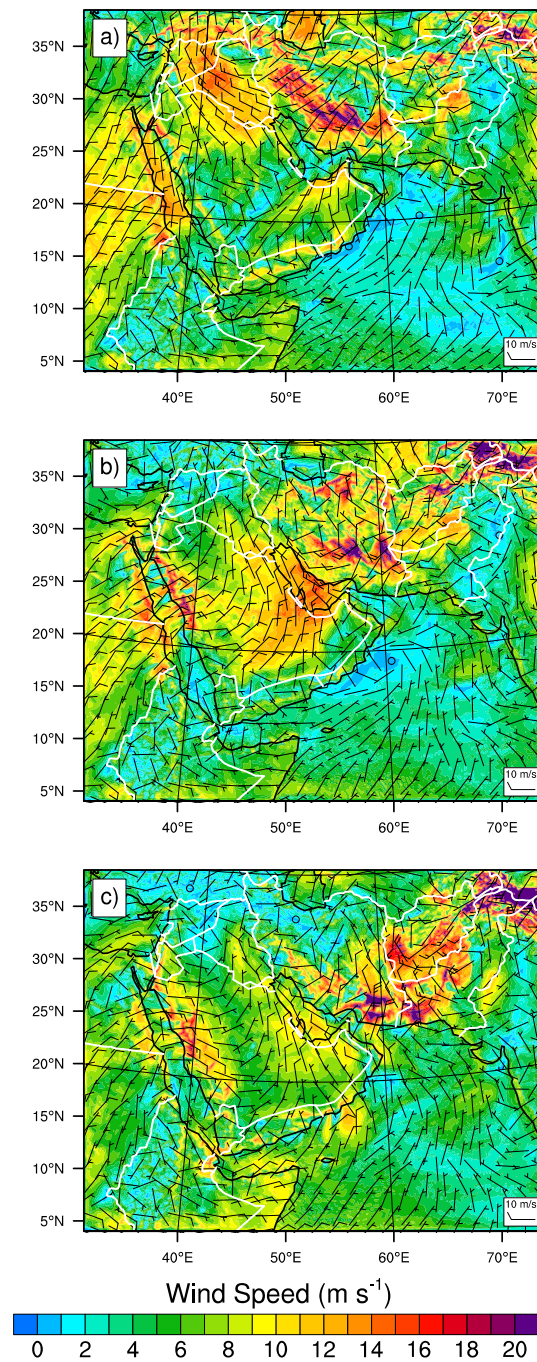


Figure 3

5. Page 19199, lines 24-29: The authors present an estimate about the numbers of dust storms, where the dust plume covered more than 20% of the Middle East area. It is not clear, though, how this number was exactly derived. The link to the Image of the Day published by the NASA Earth Observatory is not sufficient as

description of the data source and of the methodology for someone who wanted to reproduce this result. The authors should provide a more detailed description of the analysis for this part (in the Methodology section of the paper).

In this paper we have been interested in the total frequency and spatial distribution of severe dust events over the Arabian Peninsula and the Red Sea region. For this purpose we choose to “subjectively” analyzed the NASA aerial photos because we believe that this resource is not fully exploited yet. The development of full-scale dust storm tracking software to run on existing aerosol retrievals and satellite images is relevant and interesting, but is beyond of the scope of the current study. We counted dust events based on their extent and optical depth estimate. We found that for the entire region the number of extended dust events reaches about 20 for the period of observations. The number of specific events in a particular region, e.g. over the Red Sea, reaches 6-8. The number of regional storms is consistent with the results of the previous studies (e.g., Rezazadeh et al, 2013 and Prospero et al., 2002) and our estimates using observations from the sparsely distributed AERONET stations. The text at the bottom of page 19199 and at the top of page 19200 is modified to include this discussion.

6. Page 19204, line 17-18, and Figure 13: The authors write, “... aerosols exert a cooling or warming influence on the climate ...”

This is not wrong, but the effect is not just on climate, but on the atmosphere and the surface in the context of weather. The study itself does not analyze climate. Instead it analyzes the effect of dust aerosols related to a weather event. Thus, I would write “... aerosols exert a cooling or warming influence on the atmosphere and at the surface ...”

Equally, in the caption of Figure 13, replace “Positive values correspond to the heating of the climate system” with “Positive values correspond to heating” or with some other phrasing.

We agree to correct these sentences as proposed.

7. Section 3.4: It would make it easier for the reader, if the authors summarized the values for the domain averages of the radiative effect for shortwave, longwave, and net radiation at the top and bottom of the atmosphere in an additional table.

We have summarized the domain average aerosol direct radiative effects in the new table, as suggested by the reviewer. See the table below:

Level	SW DRE ($W m^{-2}$)	LW DRE ($W m^{-2}$)	Net DRE ($W m^{-2}$)
TOA	-4.25	0.25	-4
BOA	-16	6	-10

Table3. The domain average daily mean clear sky direct radiative effect (DRE) for shortwave (SW), longwave (LW) and net (SW+LW) radiation at the top (TOA) and the bottom (BOA) of the atmosphere on 19 March 2012.

Typos

1. Page 19194, line 2 and 3: write: “ERA-Interim uses an improved atmospheric model ...”

Thanks, corrected.