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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosols optical and physical characteristics and direct radiative forcing during a "Shamal" dust storm, a case study" *by* T. M. Saeed et al.

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We thank referee #2 for taking the time to read the manuscript and point to us many issues that needs to be either clarified or corrected. Here is a detailed response to the comments. For the sake of clarity the commentator's paragraph have been pasted between parenthesis while the response is stated below each paragraph.

"Specific comments: In the introduction, the authors should make it clearer that the paper is in fact an expansion of their previous study (Saeed and Al Dashti 2010) in order to facilitate the linkage."

A sentence in the introduction of the revised manuscript has been added to clarify this point and facilitate the linkage.

"It would also be useful to include prior literature on the subject of Middle East dust events. Rashki et al. 2012, Notaro et al. 2013, and Rezazadeh et al. 2013 come to mind. The latter is more or less directly linked to this work. Formenti et al. 2011 summarized the current understanding in dust properties; a reference which should not be omitted given it is the main subject of the paper (particularly in chapters 6 and 7)."

The references mentioned above have been incorporated in the revised manuscript, as per the reviewer suggestion.

"As in the 2010 paper, chapter 2 could conveniently be merged with chapter 4. Alternatively, why not simply referencing this very paper instead of repeating what's already been said. In any case, in line 19 at page 23899, please change "weather" to "climate"!"

We prefer to leave chapter 2, Study site, as it is so that to introduce the reader to our region of interest. The "Study site" has been stated in a very concise paragraph to make sure that the readers, as they skip through the article, know what type of an environment the paper is discussing. If the study site is removed and referenced only, as you suggest, then from our knowledge, not many readers would bother to search the reference and hence the paper becomes totally ambiguous and the reader will only be left to guess the type of environment the article is discussing.

"Weather" has been changed to "Climate" in line 19 page 23899.

"I strongly suggest that the authors sub-divide chapter 4 into ground-based and satellite data retrievals. The discussion of the dust "hot spots" deserves another sub-chapter."

The sub-chapter has been included in the revised manuscript.

"One major issue (which has already been raised in the comments) is the use of MODIS Terra over land. The algorithm (at least in its current version C5) is not designed to produce meaningful results over bright land surfaces. The MODIS Deep

Blue collection has been specifically developed for this purpose and should therefore be used. It can be combined with MODIS Terra or Aqua over oceans. The NetCDF data to achieve that are (usually) available on the Giovanni web interface: http://disc.sci.gsfc.nasa.gov/giovanni/".

Aerosol optical thickness product from MODIS Aqua and Terra platforms, level 3 collection 5 of the Deep Blue algorithm have been retrieved and are presented in the revised manuscript. This point had been raised by Dr. Sayer and the authors had adjusted the data accordingly.

"There you also find the MISR product which is available since 2002. I concur with Andrew Sayer's comment, that care has be taken when it comes to the interpretation and re-utilization of satellite derived optical parameters. While AOD and Angstrom exponents might be valid, they all have their intrinsic limitations which should be kept in mind when making quantitative statements or comparisons. Typically, this becomes apparent when several products are compared with each other. The mass concentration product does not seem to be validated at all. I therefore ask the authors to remove Fig. 9 and replace it with MODIS DB and MISR. It could nicely be combined to one Figure, including the TOMS AI from Fig. 10. Please modify Table 2 accordingly."

It was found that MISR, due to its narrow swath, did not cover our region over our days of interest. Therefore only MODIS Deep Blue algorithm AOT product will be presented in the revised manuscript together with TOMS aerosol index. Table 2 has been modified accordingly. Angstrom exponent product has not been considered since our interest is of dust aerosols over land mainly and the mass concentration product has been dismissed due to lack of validation.

"Further in chapter 4 (starting line 2 at page 23905), I am not convinced that it is a particularly compelling method to identify dust source "hot spots" from a few days of satellite observations. Not only would it require a much longer observation period to identify typical sources, but also is TOMS not the optimal tool to do that for specific

events (due to the limited temporal availability). You might wish to look at MSG SEVIRI for that purpose, as it is higher spatially and temporally resolved (as recently exploited by Ashpole and Washington 2012, 2013 to deduce a source map for Northern Africa). Note also, that Ginoux et al. 2012 recently published an updated source map (Fig.8) which is superior to your approach given the considered time interval. They even tried to attribute anthropogenic and natural sources. I therefore suggest removing this part of the paper (as it appears a bit outdated) and to use this reference instead."

TOMS AI distribution map will be used together with MODIS Deep Blue AOT product to look at the spatial distribution of dust aerosols. Source regions are discussed in view of Ashpole and Washington (2012, 2013) results, Ginoux et al. (2012) recent source map and also the older references mentioned in the manuscript. Unfortunately MSG SEVIRI data are not currently accessible to the authors.

"In chapter 5, again, sub-dividing the chapter would facilitate the reading of the paper. You first describe the SKIRON model, specify then the conducted experiments (including domain), and go then on to show the results. In this context, it isn't clearly stated (neither in the text nor in the figure caption) that Fig. 12 is already shows the first model results. In Fig. 13, it would be helpful to complement the caption with the important information that it is an aerial integrated vertical model profile."

The manuscript has been modified according to the reviewer suggestions.

"The main issue in this chapter is however, that the model description is based on Spyrou et al. 2010, rather than the more recent paper by the same authors (Spyrou et al. 2013). It is important, because the latter states that SKIRON now contains the RRTMG (rapid radiation transfer model) in order to simulated dust radiative effects. Why isn't it used in this study? I think everyone would be eager to see how the model performs and how it compares to the SBDART model with its underlying assumptions regarding the dust aerosol properties. As already mentioned above, I strongly encourage the authors to run these experiments and include them in the paper. In the current form, I have doubts that the mere results from the SBDART modelling exercise justify publication in ACP. This way, maps of the spatial distribution of the TOA and surface dust radiative forcing could be provided and compared/discussed with the available literature."

Indeed as the reviewer states the SKIRON model now have the capabilities of calculating radiative forcing due to dust particles, as stated in Spyrou et al., 2013. However the purpose of the manuscript is not to conduct a modeling study of the area, but to focus on a specific dust episode using mostly ground station data. In this context the SKIRON model is used as a supplementary tool to see the extent of the dust cloud. Also another reason for using SBDART is that it allows for a more thorough examination of the radiative effects, as it is a stand alone model, without the limitations of a radiative code operating in a LAM (limited area model), as SKIRON. So we believe that for the purposes of this study the SBDART model is more appropriate and more flexible for the calculations of the radiative fluxes. However the reviewer is correct that it would be very interesting to see how the atmospheric model fares and how it compares with SBDART and actual data, which can be the subject of a later study.

"Chapter 5, page 23907, line 16: What is the spin-up time of the model? Typically, several days are required to make sure the background dust loading is represented, despite the fact that this can never be entirely assured in regional simulations."

The spin-up time of the model was 20 days, in order to create a proper dust background as the reviewer correctly states. This information was added to the manuscript.

"Further on to chapter 6, sub-dividing the chapter into model specifications, aerosol parameter selection, results, and the discussion of the results would, once again, facilitate reading. Chapter 7 could then be easily merged with chapter 6 as another sub-chapter."

The manuscript has been modified according to the reviewer suggestions.

"In this context, I wonder whether it is justified to take the "average temperature value" (page 23911, line 6) – which, I assume, is equivalent to the climatological mean – to deduce the dust induced temperature change on both days in consideration. Given the synoptic activity, I can't make any sense of this statement as I don't expect the average temperature in the region to be constant. Hence, the dominant weather pattern (or meso-scale circulation regime) should have a considerable impact on the local temperatures. Please clarify or take out completely (including Fig. 14)."

Fig. 14 and the related text demonstrate the drop in surface temperature due to the change in radiative flux at the surface level. Since the instantaneous radiative forcing at surface level was considerably large we thought to look at its instantaneous effect on surface temperature. Therefore we compared the hourly variation in surface temperature during dust storm hours to temperatures in the absence of dust storm. Hence we calculated the mean hourly temperatures for clear sky conditions in the absence of dust activity over similar days for several years before and after 2003. We assume that this is a crude method of estimating the drop in temperature due to the dust cloud. Fig. 14 has been removed as it does not add more information than what the text does.

"With respect to heating rates (chapter 7, page 23912, line 16), it might be worth adding that they can change (regardless of the dust loading) owing to flux adjustments in response to the exerted radiative forcing (see e.g. Heinold et al. 2008). It remotely relates to the "effective radiative forcing concept", as it has just been introduced in the IPCC AR5"

Has been added.

"Minor comments and corrections: Introduction, page 23897, line 25: It is Goudie 2009. Same error on the next page, so please make sure the references are correctly spelled (e.g. IPPC=IPCC)"

Corrected.

“Introduction, page 23898, line 14: Ansmann et al. 2011 is another suitable reference”

Has been included.

“Chapter 3, page 23900, line 6: Providing 2 decimal places for an approximate value is mutually exclusive.”

Has been changed.

“Chapter 3, same page, lines 12-17: Please reformulate! “Steep isothermal gradient” would be more understandable. Also, “leading to cold air advection” seems more appropriate.”

Has been changed.

“Chapter 3, page 23901, line 6: “Pressure gradient force” is the only quantity which is used in models and theory likewise. The pressure gradient is – at least to my knowledge – no meaningful quantity which could be compared with other data.”

Has been corrected.

“Chapter 3, same page, lines 15-20: Not sure the radial (base) velocity scans are particularly helpful in illustrating the magnitude of the event. In my opinion, the radar image is more than sufficient.”

Has been removed.

“Chapter 4, page 23903, line 1 and 4: Are the given values associated with a dust storm (DS), rising dust, and suspended dust (S) your own definition? If not, please provide a reference (could very well be the case, that I missed that these definitions indeed exist). If true, please elaborate how the threshold values are determined.”

These definitions are given by the World Meteorological Organization and are used by Kuwait Meteorological Department. Reference has been provided.

“Chapter 5, page 23907, line 16: What is the spin-up time of the model? Typically,

several days are required to make sure the background dust loading is represented, despite the fact that this can never be entirely assured in regional simulations.”

The spin-up time of the model for background dust is 20 days. This has been included to the manuscript

“Chapter 6, page 23910, line 3: Any reference for that statement?”

This value is not published yet, only presented in a local conference. The authors had since measured absorption and scattering coefficients in Kuwait City over two years and single scattering albedo have been calculated. The assigned value of single scattering albedo is calculated for similar dust loading.

“Chapter 6, same page, line 18: I assume it should read Fig. 13. I also assume that the uppermost height range is between 6-10km, rather than 6-100km?”

The reviewer is correct. This has been corrected.

“Literature (including a non-exhaustive list of other relevant papers which I haven’t mentioned yet):

Ahn et al. 2007, J. of Applied Meteorology and Climatology: Effect of Direct Radiative Forcing of Asian Dust on the Meteorological Fields in East Asia during an Asian Dust Event Period

Heinold et al. 2008, GRL: Dust radiative feedback on Saharan boundary layer dynamics and dust mobilization

Tegen et al. 2010, JGR: Effect of measured surface albedo on modeled Saharan dust solar radiative forcing

Ansmann et al. 2011, Tellus B: Saharan Mineral Dust Experiments SAMUM-1 and SAMUM-2: what have we learned?

Formenti et al. 2011, ACP: Recent progress in understanding physical and chemical

properties of African and Asian mineral dust

Ashpole and Washington 2012, JGR: An automated dust detection using SEVIRI: A multiyear climatology of summertime dustiness in the central and western Sahara

Ginoux et al. 2012, Rev. of Geophysics: Global-scale attribution of anthropogenic and natural dust sources and their emission rates based on MODIS Deep Blue aerosol products

Rashki et al. 2012, Aeolian Res.: Dust storms and their horizontal dust loading in the Sistan region, Iran

Ashpole and Washington 2013, JGR: A new high-resolution central and western Saharan summertime dust source map from automated satellite dust plume tracking"

Notaro et al. 2013, JGR: Trajectory analysis of Saudi Arabian dust storms Rezazadeh et al. 2013, Aeolian Res.: Climatology of the Middle East dust events"

These references have been included in the revised manuscript.

"Valenzuela et al. 2012, ACP: Aerosol radiative forcing during African desert dust events (2005–2010) over Southeastern Spain"

The paper computes mean monthly radiative forcing due to Sahara desert dust over several years. It also compares radiances simulated by SBDART to those obtained by AERONET. It is not directly related to our work since our work focuses on extreme dust loading in the Arabian Peninsula. Therefore it is not included in the revised manuscript.

"Di Sarra et al. 2013, ACP: Estimate of surface direct radiative forcing of desert dust from atmospheric modulation of the aerosol optical depth"

This paper focuses on developing a method to estimate the radiative effect of aerosols in the presence of natural oscillation in the atmosphere. Hence it does not relate closely to our work and therefore is not included in the revised manuscript.

"Spyrou et al. 2013, ACP: Modeling the radiative effects of desert dust on weather and regional climate"

In view to the previous response regarding the application of the upgraded SKIRON model, this reference is not included in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23895, 2013.