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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.

Anonymous Referee #2

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General comment:

Saeed et al. present an interesting case study which, though examined in an earlier publication of the same authors, provides insight into the radiative effects of an intense dust storm. The meteorology which lead to this event is described and its vertical distribution simulated with the SKIRON dust model. Using a radiative transfer model, they try to determine the direct radiative forcing and heating rates due to mineral dust. It is certainly useful an exercise, which would add positively to the existing literature on the subject.

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I do, however, think that the authors of the study should have gone a step further in that they deploy the atmospheric model to test the results of the radiative transfer model. This way, they would have provided essential guidance for the modelling community as to how dust particle properties might be better represented in models, which ultimately improves our confidence in the simulated dust-radiation feedback. While I strongly encourage the authors to expand the study as suggested, the manuscript requires major revisions even in the current form as there are several shortcomings and problems which I am going to address individually below. Moreover, the language needs to be improved.

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. 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).

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"!

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.

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

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NetCDF data to achieve that are (usually) available on the Giovanni web interface: http://disc.sci.gsfc.nasa.gov/giovanni/

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.

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.

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.

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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.

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.

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).

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.

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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)

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

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

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.

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.

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.

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.

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.

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

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?

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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

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

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

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

Notaro et al. 2013, JGR: Trajectory analysis of Saudi Arabian dust storms

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

Rezazadeh et al. 2013, Aeolian Res.: Climatology of the Middle East dust events

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