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Interactive comment on "Direct radiative effects of intense Mediterranean desert dust outbreaks" *by* Antonis Gkikas et al.

Anonymous Referee #3

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The paper presents an interesting study for calculating DRE with the use of the NNMB-MONARCH model (NNMB). It is a well written paper which with the following revisions it could be published in the ACP journal. My main comments are:

- In order to accept the results of such a study, a more comprehensive validation of the presented outputs using real measurements and an analysis of the uncertainties introduces in several phases of the method have to be presented.

- A major aspect of the paper is not clarified. The abstracts talks about DRE and as the authors point out this is mostly aerosol optical depth (AOD) dependent. MODIS retrieves total (dust + other types) AOD while NNMB only dust AOD (that is what is shown throughout the text and in e.g. figure 3). So the authors have to clarify if they talk about Dust DRE or DRE. If someone assumes that these 20 events are purely dust

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Discussion paper

events, an AOD comparison of MODIS AOD and NNMB AODs have to be included (not quantitevily as in fig. 3), in order try to assess the model results.

- A major issue of the paper is the link between the NNMB results and the Gkikas et al., methodology (GM) for identifying dust episodes. Some questions that have to be clarified on the manuscript are the following:

(i) Are the domains seen in figure 3 and 1 have been used in the GM for all the episodes that are presented in the table 1? Is there a mix of surface and sea Modis pixels used?

(ii) When GM identifies an episode (e.g. example of figure 3) are the DRE calculations of NNMB account only the relative (episodic) modis pixels ? I think the answer here is no but it has to be clarified. So, If the answer is no (thus the whole domain (e.g. MSD) is used for NNMB) then the importance of the GM episode identification is only partially valid. (e.g. a lot of white in fig. 3 are used based only on NNMB and not on GM). As identifying an episode in a limited area in the MSD domain does not mean that this is valid for the whole domain.

(iii) If the whole domain is used are results of table 1 dependent in addition to dust AOD to the spatial extension of the event? Can a number of different episodes with different spatial extends and AODs, averaged (table 2)?

Another example of the last point above is that modis GM detects a plume (high AOD) covering very few pixels in the western part of MSD (for example last row of figure 3). Then based on GM the whole MSD domain is considered as the one that will provide the DRE. In this case the link on GM used as a proxy in this work is very week as it covers only a small part of the domain, plus AODs are not compared. So also a number of episodic pixels should be included in these GM dust episode restrictions. Or simply dust outbreak identification can be based on NNMB spatial and NNMB-AOD absolute criteria as now the link with GM is really weak.

In addition, in this case (and others e.g. west domain of fig. 3b) NNMB dust pixels

cover less than 50% of the MSD. When averaging the 20 cases this percentage of pixels varies a lot. In the end you are averaging and provide a result e.g. SW = -9.7. So some of the outbreaks contribute much more and some others not, based on the dust coverage on the MSD only. Where can such statistics be used?

To summarize, if GM is not used for AOD validation and GM identifies as "dust episodic pixels" only a fraction of the pixels used finally from NNMB for calculating dust DRE, then its use becomes not important for this study. So if someone trusts NNMB for DRE calculations, then it is much more easy to trust it also for dust outbreak identification.

- There are more than 100 references and a lot of discussion about aerosol effects and model applications, but very few about NNMB validation on e.g. AOD retrievals. And only one (Ohmura) on BSRN radiation related validation. I think it is more essential to prove the validity of AOD NNMB output (e.g. radiation) and intermediate parameters (e.g. AOD), than a numerous studies cited here, with a very theoretical link to the paper.

- The validation using BSRN is incomplete. In the document and in the abstract you are talking about this validation and 8 stations. Then in the manuscript only one station is shown. And from that only 4 days. In order to validate the results a more comprehensive analysis of long term periods of these 8 stations is needed. Probably Ohmura has answered some of the validation related questions, but this paper focuses on "intense dust outbreaks", and a specific model, so results might differ from the Ohmura related ones.

- There are several issues that have to be clarified/commented on the input parameters of the model:

(i) Optical properties proposed in figure 2. Have been validated ?

(ii) Water vapor, carbon dioxide, ozone, methane and oxygen. Where do you find these inputs ?

(iii) Differences in dust optical properties of Sahara and middle East sources What did you use and how much uncertain are they? and what is the contribution of this uncertainty in the final DRE budget ?

- BSRN and model differences in wavelength integrals of solar radiation. You mention: “These differences might contribute to the level of agreement between model and observations; however, are not discussed in our evaluation analysis”. I think this is an important issue that have to be clearly discussed if a proper validation is included.

- As already mentioned AOD comparisons from MODIS and NNMB could add value to this work. “The model’s ability to reproduce correctly the spatial patterns and values of dust AODs is crucial for a successful computation of the dust DREs, since DREs are determined to a large extent by AOD”. In addition you are mentioning modis uncertainty in section 2. Is this getting high (e.g. $\sim$0.5) for both sea and mostly surface retrievals when you examine AODs in the order of 2-3 based on the table 1? And is this uncertainty already important for such outbreaks for the GM and indirectly for the DRE related uncertainty ?

- Table 2. These statistics are not referring to the model uncertainty but is an averaging of the episodes provided by the GM. NNMB DRE uncertainty is much more useful for any future user of these results. For example a systematic bias can not be identified here. This is also because the GM thresholds are mostly subjective as:

(i) Mean AOD values on dust related areas do not have an important statistical meaning due to the non normal distribution of AOD. It is clear that this is a published work and I have tried to follow the previous work by Gkikas et al and the relative open discussion, describing the method. However, as this is an open public statement I have to comment that AOD does not follow necessarily a normal distribution so using the mean is not absolutely correct. Moreover, dust outbreaks related pixels/locations can be characterized more from a bimodal distribution of AODs when another (than dust) important AOD source is rarely present (e.g. most of the marine grids of Mediterranean

domain).

(ii) GM: By definition high mean AOD values per pixel are closer to dust sources. That makes possible that a pixel with high (in an absolute sense) AOD close to a dust source to be considered non episodic and a pixel with lower AOD, away from the sources to be considered episodic. This is ok, as it is just a matter of definition. But it gets more important when it is used for DRE calculations. So, the latest can be problematic when you calculate DRE in dust outbreaks or filter the outbreaks, as for the first example pixel (high AOD) it is not an outbreak and for the second (lower AOD) it is characterized as an outbreak. The results using this method for DRE calculations become not easily useful and applicable.

- Last but very important, the paper is very long and in various cases the discussion includes a lot of details that in the end confuse the reader on what is the important findings here and which are not. Even for scientists in the field it becomes difficult to read. Authors have to try to reduce the length of the manuscript keeping the important aspects of the results presented. Basically for section 5 I would suggest to try to take out a lot of information that are secondary and to focus on the important results.

Minor comments:

Line 141: it has already mentioned previously.

Line 173: developed – improved

Table 1: episodes = grid cells

The overall approach of this paper is valuable and worth publishing. I strongly believe that after the above revisions, corrections and additional analysis it will be essentially upgraded and then it could be published in the ACP journal.

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