

Interactive comment on “North African dust transport toward the western Mediterranean basin: Atmospheric controls on dust source activation and transport pathways during June–July 2013” by Kerstin Schepanski et al.

Anonymous Referee #1

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Review of: 'North African dust transport toward the western Mediterranean basin: Atmospheric controls on dust source activation and transport pathways during June–July 2013' by K.Schepanski et al.

This article presents an analysis of the tropospheric mineral dust transport from Africa to Western Europe and during the Charmex field campaign of summer 2013. The topic is very important, mineral dust being difficult to measure and to model, but having a large impact on particulate matter concentrations in the troposphere. The use of EOF is an original way to sort multiple and complex meteorological events, combined to

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complex soil and surface properties, leading to huge difficulties to know where and when mineral dust emissions may occur. But several questions remains, which are listed below in the 'Major remarks'. Some other minor remarks are also proposed at the end of this review.

This article may be accepted but after major revisions.

Major remarks: The main concern is the lack of originality of the paper: the use of a statistical analysis is original but the main goal of the paper is not. A lot of papers are already published about this kind of transport and over the Mediterranean. These articles are abundant in the literature (mainly ACP and JGR-atm), including the ACP/AMT Charmex special section. We recommend to the authors to better reference the recent studies and to extract a new way to introduce the results in order to be really original. A suggestion: estimate EOF for several years over the region (using GFS or ECMWF model outputs for example) and characterize the specific year of 2013 in this ensemble. Then, using the already modeled period, conclude if 2013 led to less/more mineral dust from Africa to Europe. This suggestion requires to extend and improve the EOF part of this paper. But I think this could give a real originality of the used approach and to this study.

The second lack is the validation of the modeled meteorology and the mineral dust production model used. It is clear that mineral dust emissions are a combination of 'favorable' meteorology (surface wind speed) and 'favorable' soils and surface (including roughness length, soil humidity, vegetation, topography). The accuracy of the result will be the multiplication of these two large uncertainties. But the modeled wind speed is not validated and the mineral dust production model is an old one, with a large set of uncertainties: the vertical dust flux is tabulated with a constant, the number of bins are low. In addition, the model is regional and applied to a period corresponding to an intensive field campaign: numerous papers are on the ACP/AMT section. Why are they not used?

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The third lack is the differences between the used tools and the goal of the paper. The use of EOF and meteorology at 850hPa is a climatological approach. This provides informations on long-range transport only (and certainly not on surface wind speed, the main engine for mineral dust production). On the other hand, the simulation is carried on for two months only: perhaps a specific case, not representative of general circulations, this has to be evaluated and discussed. This remark may be smoothed by extending the paper as suggested in remark #1.

Minor remarks:

- The abstract is too long. New results must be better highlighted.

This sentence shows the confusion about the meteorological scales in the study: 'The study elaborates the question on the variability of dust transport toward the Mediterranean and Europe in dependence on the atmospheric circulation as a driver for dust emission and a determinant for dust transport routes...'. The atmospheric circulation is not the driver of emissions. It is only a driver for transport, once dust are emitted.

- Introduction: A key point is the well-cited publication of (Shao, 2011). This shows this kind of study was already done. Perhaps the authors may extend the presentation of this publication to better place their own findings. Same remark for the studies of (Moulin et al.): the influence of NAO was deeply studied in these papers and their results could be better presented.

2 Data and methods:

- Definition of wind shear stress could be deleted, being well known. For the model introduction, please add more details on the uncertainties.

- p5.l.5: The alpha constant is not defined. But this could clearly be a very important parameter.

- p5.l.20: If the model is on-line, the shape of the dust (and the related constants) may

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have an effect on AOD but also on direct and indirect effects. Please clarify.

2.2 Validation of simulations using only AOD is frequent for global models. But may appear too simple for regional models. The paper could be improve using more and finest data, especially because the study is linked to an intensive filed campaign.

2.3 Indeed the EOF are designed for long-time period. Please discuss the fact this tool is only used for a short period. What is the representativity of the results in this case. Or think to the suggestion #1 of this review.

3.

This section is interesting, a good bibliography but very long and a mixture of several topics. The first part is close to the introduction (some references are the same) and the second part presents applications of EOF: the topic of section 2. Please simplify and merge these three sections. Results for EOF could be in a new section 3: 'Meteorological validation against measurements and EOF results'.

4.

p.9, l.9: 'Dust source activation...' the concept for dust emissions (meteorology and soil/surface) was already described and cited several times before. The authors may be more synthetic and directly goes to the new results.

5.4 Dust deposition. This is an interesting section, but a validation to existing data is necessary before to conclude with the model only. In particular, the wet scavenging is often roughly designed in the models and the uncertainty is important.

Conclusion: The end of the conclusion is more related to a bibliography. Please focus on your results only.

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