

Interactive comment on “Pivotal role of the North African Dipole Intensity (NAFDI) on alternate Saharan dust export over the North Atlantic and the Mediterranean, and relationship with the Saharan Heat Low and mid-latitude Rossby waves” by E. Cuevas et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper uses re-analysis and satellite data to investigate statistical relationships between aerosol, wind, temperature and geopotential height variations over northern Africa during summer. While the topic itself is interesting as such, I find this work offers rather little to justify publication. The text is extremely long, quite tedious to read and largely confirms things we know already from previous work. The quality of the comprehensive analysis is a little thin in places with no statistical significance of results

C1

provided and some of the index definitions being somewhat arbitrary. The physical interpretations are quite speculative throughout most of the paper. In addition, the structure is not as clear as it should be with many literature results deeply woven into the results section. Therefore, I suggest rejection of this paper. There are ways to bring this to a level sufficient to justify publication, but this would almost mean writing a new paper in my eyes.

MAJOR COMMENTS

1) Length: Everything is unnecessary lengthy about this paper: the title, the abstract, the main text and the conclusions. There are 4 Table and 14 (often multi-panel) figures in the text plus many in the Supplementary Material, which is referred to way too often. In the Introduction you list too many papers for some statements. To get this published, the author should think very hard to concentrate on the essential information and to avoid repetitions.

2) Statistical significance: A lot of statistical results are provided but no significances are given anywhere, not even for simple things such as Pearson correlation coefficients. This needs changing throughout the entire manuscript and only statistical significant results should be discussed in the text.

3) Aim of this work: Until the very end, it has not become entirely clear to me what the purpose of this work really is. I understand that in the end you present a conceptual model of the various elements you have investigated but what is the goal of all this? Is it the dust export you want to understand or something else? Are you trying to build a statistical model that could be used for dust export forecasts even? Are you attempting a critique of existing work on this region (you do in some places but it is never clearly articulated)? In my eyes, this paper needs a very clear aim and then a clear concept to reach it. At the moment it is quite a mixed bag of things and is therefore hard for the reader to digest.

4) Structure: The Introduction needs to concisely summarise the state-of-the-art.

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Based on this, you should explain where you have identified the need for more research or a new approach to justify your work. As it stands, a lot of relevant literature results appear late in the paper in the results section, making it hard to read and understand.

5) Method: I find it somewhat bizarre to introduce a new index in a 2015 paper and now read that the same authors change that index again in this work but keeping the name. This will confuse a lot of people, even before your idea could ever be established. In addition, your choice of index seems quite arbitrary to me. Why Morocco and Nigeria as points? Others before you have used EOF or similar to find patterns but your choice is not very well justified in my eyes. Many pressure dipole patterns are mass seesaws with strongly negative correlations (e.g. NAO). The motivation for seeking no correlation between points is not clear to me.

6) Innovation: Throughout the manuscript you keep stating that your results are consistent with Chauvin, Lavaysse, Roehrig, Rodriguez, Varga and others. So what exactly is the progress we are making with this? Plus there is also a sister paper to this by Garcia et al., which I assume may have some overlap. In fact your NAFDI is so highly correlated with the SHL east and west phases that I really wonder whether we need a new index (and thus paper) at all.

7) Physical interpretation: You claim in various places in the paper that you will provide a physical interpretation, where others have not. Most of what you provide then I find quite speculative and hand-waving with rather little in terms of solid evidence. This pertains for example to dust emission, which is nowhere shown. You often infer causality where there is only statistics. In addition, some interesting questions such as the one why June behaves so differently in many ways than the other two months are not really discussed. Also some of the expressions you use for the physical discussions are a little unclear to me, such as “mesoscale baroclinic processes”. What do you mean here?

C3

8) Comparison of parameters: I think you need to reflect more on how independent the various parameters you show are. For example, when comparing NAFDI composites of MODIS and MACC, you seemed to be surprised that they agree so well, but of course these three are directly related to each other with MODIS being assimilated in MACC and MACC geopotential being used to define NAFDI etc. The geopotential is closely related to wind and temperature etc. It is good that you test consistence between ECMWF and NCEP but then you should stick to one and not use them exchangeably.

MINOR COMMENTS

As I think that this paper needs a thorough re-write, I won't give any more detailed minor comments here. Overall, the paper is well written with acceptable English except of a few odd expressions or minor grammatical errors.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-287, 2016.

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