

May 26, 2016

Authors reply to the Anonymous Referee #2 comment posted on May 21, 2016.

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.

We thank Referee #2 for his/her report. We are happy for his/her positive opinion about the content of the paper and for his/her constructive critical comments will help us to improve substantially the manuscript.

General Comments:

I state at the outset that I am not a meteorologist although I am quite familiar with aspects of African meteorology related to dust events and much of the literature related to dust and meteorological forcing. So my review is as an informed “user” of meteorology in this field of research. That said, there are aspects of this paper which I do not fully understand – but this may be because of my limited background. This paper builds on an earlier paper (Rodriguez et al., ACP, 2015) where they introduced the concept of the North African Dipole (NAFD). They developed an index (NAFDI) that was defined by the relationship of high pressure over the Sahara and low pressures over the tropics. They showed that the large interannual variability in longterm (1987 to 2014) dust concentrations measured at Izaña Observatory (on Tenerife, Canary Islands) could be explained by variations the intensity of the NAFD over the period and that dust export could be related to changes in rainfall patterns and wind fields linked to NAFD. The present paper further examines the role of the NAFD by bringing in a broader range of satellite and meteorological products and by extending the time period of the study. As a result they revise the index that quantifies the North African Dipole Intensity (NAFDI) and show that the Saharan Heat Low (SHL) and mid-latitude Rossby waves play a role in the NAFDI. The paper addresses an important topic - the factors driving the variability of dust transport out of North Africa. There are interesting aspects to this paper especially as they could eventually be linked to climate variability over time. However the paper is difficult to read. It is too long and detailed. I became lost in the many facets of the discussion. This could be due to the fact that I am not a research meteorologist. There are many aspects of the paper that make sense to me but there are others that I do not understand in the context of the topic. My recommendation is that this paper has the potential to make a significant contribution to the field but it will require substantial revisions before it is suitable for publication.

We agree with the referee. We have focused our major efforts to give consistent, rigorous and detailed arguments supporting our findings and it seems clear that we have not optimized the exposure and wording of the objectives and results, reporting them without enough brevity and clarity. We admit the paper is difficult to read, probably because two very different fields

converge in it: dust-aerosols and atmospheric dynamics. However, this fact should not constitute a problem for re-writing the paper in a smoother way we had done. We are making a substantial revision of the way the article has been written, making it shorter and smoothing it significantly (e.g., moving a lot of secondary details of the results to the Supplement).

MAJOR COMMENTS (MC)

MC1. Length: Aside from the readability problems that might be due to my limitations, there is clearly a tendency in this paper to ramble on. An example is the abstract which is far too long and far too detailed. It would discourage many readers before they reached the body of the paper.

Referee #2 agrees with Referee#1 concerning the length of the abstract; and additionally he/she raises the point that the abstract contains too many details. So, we will remove many details and restructure the text to clearly state the major outcomes, reducing it to about half its length.

MC2. Objectives: The background and the objective of the study are not clearly stated. Nor are the conclusions. It should start with the statement of the problem (i.e., the role of dust in climate, the need to understand the response of sources to meteorology and the variability thereof). Then a sentence on the old definition of NAFDI and then address the effort to revise it and why. The section on p 4, line 25 is a more clear statement of what is done. It could be paraphrased in the abstract. The abstract would end with a discussion of results but much simplified from the present discussion which is too detailed and not understandable by the reader without having read the paper.

Thank you for the suggestions. In the new version of the manuscript, the introduction will start with the statement of the problem, and objectives will be clearly stated and motivated in the introduction, as we identify main gaps in the state of art. We agree the objectives presented in page 4, line 25 are not well linked with the present state of the art and the existing gaps. Concerning the abstract, please, see Reply to MC1.

3. Significance: In general, it is difficult to relate the NAFDI to real-world results. How does the NAFDI approach relate to other efforts in this field? Many papers address specific meteorological systems that seem to drive dust events. How does the NAFDI relate to these other approaches? There is much reference to statistical metrics to show that the new NAFDI improves on the old. But it is not clear if the improved statistical significance is of practical "significance". For example, on page 29, line 14: "As a result, the total dust concentrations measured at the Izaña Atmospheric Observatory in August months (from 1987 to 2014) and the NAFDI time series for that period show a better Pearson correlation coefficient between them when using the improved NAFDI (0.72 instead of the value 0.67 that is obtained when

using the original NAFDI definition)." It is not obvious how this improvement is manifested in a larger sense and how this compares with other efforts to characterize dust export. To me, figures such as Fig. 1, 2, 3, 4, 8, 9, etc. are more persuasive than Fig. 11 and 12 for example.

Probably we have not adequately explained the improvement on the criteria used to compute the NAFDI index, giving the impression this improvement is of great importance when it is not. Actually, it is a secondary methodological aspect that refers only to the quantification of NAFDI but it does not affect, or alter at all, the concept of NAFDI established by Rodriguez et al. (2015). In fact, most of section 3.1 will be moved to the Supplementary material.

The new outstanding findings concerning NAFDI, not addressed by Rodriguez et al. (2015), are the following:

- 1) NAFDI shows intra-seasonal variability. So, in the same summer (same year) there can be months with positive and negative phases.
- 2) NAFDI phases drive dust transport over the Mediterranean too, and not only over the Atlantic.
- 3) NAFDI phases modulate the position of the SHL, and therefore some mesoscale processes responsible for dust mobilization in the Sahara which are closely linked to the intensity and position of the SHL. We explain the relationship between NAFDI and SHL providing strong physical arguments and support it with statistical analysis.
- 4) The changes in NAFDI phase, and thus in the SHL phase, are modulated by the Rossby waves (propagating through the North-Atlantic--North-African waveguide) that penetrate deep enough into the lower troposphere.

We will clarify the significance of these results, which explain intra-seasonal variations from months to days in dust transport towards the Atlantic and the Mediterranean, as well as dust mobilization on the Sahara.

Figures 11 and 12 will be moved to the Supplementary material.

MC-4. Conclusions. This section provides some interesting insights. But there is a lot of discussion in this section that should not be a part of "conclusions". Many of the insights are lost in the very long and convoluted text.

We agree. We will remove the long summary and discussion, and the conclusions will be succinctly exposed. The brief summary concerning the conceptual model will be kept.

Summarizing, the substantial revisions (suggested by the Referee and concerning the way the manuscript has been written) we are performing are the following:

1. A new shorter abstract, re-structured and cleared of details, in which main results will be clearly exposed.
2. A new writing of the introduction following the Referee recommendations
3. Most of the content of Section 3.1 will be moved to the Supplementary material.
4. The text, tables and graphs that have to do with methodological details and secondary arguments in sections 3.2, 3.3, and 3.4 will be moved to the Supplementary material or removed.
5. The present “Summary and Conclusions” section will be shortened drastically becoming a “Conclusions” section. Outstanding results will be briefly enumerated with no discussion, and will be complemented with the final conceptual model.

We will upload the new version of the manuscript as an author comment to the Discussion page, within the next few (3-4) weeks (ideally after the Referee #3 comments are posted), to allow further comments of the Referees.