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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Impact of the North Atlantic Oscillation on the variations of aerosol ground levels through local processes over Europe" *by* S. Jerez et al.

**Anonymous Referee #2**

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Referee Comments on the manuscript "Impact of the North Atlantic Oscillation on the variations of aerosol ground levels through local processes over Europe" by S. Jerez et al. submitted to ACPD.

General comments

This work presents an attempt to characterize the variations of aerosol ground levels over Europe caused by local processes and NAO patterns. In terms of scientific quality and significance, the subject of this paper is of scientific interest but the presentation of the work needs a number of revisions to make it suitable for publication with ACP. My



main concern is focused on the following issues: the evaluation of the model results that the authors state it will be part of another paper, the use of NAO index from CPC (NOAA) without describing how it relates to the ECMWF data used for the MM5 that drive the air quality simulations and the lack of specific and detailed information in several parts of the text. For example, how can the authors discuss about the NAO patterns without including the Atlantic Ocean in their modeling domain?

The title of the manuscript reflects part of the contents of the paper, but since the results are based on non-realistic model simulations, I would suggest a slight change in the title: "Impact of the North Atlantic Oscillation on the variations of aerosol ground levels through multiannual model simulations over Europe using fixed anthropogenic emissions"

The specific comments that follow will help to clarify important issues and suggest a number of changes to be made in the text. My suggestion for publishing this work with Atmospheric Chemistry and Physics is to reconsider after the major revision comments have been addressed.

Specific comments

Abstract: I cannot see how the objectives described in the last sentence are met in the work presented here. How can we improve the predictability of climate-air quality interactions based on the findings of this work? This is not supported by the conclusions. I suggest that the abstract follows the conclusions more carefully and avoid using statements that cannot be substantiated.

Introduction:

1. Page 13891, line15: The influence of radiation and temperature on gas-phase chemistry has been studied by numerous researchers worldwide. The authors should cite here more publications besides Katragkou et al. (2010).

2. Page13892, line 29: How did the authors disregard the contribution from the large

scale transport mechanisms? This is not adequately explained in the text. And why would they want to disregard a physical mechanism that strengthens the realistic representation of the atmospheric conditions?

Section 2:

1. Page 13893: How can the authors discuss the NAO patterns without including the Atlantic Ocean in the modelling domain (only a small part)? Is the MM5 domain different from the one we see in Figs3-5? This should have been explained in the text.

2. Page 13894, lines 5-9: The authors have used meteorological fields with coarse spatial resolution of 90km and these meteo fields have been interpolated to 0.2deg for the air quality model simulations. Even though the present work covers the European continent and most part of the Mediterranean Sea, the authors refer to publications on the Iberian Peninsula for the discussion on the skills of the modelling systems. This is quite misleading, as the basic question that arises from this part is how a 90km horizontal resolution can give reasonable results on rainfall patterns for the entire European continent. In addition, there is no mention in the text if the model setup was exactly the same as in the 2 cited publications. I believe that all the above present a very weak point of the presented work. The authors have not thoroughly evaluated any part of their simulations (meteorological or air quality fields) and they state that this is part of an on-going paper. Yet, the results and conclusions of this paper depend entirely on the performance of the modelling systems, both for the NAO pattern and the atmospheric pollutants concentration.

3. Page 13894, lines 17-22: What is the exact meaning of this sentence? That the long-range transport is disregarded because of the climatological boundary conditions? If this is the case, I have to express my disagreement with this statement. Long-range transport occurs when atmospheric pollutants travel thousands of km away from their sources, i.e from N-Europe to N-Africa, and the domain shown in the figures can include part of such transport mechanisms. Especially, the last sentence has to be con-



sidered erroneous “the experimental design allows to better isolate and understand the role of the local processes, including the pollutants transport between different areas within our domain”. I would suggest to clarify what is the meaning of “local processes” when simulating atmospheric pollutants in such coarse domain, since pollutant transport and transformation is included by default.

4. Page 13895, lines 1-5: How does the model produces wind-blown dust and resuspended dust? This part is missing from the text. Especially since the African deserts are outside the modeling domain, I am not sure of the origin of the wind-blown dust that is shown in the results. A proper discussion should be included in the text.

5. Page 13895, lines 6-7: The model-observation comparison has been performed using monthly, weekly, daily or hourly data? This information is not included in the text and it is important to understand how the correlation is above 0.7 most of the times. The authors should also consider giving the bias (not the standard deviation as in Fig.1). If the results are based on monthly PM10 and PM2.5 data, then there is no information on whether the model can capture the variability of the observations, since the lifetime of most aerosol species is a few days up to maximum one week. In that case, it should be made clear in the abstract and introduction that this is a seasonal analysis of the NAO impact. The 20 years simulation gives an overwhelming amount of data to handle, but I believe a more proper comparison should be included in the paper to convince the readers of the validity of the approach. Including the evaluation of the modelling approach in another publication when the model results support the findings in this work, is not appropriate.

6. Section 2.2 is quite small in length and it does not need to be a separate sub-section. This can be part of section 2.1 (which will be named section 2). In this paragraph the authors say that they isolate the influence of climate variability on air quality by keeping the anthropogenic emissions fixed for 2005 during the 30-year run. Please explain the reason for choosing the year 2005 in the text. Since the climate variability is affected to a large extent by the feedback mechanisms between air quality and atmospheric

conditions, it seems that this setup does not correspond to the real atmospheric conditions. It is rather a sensitivity model experiment that tests the model response (fixed anthropogenic influence) to NAO patterns. This should be clearly described in the text.

Section 3:

Page13896, lines 23-25: The authors are using the NAO index provided by CPC (NOAA) and the use ERA40 or ECMWF analysis fields for the MM5 simulations. How well the 2 systems relate when it comes to the calculation of the NAO index? If the two datasets give very different indices then the results from this work cannot be justified as they compare NAO patterns that do not relate to the air quality simulations. This is a very important part of this work and should be handled with caution in section 3.

Section 4:

1. Page 13898, line 1: The phrase "essentially to evaluate the ability of our climate simulation" is not supported anywhere in this section. How is this evaluation performed? There is no comparison with observed or measured values and the above statement is not appropriate. There is no evidence that the differences shown in Fig.3 are representative of the actual atmospheric conditions. This is also where one of the main questions arise again: the NAO phases are calculated with the NOAA index but the atmospheric simulation is driven by ECMWF data. Are these comparable?

2. Page 13900, lines5-7: How is the enhanced DUST concentration in the Iberian peninsula (Fig.4c) related to the precipitation in the same area (Fig. 3e)? Please be more descriptive on the analysis of the results in cases like this one.

3. Page 13901, line16: The SOA levels are shown in Fig.5f not 5d.

Conclusions: The results from this work, as discussed in this section, state that the aerosol concentrations are influenced by the changes in precipitation, temperature and wind fields. This is a result already known from the physics and chemistry of the underlying processes, without the need to perform a 30-year model simulation. I suggest that



the authors focus their conclusions on the new findings of their work that are associated with the NAO phases and impacts on the European continent and the differences therein.

Figures 3 to 5: The quality of these figures that present the main findings of this work is not acceptable. It is very difficult to see the details in each plot and see how the text is supported by the figures. The authors should leave 4-6 panels (maximum) in each figure and make sure that the details are easily discernible.

Technical corrections

1. Please replace "non-antropogenic: with "non-anthropogenic" everywhere in the text.

2. Abstract, line 13: replace the word "rebounds" with "influences" or "affects".

3. Abstract, line 16: please rephrase the part that reads "of this later" as the meaning is not clear. What is this later?

4. Introduction, p13891, line 7: please replace the word "paramount" with a more modest one.

5. Section 2.1, page 13893: The sentence "This resolution enhances from previous works. . .." must be rephrased as the verb "enhance" is not appropriate.

6. Page13895, line26: Please rephrase the "it arises mandatory" with "it becomes mandatory".

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