

Interactive comment on “Model simulation of ammonium and nitrate aerosols distribution in the Euro-Mediterranean region and their radiative and climatic effects over 1979–2016” by Thomas Drugé et al.

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This is an interesting regional modelling study which investigates the simulated ammonium and nitrate aerosols distribution over Europe and Mediterranean region and their radiative and climatic effects over the period 1979-2016. In general, it is well written and structured and there are original model results presented and discussed. Please see below a few comments that have to be taken into consideration before acceptance of the manuscript for publication.

I would like to thank the anonymous referee for his comments which mention different

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points listed below.

Page 3, lines 3-4: Recent studies indicate that dust may play a very important role in nitrate formation, e.g. Karydis et al. (ACP, 2017) suggest that the tropospheric burden of aerosol nitrate increases by 44% when interactions of nitrate with mineral dust are considered.

A sentence has been added to complete the introduction and mention this article (section 1).

The authors write that in the present simulations, aerosols are not included in the lateral boundary forcing because the domain is supposed to be large enough to include all the sources of aerosols affecting the Mediterranean region. In this case Mediterranean region could be affected by dust aerosols advected from Sahara and Middle East. Please comment if the dust source regions are within the selected domain. A similar issue for the selection of the domain and extension south of the Sahara is discussed by Tsikerdekis et al. (ACP, 2017) in order to reduce the contribution of dust aerosols from outside the domain.

The area is large enough to integrate the two main sources of dust, which are the Sahara (more precisely the Bodélé and the area covering eastern Mauritania, western Mali and southern Algeria) and the largest part of the Arabian Peninsula. The dust sources further south are not taken into account because they have no impact over the studied area (e.g. Moulin et al., 1998).

The authors use 3 dust bins (0.01 to 1.0, 1.0 to 2.5 and 2.5 to 20) for their experiments. Foret et al. (JGR, 2006) suggests that dust size for the transported bins should range from 0.09 to 63, considering both the total number and the mass distribution of soil particles. Please briefly discuss how sensitive could be the model results with the number and range of size bins?

We only use 3 dust bins to keep a model with a relatively low computational cost. We are aware that this is a limit for our study. Indeed, Foret et al. (JGR, 2006) indicate that dust size for the transported bins should range from 0.09 to 63 and at least eight size bins are necessary to secure an 8% accuracy on the total suspended dust mass after two days of transport. This limit must therefore be taken into account. On the other hand, our model approaches the LMDz-INCA (from which our nitrate scheme originates) in term of number of bin because they also use 3 ranges (<1 , 1-10 and >10. A sentence has been added in the article to inform of this limit (section 2.2.3).

Please provide some references for the selection and use of the proposed values on the deposition velocities?

For fine nitrates, as Hauglustaine et al., 2014, we used deposition values close to sulphates, already used in the model. For coarse nitrates, the values close to coarse dust and sea-salt aerosols have been used. All data are detailed in Michou et al. 2015 (Table 1). The reference has been added to the article (section 2.2.4).

Page 8, lines 30-31: The authors may also consider a recent article that focuses on the differences between the MODIS Collection 6 and 5.1 aerosol datasets over the greater Mediterranean region (Georgoulias et al., Atmos. Env., 2016). Details about the different MODIS algorithms and the corresponding uncertainties can be found there. The authors may consider to replace ± 0.03 with the values given there for each algorithm separately and also mention that they use the combined DT+DB aerosol product.

These details and this reference have been added to the article (section 3.1).

Page 15, lines 33-34: The authors mention that “An interesting point is that the nitrate AOD550 rise is not due to an increase in its precursors (ammonia and nitric acid).” It might be interesting to compare maps of nitrate trends per decade with recent

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satellite-based trends on tropospheric NO₂ which show negative trends during the last 10-20 years (e.g. Hilboll et al., ACP, 2013; Georgoulias et al., ACPD, 2018) over the western part of the Euro-Mediterranean domain and positive over the eastern part.

It is indeed interesting to notice these trends in NO₂, but a detailed study of the trends of this species is out of the scope of this paper. However a sentence mentioning the decline in tropospheric NO₂ over the western part of the Euro-Mediterranean domain and these two references have been added to the article (section 4.6).

The authors refer to article of Zanis (2009). However, there is also a more robust study with a 12-years simulation (Zanis et al., Climate Research, 2012) indicating a limited direct shortwave effect of anthropogenic aerosols (carbonaceous and sulphate) on the regional European climate with the greatest negative temperature difference of -0.2C over the Balkan Peninsula.

This result and the reference were added to the article (section 5.2).

An issue that it is not discussed at all is if the aerosol induced signal on the temperature fields is higher than the model's internal variability. I think at least a few comments on this issue are necessary. This is also a part of the limitations in such simulations.

A t-test has been done using the 38 years of the simulation with a significant level of 95%. Furthermore, areas with high temperature differences are areas with high A&N AOD so it is unlikely to be due to model's internal variability. These details have been added to the article (section 5.2).

The author write in line 11 of page 3 "Nevertheless, the predicted trend in surface nitrate ..." Does this refer to particulate nitrate?

Here, this refer to nitrate concentration at the surface. A clarification has been brought to the text (section 1).

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Technical comments 1) Page 2, lines 21-23: Please consider rephrasing. e.g. “After the small particles reach equilibrium ...” 2) Page 5, line 24: Please rephrase, accordingly. I guess the authors mean that there is no change from year to year in the annual cycle of HNO₃. 3) The equations should be numbered within the text. 4) Page 7, line 9: [NH₄] = TA - [NH₃]. In accordance with the previous notation NH₄ should be NH₄ cations. 5) Page 8, line 12-13: Please add the units for the values 4.3, 34.9, 4.8 and 34.2. 6) Page 10, line 31: I think it should be Figure 4 instead of Figure 2. 7) Page 12, line 4: It should rather read “... some biases are identified ...” instead of “... some bias are identified ...” 8) Page 12, line 18: I think it should be Figure 6 instead of Figure 4.

Done.

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