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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future evolution of aerosols and implications for climate change in the Euro-Mediterranean region" *by* Thomas Drugé et al.

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We would like to thank the two anonymous referees for their comments mentioning different points listed below.

Anonymous Referee 2

The paper analyses the differences in aerosol load and DRF between the end of the 20th century (1971-2000) and the mid-21st century (2020-2050) in the Euro-

Mediterranean region using a regional climate model (CNRM-ALADIN63) coupled to the TACTIC (Tropospheric Aerosols for ClimaTe In CNRM) interactive aerosol scheme and driven by the global CNRM-ESM2-1 Earth System Model (used in CMIP6). The study reports the already well known decrease in sulfate and increase in nitrate between the two periods, estimating a DRF decrease of 2.6 W/m2 and increase of 1.4W/m2 respectively. The study also concludes that the extra-warming attributable to the anthropogenic aerosols evolution over Central Europe and the Iberian Peninsula (0.2âŮęC) during the summer period is due to "aerosol-radiation" as well as "aerosol-cloud"interactions processes. The study is interesting and deserves publication but I have some general comments that should be convincingly addressed before a final decision on the manuscript is made:

1- The title: I believe the title is slightly misleading. The study adresses the implications of the aerosol evolution upon climate change in the Euro-Mediterranean region using one regional model driven by one ESM. I also note that, despite the higher resolution of the model compared to most ESMâĂšs, it includes several simplifications (omission of processes and simplified schemes) that do not necessarily represent the state-of-the-art when it comes to the interaction of aerosols with radiation and clouds. I believe the title should clearly reflect that the study focuses in one model (therefore implicitly conveying the more correct message that the results may be to some extent model- and assumption-dependent). As it stands the reader may expect a multi-model study including uncertainties, which is not the case. For example: “Future evolution of aerosols and implications for climate change in the Euro-Mediterranean region using the CNRM-ALADIN63 regional climate model” or something similar would be more appropriate.
Indicate in the title the name of the model is indeed more appropriate. The title has therefore been changed.

2- I think it is important that the authors nuance a bit more their statements in the introduction about the study of aerosol effects with a regional climate model. While resolution is important, the resolution used is not far from what some ESM models are already using. More importantly, there are other aspects that are key to understand aerosol impacts and at present many ESMs already include aerosol and cloud processes that are far more advanced than the ones represented in this study. The regional model used here includes a simplified aerosol scheme (without aerosol microphysics and number, for example), and many other simplifications (like introducing only the first indirect effect or using a constant nitric acid climatology). Besides that, regional climate modeling cannot account for slow climate responses to aerosols in the domain, and in fact heavily depends upon the ESM driver through the boundaries. I think these aspects and the associated limitations should be clearly explained and discussed both in the introduction and the discussion.

Effectively, the resolution used here is not far from what some ESM models are already using and the representation of the first indirect effect in our model is rather simplified. On the other hand, this simplified representation of the first indirect effect is still used in about 50% of global climate models and the representation of the direct aerosols effect in our model is rather realistic. The limitations of this study (constant nitric acid climatology and simplified indirect aerosol effect) are discussed in the conclusion. The use of a constant nitric acid climatology, which is a major limitation of the study, is now clearly indicated in the text (Page 5, lines 30-34) and in the abstract (Page 1, lines 12-14). Moreover, in this study, the aerosols effects on SST are effectively not taken into account and we therefore focus mainly on fast climate responses to aerosol forcings (Page 4, lines 18-19).

3- Nitric acid is implemented in the model as a constant monthly climatology based on the CAMS reanalysis. One key result reflected in the abstract is the increase in nitrate and its impact upon the DRF. At least, the abstract should clearly acknowledge the constant nitric acid assumption.

This assumption has clearly been added in the abstract (Page 1, lines 12-14). For information, Figure Appendix B1 (will not be shown in the article) presents the HNO_3 evolution in 4 CMIP6 models and in the CAMS reanalysis. There is indeed a HNO_3 decrease in the future period in the lower layers (925 and 850 hPa). On the other hand, at 750 hPa this decrease is less visible. Moreover, given the differences between the models, the uncertainty remains relatively high. During the nitrate aerosols implementation in the TACTIC aerosol scheme, we also performed a sensitivity test to evaluate the impacts of a time-dependent or a constant nitric acid climatology. Figure Appendix B2 (will not be shown in the article) shows that, in our model, the nitrate concentration is relatively little impacted by the use of a constant or time-dependent nitric acid climatology. For this reason, we decided to use a constant nitric acid climatology in this version of the model (page 5, lines 30-33). Nevertheless, the implementation of a time-dependent nitric acid climatology is envisaged in a future version of the TACTIC aerosol scheme (page 17, lines 5-6).

4- In page 6 you state: “The future period has been selected in the near future because, unlike greenhouse gases, the most important aerosol change is up to the middle of the century. Moreover, the near future horizon period is most suitable to help public decision-makers.” I agree with this statement. However, I find that the selection of the reference period (1971-2000) is not well justified and may even been a bit inconsistent with the argument that “is most suitable to help public decision makers”. Why the reference period is 1971-2000. Is it because of sulfate and the associated large signal in the DRF? WouldnâĂŹt policy makers prefer to see the differences between mid-century and the present day?

Indeed it is not the best choice to help public decision makers but we have chosen this period because it is a reference period commonly used (page 6, line 17).

5- Given that you are using a regional climate model driven by an ESM with a similar

(although not identical) aerosol scheme, they should be compared. In fact, average results of the DRF are compared to other studies throughout the paper but I really miss a clear and consistent comparison with the parent ESM. This could also respond to the question: how useful is resolution and downscaling for the diagnostics that are discussed in the paper? It would be even better if other available CMIP6 ESM results could be consistently compared within exactly the same domain. That would provide a solid comparison reference.
A figure showing the comparison between the RCM and the driving ESM for AOD and surface SW DRF has been added in Appendix (Figure A1 and Figure A2). The forcing model also shows a similar AOD and surface SW DRF trends over Europe and the Mediterranean Sea (page 8, lines 27-29 and page 11, lines 31-34). However, we can observe a surface SW DRF decrease more pronounced with the CNRM-ESM2-1 global model due to the fact that nitrate and ammonium aerosols are not taken into account in this model (page 11, lines 33-34). A comparison with other CMIP6 ESM would indeed be very interesting, but is outside of the scope of this work.

6- Please clearly state that you are calculating the instantaneous RF. Current practice is to calculate effective radiative forcing which includes the forcing and the fast response. Also, why only the direct radiative forcing is discussed in section 3.2 and not the indirect effect? What is the relative role of both upon the total forcing? This is particularly strange as in section 4 you discuss on the interaction with clouds and you suggest influences of the indirect effect upon the certain results.
We have now clearly specified that we calculate the instantaneous RF in section 3.2. (page 10, line 26). Studying the indirect radiative effect is not the main objective of the article because it is not fully taken into account (only the first indirect effect and nitrate particles are not yet included). The primary objective of this study was therefore to focus on the direct radiative forcing of aerosols. On the other hand, the cloud radiative forcing and the forcing due to semi-direct aerosol effects have nevertheless been studied in some interesting regions (central Europe, Iberian peninsula) in a qualitative way

with the different mechanisms involved (sections 4.2 and 4.3). In addition, to complete this study, we have now added a figure presenting the effective radiative forcing (ERF), at the Top Of Atmosphere (TOA), due to aerosol-radiation interactions (ERFari, Figure 13 (d)) and to aerosol-cloud interactions (ERFaci, Figure 14 (b)). These figures are discussed in the text (page 1, lines 22 and 24, page 15, lines 1-4 and page 15 lines 26-27).

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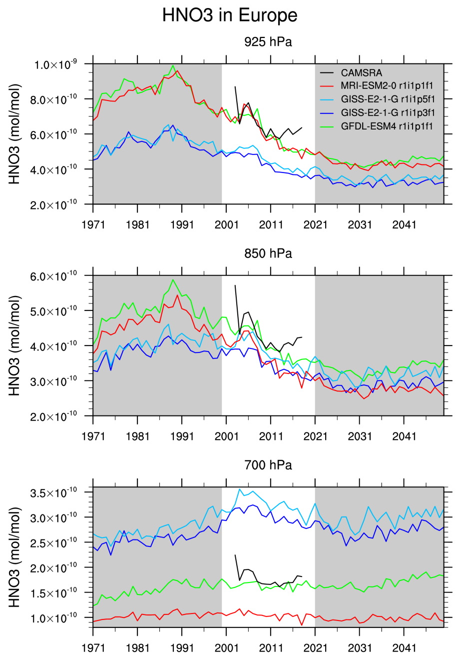

Fig. 1. B1 HNO3 evolution in 4 CMIP6 models and in the CAMS reanalysis over the period 1971-2050 at 925, 850 and 700 hPa (not shown in the article)

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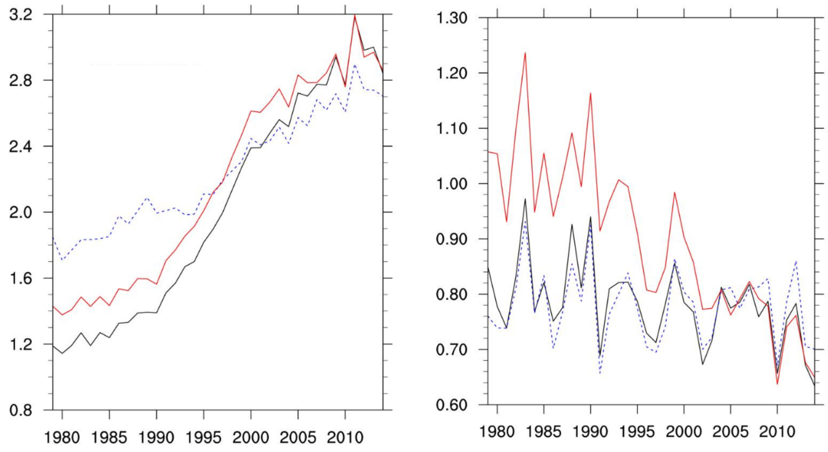

Fig. 2. B2 Nitrate concentration evolution (fine bin on the left and coarse bin in the right) over Europe using a time-dependent (red) or constant (black) hno3 climatology. The blue dotted line was used for a

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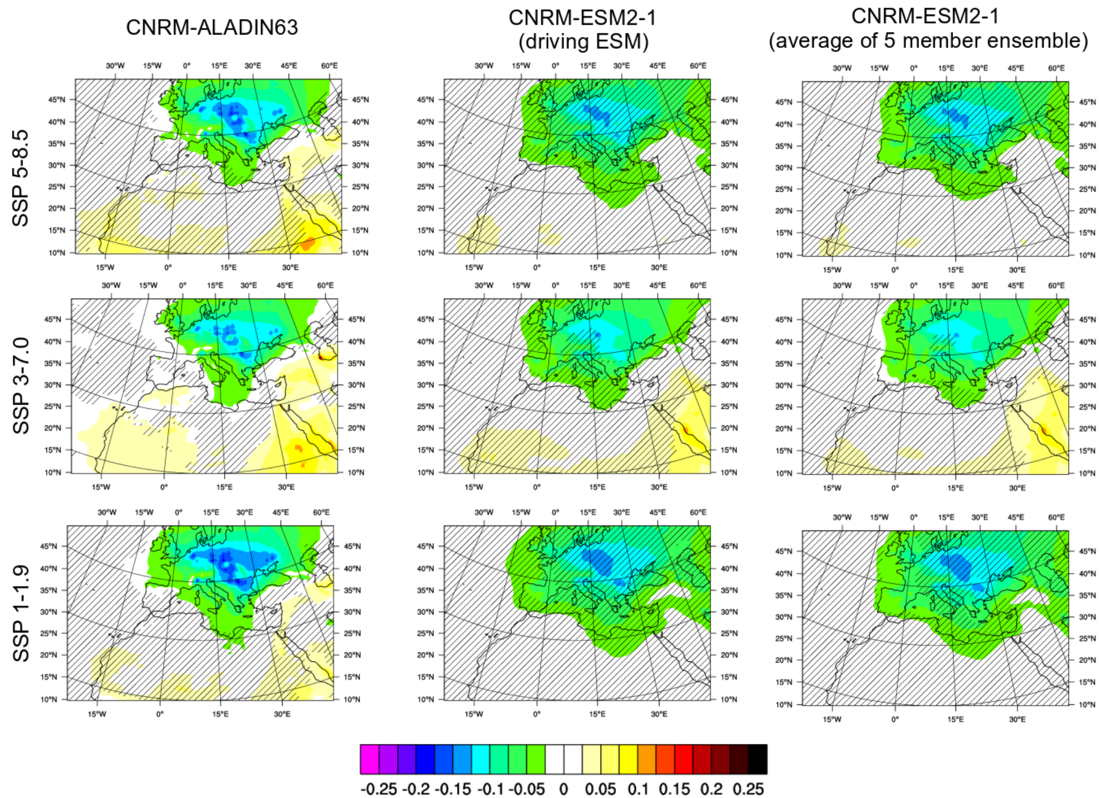

Fig. 3. A1 Total AOD evolution between the past period (1971-2000) and the future period (2021-2050) with the CNRM-ALADIN63 model and its driving ESM.

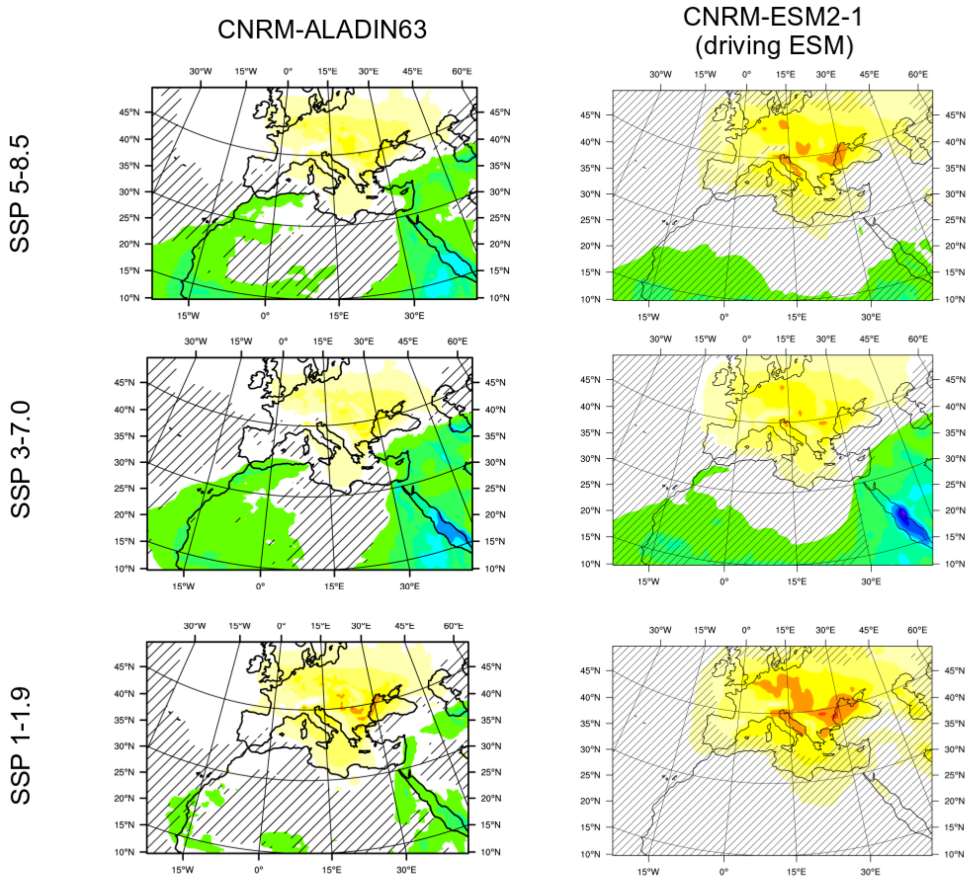

Fig. 4. A2 Surface SW DRF evolution between the past period (1971-2000) and the future period (2021-2050) with the CNRM-ALADIN63 model and its driving ESM.

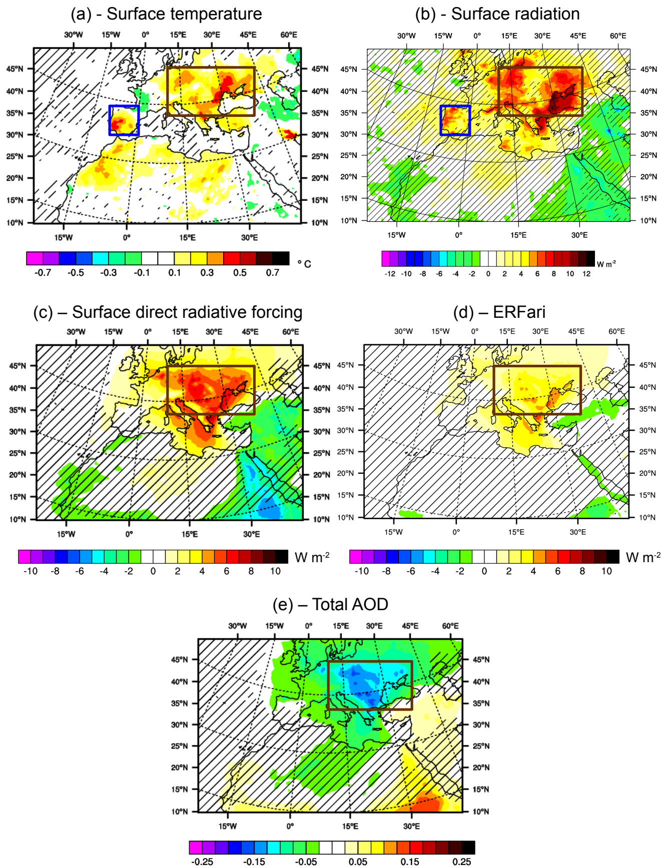

Fig. 5. Figure 13 Mean differences, for the months of June, July and August, between SSP585 and SSP585cst simulations over the period 2021-2050 for different parameters

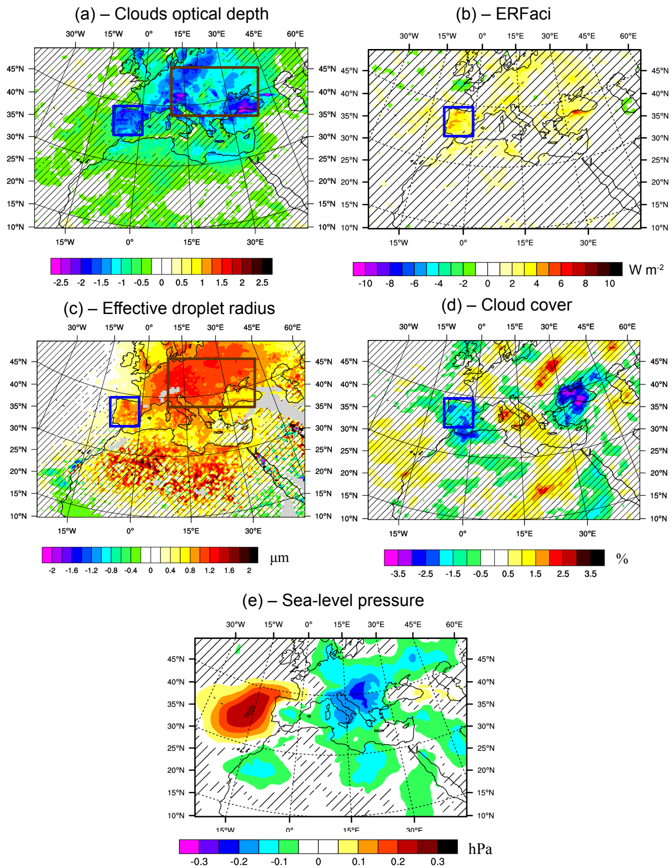

Fig. 6. Figure 14 Mean differences, for the months of June, July and August, between SSP585 and SSP585cst simulations over the period 2021-2050 for different parameters

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