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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol chemistry above an extended Archipelago of the Eastern Mediterranean basin during strong northern winds" *by* E. Athanasopoulou et al.

Anonymous Referee #1

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The contribution by Athanasopoulou et al., entitled "Aerosol chemistry above an extended Archipelago of the Eastern Mediterranean basin during strong northern winds" presents a sound study of the aerosol concentrations and composition in this target domain, combining modeling and a large dataset of observation of the chemical composition of aerosols in the Aegean Sea. Generally, the paper is well written, the overall structure is clear and easy to follow for the reader and the number and quality of the references appropriate.

Moreover, the paper addresses relevant scientific questions and uses a comprehensive database to assess aerosol-related air quality in the Eastern Mediterranean domain.

Despite the methodologies are not very innovative, the amount of information used and the analysis of results done make this contribution very valuable for the scientific community. However, several aspects should be addressed before publication.

The authors state (page 9359, line 10) that the relation between meteorology and aerosol load "is not extensively studied" over the Eastern Mediterranean. I must somehow disagree. The authors cite at least four works covering this topic, but there is a number of modeling works covering the topic. So, I would rephrase the sentence, but if the authors want to keep the sentence as it is, they must provide stronger arguments for this fact.

Also, they state (Page 9360, last paragraph): "[...] providing the first extensive evaluation of aerosol simulation performance over a wide region of the Mediterranean". I also believe that this sentence is not correct; again, a number of works have provided extensive evaluations of modeling performance over the Western and Eastern Mediterranean. This sentence is completely unnecessary in this context and should probably be removed from the manuscript.

In Page 9363, line 22, the authors comment: "All air quality predictions presented [...]", which leads to think that the modeling results are forecasted outputs. However, the WRF model is driven by a reanalysis form NCEP, and therefore regional results are not forecasted, but driven by a reanalysis. Probably the authors should change the word "predictions" in order to avoid further confusion.

For the type of evaluation done in this work, having a detailed resolution for the vertical layering is an essential issue. I have doubts about the vertical resolution used. The authors indicate that they use just 14 model layers up to 5.8 km to cover the troposphere with the 1st layer at 21 meters (Page 9365, line 18), but do not provide further information on the vertical coordinates of the model. Further discussion should be devoted to this issue in the manuscript.

In Page 9372, line 26, the authors indicate that "sulfate model performance is not

consistent throughout the troposphere". The authors should elaborate on the causes of this discrepancy. Is it just a consequence of the low concentrations leading to a high value of the statistical figure considered, or is there any other cause for this vertical gradient of the error?

In Page 9376, line 10, the manuscript claims that "fire activity is the main deficiency in the current model application with respect to organic aerosols". This sentence has to be carefully considered. First, the authors should provide evidence for the fire activity (e.g. satellite images) in the target region during the August-September 2011 episode. Moreover, if the fires were causing under predictions in particles levels, a substantial underestimation should be also observed for black carbon (which is not observed in this study). The authors should also explore the VBS mechanism for the formation of SOA, which could be causing the underprediction of organic aerosols.

Minor comments:

Abstract, line 1; Page 9360, line 26: What is exactly a "carefully designed model system"? How can a model system be designed "carefully"?

Page 9363, line 14: please change "nitrogen oxide" to "nitrogen oxides"

Figures 2 and 6 are hard to read; the quality of the figures must be improved before publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9355, 2015.