

Interactive comment on “Air quality modelling in the summer over the Eastern Mediterranean using WRF/Chem: Chemistry and aerosol mechanisms intercomparison” by George K. Georgiou et al.

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We would like to thank the referee for the constructive comments and suggestions on how to improve the manuscript. Please find our replies to all comments and the supplement with all additional images below.

1) page 2, lines 11-13: It is not actually the Azores High pressure system the anticyclonic center of action that results to the etesian winds in combination with the Asian low. Many researchers underline the differences between the anticyclonic center causing the Etesians and the Azores permanent Anticyclone (Prezerakos 1984; Tyrllis et al. 2013; Anagnostopoulou et al. 2014) because unlike the Azores anticyclone, the ridge

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over the Balkans retains its distinct signature up to 500 hPa implying different dynamics involved in its formation.

Changed and relevant references added.

2) page 2, lines 19-21: There is also a recent study pointing further the role of tropopause folds in summertime tropospheric ozone over the eastern Mediterranean and the Middle East (see Akritidis et al., 2016).

We have added a reference to the study in our discussion about the tropopause folds in summertime ozone over the eastern Mediterranean and the Middle East.

3) page 6, line 12: Since the emissions are available at 0.1 deg what is the treatment of the emissions at the finest grid resolution of 4 km;

Anthropogenic emissions were interpolated in space and time to produce daily emissions using the anthro_emiss utility (<https://www2.acom.ucar.edu/wrf-chem/wrf-chem-tools-community>) which creates WRF gridded anthropogenic emission files from lat/lon gridded anthropogenic emission files.

4) Section 2.3: Please specify the chemical measurements carried out at the air quality stations.

The measurements carried out at each station are now shown on Table 2.

5) page 6, line 30: Please specify if the bias refers to ozone measurements carried out at the five air quality stations of Table 2.

We clarified in the manuscript that the bias refers to the five air quality stations of Table 2 and the CYPHEX campaign.

6) page7, line 31: The authors mention ozone overestimation due to the effect of boundary conditions from the global MOZART-4 model (Abadallah et al., 2016). Similar results have been reported in an earlier study implementing chemical boundary from another version of MOZART model showing the importance of time variant chemical

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boundary conditions for simulated near surface ozone O₃ (Akritidis et al., 2013).

We have added a reference to the study in our discussion about the effects of boundary conditions on regional air quality modeling.

7) Table 3 and 4: I think a more detailed description of the Table captions is needed. Specify also the number of the data used to extract the statistical measures. Furthermore I think that the statistical measures should be given for the individual stations, too (even as supplementary material) because one added value of this simulation is the high resolution. If discussion is based on averages of all different stations together, practically the advantage of the high resolution analysis is diminished.

A more detailed description has been added to the Table captions. Captions also contain the availability of data used to extract the statistical values at each station. Statistical measures for individual stations for meteorology have been added in Table S1 in the Supplement.

8) How the statistical evaluation measures of this study in Table 4 compare with other similar modelling studies? The correlations based in hourly data are very low for NO_x, CO and PM and slightly better but still low for O₃. It seems that the day to day variability is not captured adequately. You may check also how model simulates the diurnal variation by comparing the mean modelled diurnal cycles with the observed ones.

Air quality modelling studies over the Eastern Mediterranean in the literature mainly focus on O₃. During the second phase of the Air Quality Model Evaluation International Initiative (AQMEII), the majority of the modelling groups using the RMS and CM mechanisms with the WRF/Chem model also reported O₃ concentrations overestimation over the Eastern Mediterranean (Im et al., 2015). On the contrary, Mar et al., (2016) reported an underestimation of about 5ppbv in summertime O₃ concentrations WRF/Chem model using the RMS mechanism. Hourly Correlation Coefficients for O₃ are comparable to Mar et al., (2016) ($R \approx 0.2$ at the Ayia Marina station during summertime). The comparison of observed diurnal cycles for O₃ and NO_x has been added

in the supplement (Figures S2 and S3 respectively). The fact that no pronounced diurnal cycle is shown on observational data either (except NO_x at the Stavrovouni station which is located close to the highway) indicates that long-range transport is an important aspect of air quality over Cyprus.

9) Page 11, line 15: It is mentioned for the CYPHEX campaign stations that “ It appears that the measurement site, due to its elevation of about 650m asl, was regularly influenced from the lower free troposphere with elevated ozone concentrations. . . ” . However the closest Ineia Village stations is also at a similar altitude but does not show such high values. Maybe it would be a good idea to compare directly the ozone time series of these two stations. When looking these two ozone time series in Figure 6, I get the impression that there is a co-variability but CYPHEX ozone is constantly higher than Ineia ozone.

We agree with the observation by the reviewer and the sentence is now removed. Regarding the ozone concentrations at the CYPHEX Campaign, they are indeed constantly higher than at the Ineia station. A direct comparison between these two measurements sites has been added in the supplement (Figure S1).

10) As the authors mention, Table 4 indicates an overestimation for modelled ozone and an underestimation for modelled NO_x. I think it would be interesting to see how the global O₃ and NO_x values compare with the observations and also how the statistical measures are being modified as we go from the coarse resolution to the fine resolution of the inner domain. For example, it is important to show if the finer resolution improves the evaluation measures.

We have added the scatter plots of the observed and modelled O₃ and NO_x concentrations from the global MOZART-4 model and the three domains of the WRF/Chem model for each simulation in the supplement (Figure S4) . The corresponding statistical measures are also shown on Table S2 in the supplement. The global model overall simulates NO_x concentrations more accurately. This is attributed to the emis-

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sion inventory used, as stated in the manuscript. There is a substantial improvement on simulated O₃ concentrations when moving from the global model to the outermost domain of the WRF/Chem model (NMB reduced by 26% for the RMS mechanism and by 11% and 13% for the MM and CM mechanisms respectively). A further improvement of the order of 3% is shown when moving from the coarse to the finer WRF/Chem domain on O₃ statistical measures.

11) Page 12, line 5: What do the author mean with “similar patterns appear for CO” in Figure 8? I guess the authors similar to NO_x precursors since as I can from Figure 8, CO is underestimated by the model before and after the 13th of July.

We clarified in the manuscript that we mean that the three mechanisms show similar behaviour from July 1st to July 13th for CO as well, but the RMS mechanism gives higher CO concentrations for the rest of the simulation period.

12) Since there is a lot of discussion in the manuscript for the role of dust aerosols in the simulation after 11 July (Figures 3 and 9) maybe it would be interesting to see the evolution of simulated dust aerosol optical depth and compare with observed values from available ground based or satellite relevant measurements. This is rather a suggestion than a request for the revision.

This is a very good suggestion by the referee. We are planning a follow-up paper focusing on aerosols in which simulated aerosols and their properties over the region of study will be examined in more detail and over a longer time span to capture seasonality.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2017-781/acp-2017-781-AC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-781>, 2017.