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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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This manuscript evaluates the online coupled chemistry transport meso-scale model WRF-Chem over eastern Mediterranean with the finest grid resolution (4 km) over Cyprus using different gas phase and aerosol mechanisms. The manuscript has interesting results. I suggest acceptance of the manuscript for publication but after considering a number of comments that follow.

Comments 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;

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Tyrlis et al. 2013; Anagnostopoulou et al. 2014) because unlike the Azores anticyclone, the ridge over the Balkans retains its distinct signature up to 500 hPa implying different dynamics involved in its formation. 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 Akrtitidis et al., 2016). 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; 4) Section 2.3: Please specify the chemical measurements carried out at the air quality stations. 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. 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 boundary conditions for simulated near surface ozone O3 (Akritidis et al., 2013). 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. 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 NOx, 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. 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. 10) As the authors mention, Table 4 indicates an overestimation for modelled ozone and an underestimation for modelled NOx. I think it would be interesting to see how the global O3 and NOx values compare with the observations and also how the statistical measures are being modified as we go from the course resolution to the fine resolution of the inner domain. For example, it is important to show if the finer resolution improves the evaluation 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 NOx precursors since as I can from Figure 8, CO is underestimated by the model before and after the 13th of July. 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.

Minor comments on the text Page 3, line 21: WRFC/Chem should be WRF/Chem.

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