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Interactive comment

Interactive comment on "Ozone air quality simulations with WRF-Chem (v3.5.1) over Europe: Model evaluation and chemical mechanism comparison" *by* K. A. Mar et al.

K. A. Mar et al.

kathleen.mar@iass-potsdam.de

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The authors would like to thank Anonymous Referee #1 for their constructive comments. Below are our responses.

Page 3 line 54-55: I disagree with this statement. Many European WRF-Chem modelling evaluation studies have been published in the last few years.

We have added additional citations of papers that apply WRF-Chem over Europe. However, in our view, studies that focus on evaluation over the whole European domain are still limited to date. If there are particular studies that fulfill this criteria that are not being discussed in the manuscript, the authors would appreciate it if the referee would mention the papers specifically. The sentences in question have been updated as follows to improve clarity: "The use of WRF-Chem over Europe has increased in recent years (e.g., Forkel et al., 2012; Žabkar et al., 2015; Solazzo et al., 2012a, b; Tuccella et al., 2012; Zhang et al., 2013a, b). However, only a limited number of these studies are dedicated to the evaluation of WRF-Chem-simulated meteorology and chemistry over the whole European domain."

Page 10 line 293-295: Please, in order to prove that differences between the two meteorological simulations are negligible provide statistical indexes or a comparison figure in the supplementary material.

A table and figures showing the meteorology from the RADM2 simulation has been added to the supplementary material; see Table S1 and Figures S4-S7. Furthermore, the manuscript has been updated as follows to directly address this question.

"Differences in predicted meteorology between the MOZART and RADM2 simulations are small, with differences in MSLP less than one hundredth of 1%, and differences in T2, WS10, and WD10 generally far below 1%. Since the simulations were run without aerosol-radiative feedbacks, it was expected that the two simulations would show minimal differences in meteorology, and we conclude that differences in O3 and NOx predicted in the MOZART and RADM2 simulations (Section 4.2) are a direct result of differences in the chemistry, rather than chemistry-radiative feedbacks. Statistics for meteorology for the RADM2 simulation can be found in the Supplementary Material, Table S1 and Figures S4-S7."

Page 15 line 478: to be in line with the NOx (NO2 and NO) treatment in MOZART simulation, I suggest to briefly explore NO concentrations in RADM2

A discussion of NO concentrations in RADM2 has been added to the revised manuscript, as follows. "Like for MOZART, NO for RADM2 is underpredicted throughout the domain, with NO concentrations slightly more negatively biased than in MOZART in all seasons except Fall, when NO concentrations are higher for RADM2

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than for MOZART and show better agreement with the observations. Temporal correlation for NO2 and NO in RADM2 is also found to show similar behavior to the MOZART simulation."

Figure 1: I suggest to represent temperature using the International System unit (K) here and everywhere else in the text.

The authors prefer to keep temperature in units of Celsius. Although it is not the SI unit, Celsius is widely used in the meteorological community, and is also used in GMD publications (see, e.g., http://www.geosci-model-dev.net/9/1959/2016/gmd-9-1959-2016.pdf). Furthermore, when calculating relative bias statistics (MB, NMB, MFB) for temperature as in Table 4, using Kelvin rather than Celsius makes the denominator extremely large and the bias extremely small, making relative bias statistics less meaningful. However, if the editor agrees that the temperature unit should be changed to Kelvin, we will make these changes to our manuscript.

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