Atmospheric
Chemistry
and Physics
Discussions

Open Access

Interactive
Comment

Interactive comment on "Air quality over Europe: modeling gaseous and particulate pollutants and the effect of precursor emissions" *by* E. Tagaris et al.

E. Tagaris et al.

tagaris@ipta.demokritos.gr

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Comment: This manuscript presents model results from a CMAQ air quality simulation for Europe for 2006, with comparison to observations from the same period. While the CMAQ model application itself seems reasonable, the comparison to observations is overall weak, with only qualitative assessments provided outside of Table 1. It would be more useful to reader for authors to provide more evaluation detail in the main text, instead of just saying ozone is overpredicted here and underpredicted there. One suggestion would be to provide seasonal spatial maps of Error/Bias (based on comparison to observed data) for ozone and/or PM2.5. Previous studies have provided such fig-

ures, and they are useful for quickly identifying areas of large error/bias, and can be compared to previous studies as well.

Response: We would like to thank the reviewer for raising all those issues giving us the opportunity to improve the quality of our manuscript. We have complied with his/her suggestions and we now provide in the revised version of the manuscript: i) more evaluation details ii) seasonal spatial maps of Mean Bias and Root Mean Square Error for MaxhrO3, NO2, SO2 and PM2.5 (i.e, Figures 4-7) and iii) we compare our results to previous studies using CMAQ model for Europe (i.e., Pay et al., (2010) and Appel et al., (2012)).

Comment: I think the manuscript could benefit greatly from an improvement in section 3.1.

Response: We have improved section 3.1. providing more evaluation details and discussion.

Comment: Regarding section 3.2 (Effects of precursor emissions on air quality), simply applying a factor to the emissions based on the ratio of observed to predicted values is unconvincing. Such an application does not take into account other effects, such a meteorology, advection, etc. The authors should consider using a more robust method for adjusting the emissions, such as inverse modeling. Otherwise, the results from section 3.2 are not all that useful for other modeling exercises, and would not be applicable beyond the current study. As such, it might be a better use of the authors time to expand and improve the model evaluation portion of the manuscript, wherein providing detail on where the emissions inventory may be in gross error. Overall, in their current forms, sections 3.1 and 3.2 do not provide enough information to the reader to be useful.

Response: We have complied with the reviewer's suggestion expanding and improving the model evaluation part of the manuscript and removing section 3.2.

Comment:Finally, the manuscript is in need of good deal of proof editing, as the gram-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

mar is in many instances poor. I think if the authors could address these issues, the manuscript would be improved and provide more information to the reader.

Response: An in depth proof editing has been performed in the revised version of the manuscript. We have incorporated all the reviewer's suggestions improving the quality of the manuscript. We trust that the reviewer will find sufficient the modifications and the information discussed now in the manuscript.

Specific Comments:

Comment: When referring to ozone, SO2, NO, etc., these are mixing ratios, not concentrations.

Response: We have changed "concentration" to "mixing ratio" for all gaseous pollutants examined here (i.e., ozone, NO2, and SO2).

Comment: Section 2 (pg 6684-6685; Ln 22-1): Remove the two sentences starting with MM5. This doesn't provide any real substance to the manuscript.

Response: We have removed the sentences.

Comment: Page 6691, Line 3: I object to the describing the model as having "built-in biases". This implies that the model is designed intentionally to have biases, which is obviously not correct. There may be a lack of detail in some of the model parameterizations (such as chemistry), but no where are biases built-in to the model.

Response: The statement has been removed in the revised version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6681, 2013.