

Interactive comment on “Implementation and evaluation of online gas-phase chemistry within a regional climate model (RegCM-CHEM4)” by A. K. Shalaby et al.

Anonymous Referee #2

Received and published: 13 March 2012

This is an interesting study focusing on the implementation and evaluation of an online gas-phase chemistry scheme within a regional climate model. It fits well to the scope of the journal. Furthermore the developed regional climate model coupled online with a chemical scheme has a large future potential for a number of studies especially when looking the feedbacks of chemical composition changes on regional climate and vice-versa. Even though the evaluation is rather qualitative I think that the manuscript deserves publication after taking into consideration my comments which aim to improve it.

Comments

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1) It is clear from Figure 11 that there is phase lag between observed and modeled ozone values at Northern Europe. Some discussion on the possible reasons for this discrepancy could be added. For example the stratospheric contribution might play an important role for the seasonal ozone cycle at Northern Europe. I guess that the stratospheric ozone contribution to the tropospheric ozone budget is not explicitly resolved. This is a limitation that should be mentioned. What do you consider as top chemical boundary conditions? Do you use values from MOZART chemical transport model for top chemical boundary conditions? If yes then the stratospheric contribution is implicitly accounted in the RegCM-Chem simulations. However I would suggest the authors for their future work to look carefully how well stratosphere-troposphere exchange is resolved in MOZART which gives the chemical top boundary conditions and how the seasonal ozone cycle looks like at the lateral chemical boundary conditions.

2) There is no reference to other relevant modeling evaluation studies. The authors refer to their evaluation results but they do not compare with results from other studies. I think it is important to add discussion on how these evaluation results compare with other relevant studies thus putting this evaluation study into a wider framework. There are a number of similar studies either for case study episodes of a few days (e.g., Delle Monache and Stull, 2003) or for longer time scales of a few months to years (e.g., Tilmes et al., 2002; van Loon et al., 2007; Vautard et al., 2009; Zanis et al., 2011).

3) The reference list needs an update. For example there are recent studies that have appeared in the literature focusing on climate change effects on tropospheric ozone and other pollutants over Europe (Andersson and Engard, 2010; Katragkou et al., 2011; Huszar et al., 2011). Furthermore there are a number of recent evaluation and sensitivity studies which use offline or online coupling of the same regional climate model RegCM and the air quality model CAMx (Katragkou et al., 2010, Zanis et al., 2011, Huszar et al., 2012).

4) Page 155, The authors state that Photolysis rates are determined as a function of several meteorological and chemical inputs including overhead column densities for

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O₃, SO₂ and NO₂, surface albedo, aerosol optical depth. I would suggest adding more information about the columnar densities used. Are they based on climatological values? Are they monthly? Source of the data used?

5) Page 158, lines 17-19: The authors state that biogenic VOC emissions are close to zero during the winter months. Is this a strong statement? How their winter biogenic VOC emissions compare with other modeling studies?

6) Page 159, lines 4-6: The authors state that the climatological chemical boundary conditions are provided by the global, three dimensional MOZART chemical transport model by using a monthly average of years 2000–2007. Please clarify if the chemical boundary conditions remain the same from year to year. If they stay constant from year to year, this would have an important effect on the lack of the year-to-year variability which is discussed later on in this manuscript. In general the chemical boundary conditions play an essential role which is not pointed adequately.

7) Page 165, lines 2-3: The authors state that the model is designed for climatological simulations and uses monthly emissions; therefore they would not expect to reproduce these daily events with any fidelity. I find this statement too strong. I understand that the model is designed to be used for long-term simulations but I think the aim should be also to improve the day-to-day variability and also the diurnal response of the model. For example in the current version of the model they are used anthropogenic emissions on monthly scale. This is a limitation to point and I would suggest the authors to include for the future model developments disaggregation of the anthropogenic emissions in weekly and hourly scales which is common for air quality modeling.

8) Page 167, lines 4-5: The apparent plume of high NO_y extending south into the Mediterranean from the coast of France (Figure 9) is really outstanding. I am not sure how realistic it is. The authors attribute this to a combination of high NO_x emissions and suppressed vertical mixing over the water. Looking the anthropogenic NO_x emissions from Figure 1, I notice relatively low emissions at the coast and inland of France but

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higher emissions over the sea. Are these higher emissions over the sea linked to ship emissions? Furthermore even these NO_x emissions over the sea are at least 3 times lower than the emissions over Southern British Isles, the Benelux states and Western Germany. I would also suggest the authors to look alternatively on the ratio H₂O₂/NO_y as an index for the VOC and NO_x limited regimes. The H₂O₂/NO_y ratio (Sillman, 1995) points towards a more VOC sensitive (ratio below 0.15-0.45) or NO_x-sensitive regime (ratio above 0.15-0.45).

9) Page 167, lines 21-27: The authors present how modeled temperatures compare with observed gridded data. It would be helpful to add few lines discussing how the biases in this study compare with reported biases of previous RegCM studies. That would give a framework for comparison with previous studies.

10) Page 168: Once the authors state that in the Northern Europe region, the measurements show a bi-modal peak in the ozone maxima, with the highest concentrations in the early summer. Then they state that at the Northern Europe stations, the measurements indicate that the seasonal ozone maximum occurs in May. These two statements are confusing. Please clarify if the measurements in the Northern Europe show the highest ozone concentrations in early summer or late spring.

11) A note on the run times should be made especially in comparison with other similar models like WRF-chem.

Minor comments

Page 152, Sect. should be written section.

Page 157, line 9. Please remove the word “for”.

The colour scale of Figure 4 is not successful with white colours in the middle.

References

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