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Geoscientific Model Development Discussions

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

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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 implic-

itly 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.

The RegCM-CHEM4 contains 18 vertical levels with the top of the model at 50 hPa. Depending on surface pressure, the top layer of RegCM4 represents the upper troposphere/lower stratosphere. Initial top layer ozone concentrations (as well as horizontal boundary concentrations advected in the top layer during the simulation) are determined by a vertical interpolation of MOZART ozone concentrations. On average, the modeled ozone concentrations in our top layer during the simulation are maintained at lower stratospheric-like concentrations. Currently, we do not include a vertical ozone flux between the top model layer and upper stratosphere across the 50 hPa isobar. So while we do account for stratospheric ozone vertical transport between the high-ozone model top layer and the lower model layers representing the troposphere, we likely underestimate the ozone vertical flux under time periods of intense vertical transport because the top layer could be quickly depleted. We have added a discussion of this treatment to the simulation design (Section 2.7; lines 282-289) and as part of future work in the conclusions section (Section 5; lines 627-631).

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).

At the reviewer's suggestion, we have added additional discussion to place our work in context with prior studies. We have added text to discuss the ozone bias for the event-based discussion as compared to other European summer case studies (Section 3.3;

lines 399-413) as well as a comparison with other seasonal studies (Section 4; lines 595-601).

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).

We have revised the manuscript and added the suggested references in three locations: (1) to reference studies that only refer to the effect of climate change on ozone (section 1; lines 40-42), (2) to describe the other European studies suggested by the reviewer (lines 83-85), and (3) to describe the offline coupling of RegCM and CAMx (lines 71–75). We distinguish this development of RegCM-CHEM4 with that of Huzar et al., 2012, which is suggested by the reviewer to be an "online" model. As defined in the paper (lines 50-64), we define online as a model with fully integrated chemistry while the Huzar et al. includes two separate model runs that are frequently re-initalized and is considered "offline" by the definition on lines 50-54).

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 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?

We have added additional information to explain our data sources in Section 2.4 (lines 188-192). Because the model is a fully coupled model, the majority of these variables come from RegCM4 components and these variables are updated on the timescale of 3-30 minutes (depending on the source module).

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?

A few model studies have examined seasonal differences in isoprene and VOC emissions over Europe. Steinbrecher et al., (2009) showed that 95% of biogenic VOC emissions occur during the summer (JJA) and Poupkou et al., (2010) a 600 fold increase in isoprene emissions from December to July due to the strong light and temperature sensitivity of emissions. Our simulations show similar seasonal variations in biogenic isoprene emissions calculated from MEGAN (see supplementary figure, attached). Average daily maximum emission rates are two orders of magnitude higher in JJA than for winter (DJF) for each region, with the 600 fold increase found in Poupkou et al., (2010). Further, Poupkou et al., (2010) compared two biogenic emissions models (MEGAN and Biogenic Emissions Model (BEM)) and found average January isoprene emissions never exceeding 1 mg/m2/day over the whole of Europe. Less than 1 mg/m2/day emissions were also found in the current study.

We have included references to the two biogenic emission model studies over Europe (Steinbrecher et al., 2009; Poupkou et al., 2010) and quote the seasonal isoprene emission differences found in RegCM-CHEM4 in lines 265-269.

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.

The reviewer is correct that we used a climatological average for boundary conditions, resulting in a lack of interannual variability in the chemical boundary conditions. We

have clarified this point in Section 2.7 (page 10, lines 289-293) and note that this implementation is designed for model testing. Also, we have added text in the discussion section (Section 5; lines 627-631) that highlight this point and its contribution to the lack of model interannual variability and note this is an important component of future development.

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.

We have revised the statement to address the reviewer's concerns (page 16; lines 473-483). We are currently working on a new emissions pre-processor that will disaggregate anthropogenic emissions to weekly and hourly scales and have noted this as part of future work (Section 5; lines 631-634).

8) Page 167, lines 4-5: The apparent plume of high NOy 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 NOx emissions and suppressed vertical mixing over the water. Looking the anthropogenic NOx emissions from Figure 1, I notice relatively low emissions at the coast and inland of France but higher emissions over the sea. Are these higher emissions over the sea linked to ship emissions? Furthermore even these NOx 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/NOy as an index for the VOC and NOx limited regimes. The H2O2/NOy ratio (Sillman, 1995)

points towards a more VOC sensitive (ratio below 0.15-0.45) or NOx-sensitive regime (ratio above 0.15-0.45).

Regarding the differences in NOx emissions and NOy response (e.g., higher NOx emissions in Britain/Benelux/Germany with relatively low NOy and relatively lower NOx emission in the South of France with relatively high NOy), we believe this occurs because daytime NOy over land is diluted by vigorous vertical mixing. This overland mixing typically disperses pollutants over a height of 1500 m. or more, yet this vertical mixing is suppressed over water if the water temperature is lower than the air. Emissions from cities such as Marseilles and Barcellona, located in close proximity to the sea, can be transported over the sea and trapped in the near-surface marine layer. This was described in Sillman et al., (1993) for plumes over Lake Michigan and the North Atlantic Ocean of the coast of the US. Velchev et al. (2011) report instances of reduced O3 and elevated black carbon in ship-based measurements in the Mediterranean in the immediate downwind of Marseilles and Barcellona, which they attribute to removal of O3 by elevated NOx.

Another issue is that the regridded global emissions is exaggerated due to coastline emissions in the raw 0.5 degree MACCity emissions dataset. In the raw data, emissions are located right on the coastline, and it is likely that the regridding needed to transform the emissions to a regional projection cause a slight shift in the emissions where a single grid may contain both sea and a high-emission urban area. This can be seen along the southern coast of England.

We have clarified these issues in the text on page 18, lines 535-549.

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.

We have added additional discussion on prior RegCM versions and temperature biases

on page 19 (lines 571-575) to place our results in context.

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.

We have revised this statement (lines 584-588) for clarity.

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

Because we are not conducting a side-by-side comparison of the two models, it is not possible for us to make a definitive statement on the run-time of the same domain, parameterizations, and platform specifications as WRF-Chem. The abbreviated mechanism will likely make this a more computationally efficient simulation, as already noted in the text.

Minor comments

Page 152, Sect. should be written section.

The use of the abbreviation “Sect.” follows the EGU Manuscript Composition guidelines which state: “The abbreviations "Sect." and "Fig." should be used when they appear in running text followed by a number unless they come at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that...", available at http://www.atmospheric-chemistry-and physics.net/submission/manuscript_preparation.html.

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

This has been corrected in the revised manuscript.

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

We have updated the color scale of Figure 4 to remove white colors in the middle.

Interactive comment on Geosci. Model Dev. Discuss., 5, 149, 2012.

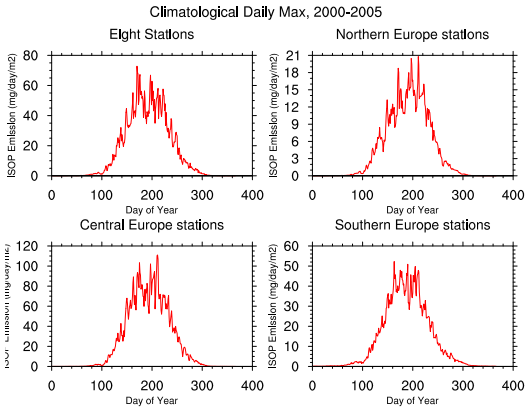

Fig. 1. Climatological isoprene emissions (2000-2005).