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Atmospheric Chemistry and Physics Discussions

Interactive comment on "GEM-AQ/EC, an on-line global multiscale chemical weather modelling system: model development and evaluations of global aerosol climatology" *by* S. L. Gong et al.

Anonymous Referee #2

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In this paper the authors presented the new global aerosol-chemistry-climate coupled model, GEM-AQ/EC. The main results of the paper are to show the model performances in simulating aerosol concentrations and distributions, to evaluate the variability of natural aerosol emissions which are strictly linked to the meteorological conditions (mineral dust, sea salts and vegetation fires). The authors showed the results from a 10-year simulation evaluating the global aerosol climatology. The presentation and the analysis of the results is generally clear, but it can be improved to clarify some of the methods and the analysis, I would suggest acceptance of the paper after taking into consideration the following comments.

Major comments:

1) The description of emissions, in particular Section 2.3, should be improved. It is not clear which anthropogenic emissions are kept constant during the simulation and which are varying during the 10 yr simulation. In particular more informations should be provided about sulphur emissions, which are an important component of the aerosols. I would suggest to the authors to summarize in a new table or expanding table 2 with sulphur emission budget. About sulphur species, in Section 2.2 there is no mention about DMS emissions, which are a significant source of sulphate. Some comments should be provided on the choice made by the authors to use quite old and coarse resolution datasets (GEIA and Liousse 1997, 4.4 x 7.5 degrees).

2) Section 3.3: the description of the comparison between AERONET data and model results should include more details. For example, how the stations were selected? There was any filter applied to the stations in other to calculate the monthly average? It is not very clear the sentence on the significance of the correlation coefficient between the observed and simulated AODs. How is it calculated? I don't think that we can observe from Figure 7 the spatial and temporal distributions of the AODs. The under-overestimations should be analyzed region by region in more detail and better related to the possible causes (e.g. anthropogenic or natural emissions).

3) The description and the analysis of the comparisons between observed and simulated PM10 and PM2.5 is not always clear (Section 3.4). The authors should provide more details on the methodology used to perform the comparison. For example, how many years and which periods were available for each measurement network (Improve, EMEP, ..)? How many stations were considered and how they were selected? Any filter was applied to select the stations? How were calculated the statistics (correlation coefficients), for example what doest it mean "0.65 for the 10-yr averaged PM10 and PM2.5"? Also the authors should comment about the impact of aerosol species which are not included in the model, such as nitrate and secondary organic aerosols, when comparing to PM observations. In principle the model should always underestimate

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PM10 and PM2.5 observations. The authors should also link what seen in the PM2.5 and PM10 observations with the discussion on AOD. For example it is seen a model overestimation of dust over NA, which can be also seen in an overestimation of AOD over the Atlantic (Figure 6). The sum of the aerosol components results in a simulated PM10 and PM2.5 generally within a factor of 2 with the observations, on the other hand in Figure 9 we can see that sulphate and dust are overestimated by the model, while BC and POM underestimated. The authors should comment more on this and also how the simulated aerosol chemical composition is influencing the simulated AOD.

4) The conclusions should be expanded. The authors should summarize the results in more detail and discuss the limitations/advantages of using a global model for air quality studies. Probably, they should also introduce in the conclusions the companion paper of Zhao et al., 2012, which contains further analysis of the same simulation.

Minor comments:

Page 9284, line 6: "at scales from global to urban domains". The authors, also in the title, define the model as a multiscale model, but they don't give any description of the model features for this specific point. Please clarify what do you mean for "Multiscale" model and provide the correspondent model description, for example in Section 2.

Page 9285, lines 10-12: "South east Asia ... pollution loads". It is not clear if the sentence is still referring to the paper of Koch et al, 2007 or is form another reference.

Page 9285, line 18: "primary sources", but sulphate is mainly a secondary aerosol.

Page 9286, lines 7-11: Consider to revise the entire sentence "For instance, ... transport models", as it results difficult to read.

Page 9286, line 17: the authors stated that the goal of the study is to develop a comprehensive emission inventory of both natural and anthropogenic aerosol for a 10 year simulation. On the other hand anthropogenic emissions as described in Section 2.3 are mainly based on existing inventories previously developed.

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Page 9287, Section 2.1 could be more detailed. Which are the couplings between the gas and aerosol phase, in particular regarding SO2 and DMS oxidation?

Page 9289, Line 7: "SF" is "SU" for sulphate?

Page 9290, Lines 9-11: "This nudge setup ... reanalysis meteorology". Consider a revision of the sentence, as difficult to follow. Also about nudging, which variables are nudged?

Page 9290, Line12: a spin-up of 3 months was performed, but which initial concentrations were used to start the spin-up? 3 months are probably enough for aerosols but what about the gas-phase chemistry?

Page 9290, Line 17-19: here the authors state that the anthropogenic emissions are constant, which is not completely clear from Section 2.3. Some emissions are varying (e.g fossil fuel) some are fixed (e.g. bio fuel).

Page 9292, Line 5-6: "Global SS within the Canadian GCMIII integrating CAM (Gong et al 2002)." Sentence not completed?

Page 9292, Line 10: the meaning of "r80" is not explained in the text.

Page 9292, Lines 1-12: sometime units of sea salt emissions are given as kg and not as kg yr-1.

Page 9292, Lines 14-20: the recent paper from Huneeus,ÂăN. et al 2011 (Global dust model intercomparison in AeroCom phase I, Atmos. Chem. Phys.) should be included in the discussion of dust simulation.

Page 9293, Line 24: "Korea" instead of "Korean".

Page 9294, line 19: the acronym BOTE was not explained before in the text, and it is mentioned only one time in the entire paper.

Page 9294, line 20: Southern Europe is "west" of the Ural Mountains.

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Interactive Comment

Page 9295, line 9: "except in 1996".

Page 9295, Line 22: ".. primary aerosol source strengths". Sulphate aerosol is mainly a secondary aerosol which is one of the main component in determining the total AOD.

Page 9295, Line 27: "model simulated AOD summarizing the contributions from . . .". This sentence is not very clear, as Figure 6 shows the simulated AOD calculated as the sum of the single aerosol species. I don't think that this can be described as the summary of the contribution from all the species.

Page 9295, Figure 6: I would suggest to the authors to improve Figure 6, by using a different color scale, or maybe providing both the MODIS and simulated seasonal maps, instead of over plotting contour lines. For example it seems that the AOD is overestimated by the model for all season over the Atlantic (dust outflow).

Page 9300, lines 4-7: it is not clear here and in the emission description if a seasonality is included in the used emission dataset.

Page 9300, line 15: from dust observations in the Atlantic (Figure 13. Barbados, Rsmas, and Bermuda) it is seen that the model underestimates the observations during the summer peak, while previously the authors showed an overestimation of mineral dust transport from Africa (Figures 6 and 9).

Page 93001, line 5-6: A reference is not given for the review mentioned in the text.

Page 93001, Lines19-20: Figure 14 is not properly described. It is not given both here and in the figure caption the source of the satellite data.

Page 93001, lines 26-28: what is exactly represented by the contour lines in Fig. 14b. Is it the inter annual variability or the seasonal variability? My understanding of the Figure is that the seasonal variability of sea-salt is large, as shown by Figure 3, but the inter annual variability is rather small, about 1-2% (Figure 14b) in the souther hemisphere below 40S (where the main SS emissions occur). The larger Std Deviation over the Oceans at higher latitudes, might be due to the variability of anthropogenic aerosol

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continental outflow.

Figure 1: Caption ". . . red dots are the stations ARE from GAW .."

Figure 8: The figures in panel b are the same of panel a (only IMPROVE is shown).

Figure 13: Caption " . . . soil DUCT . . ."

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9283, 2012.