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Atmospheric Chemistry and Physics Discussions

Interactive comment on "IAP-AACM v1.0ïijŽ Global to regional evaluation of the atmospheric chemistry model in CAS-ESM" *by* Ying Wei et al.

Anonymous Referee #2

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General Comment

The authors tried to introduce their newly developed global and regional CTM (IAP-AACM) which can be used as the chemical module of their ESM. In this paper, they explained the ability of the IAP-AACM to properly simulate the spatial and temporal variation in the concentration of major atmospheric gas species and aerosols in 2014. The simulated concentrations were compared with various observations globally and in particular with those obtained in China, which showed a comparable representation level of their model to the other global and regional CTMs. I think the purpose of this paper is more suitable for other journal such as Geosci. Model Dev., but the paper can be also within the scope of ACP. I will leave the judgement to editor which journal is suitable for this paper. In any case, however, I noticed several issues in this paper

which cannot be passed over to be published in any journal. I suggested that the authors should consider the following comments.

Major Comment:

The main purpose of this paper is to clearly show the ability and/or inability of their model to simulate the observed spatial and temporal variation in the concentration of the chemical species in the atmosphere. Based on those findings the authors and also the readers of the paper can understand what kind applications are suitable for this model and what aspect of the model should be further improved in order to apply it to a particular issue. From this point of view, the self-evaluation about the ability of the model by authors were often insufficient and unclear. The good points and also the shortcomings of the model should be described more specifically in the text. I pointed out some of those points as specific comments in the following, but I strongly recommend the authors to reexamine the descriptions particularly in the model evaluation parts.

Specific Comments:

- L24: What are the aerosol effects here?

- L38: Only R-value can not ensure the accuracy of the simulation. How about MB or NMB?

-L58: Why didn't you cite the latest AR5 report here?

- L79: Typo? e.g.

- L86: EA, this should be defined at its first appearance in the text.

- L108-109: What do you mean here? Could you use more words to explain "localization of the process parameterization"?

- L135-139: Are there citable references for CoLM, CICE, and IAP-OBGCM?

- L158: What is the main difference between these two models (GNAQPMS and IAP-AACM)?

- L204: What does "synchronous time step" mean ?

- L206: What is the reason for choosing the year 2014 as the focal year?

- L224-225: Emission data used in the study are not up-to-date, the base year of each database is a bit old. Therefore, adjusting the emission data to input them to the model is suitable for the purpose of this study. However, you only mentioned about the adjustment of SO2 emission in China in the text. Did you adjust other species emission ?

- Figure2 : Why did you compare with NCEP R1, not with NCEP-FNL? What is the purpose of it?

- L244: This statement is not correct. The difference in RH2 between WRF and Re-analysis is much larger in general as shown in Fig2 over land area.

- Table2 and L246-248: If you want to mention only the correlation coefficient of annual mean values, you should remove Table 2. If you want to retain Table2, you should explain the table more precisely here. Table 2 is hard to read and insufficient caption.

- L270: Why did you take average of 2006-2015 only for WDCGG?

- Table3: It is better to include the information of region of each observation datasets.

- L298: The value (23.3) differs from that in Table4.

- Table5: Table 5 is not completely filled with the necessary information for OM (other sources and total sink are missing).

- Figure4: Fig4c and 4d should be switched to be in accordance with the order of panels in Fig3.

- L363: Typo?: Northern Hemisphere

- L368-369: Why did IAP-AACM show the lowest concentration of CO over ocean among the models considered here?.

- L378-380: The seasonal variation of surface O3 should be different in different environment even in the same region. So, I recommend the authors to compare separately for different environment (e.g. maritime area vs mountainous area). Otherwise, I can not regard the Fig6 as an evidence that the model can well simulate the seasonal variation of surface O3.

- L385: Underestimation in Antarctica is not small. Such an underestimation could be seen in the other CTMs. Can you use more words about this issue here?

- L385-387: In the NH land area, it seems that the model completely failed to represent the seasonal cycle of surface O3, but the author regarded it as just a positive bias during July-September. More words for this issue are necessary. For example, what do you think the apparent underestimation in cold season in NAmerica and Europe?

- L388-390: In Badia et al (2017), they suspected the excessive emission height of NOx which will cause low NOx at surface and consequently might lead to weak NO titration. Do the same things happen in your model?

- L390-391: The AACM apparently showed larger concentration of surface O3 in the tropical regions (central Africa, South America, and Southeast Asia) than the other models. However, the concentration of O3 precursor species (CO and NOx) in these regions are not so different among the models. Can you give discussion about the issue here?

- L391-394: These two sentences are not consistent to each other. In general, the region of high O3 concentration can be different in different season. If you look at the "annual mean" concentration, the highest O3 usually occur in the source region in summer, but that in the downwind region in winter. However, if you see the different index such as MD8H O3, you can see completely different seasonal cycle. I strongly

recommend the author to carefully revise these sentences.

- L402-403: An overall evaluation of O3 dry deposition in global CTMs can be seen in Hardecre et al. (2015). I recommend to check it out. Hardacre et al. (2015) An evaluation of ozone dry deposition in global scale chemistry climate model, Atmos. Chem. Phys., 15, 6419–6436, doi:10.5194/acp-15-6419-2015.

- L416-418: The concentration of NOx over oceanic areas are larger in AACM than in other models, which might stem from larger emission or longer life time of NOx in AACM than the other models. I recommend to discuss this issue further here.

- L436-438: This is misleading statement. The model results are not generally with in a factor of two, but they apparently tend to overestimate the observation in all the three regions. The NMB value for sulfate in Europe, 0.11, is incorrect which is 1.1 in Table 6.

- L438-439: How can you conclude like this (2ugm-3 higher)? What is the ground of this statement?

- L442-443: What aspect of the observation do you think your model can reproduce? You should be more specific.

- L446-449: About the simulation of ammonium, I can see obvious underestimation in NAmerica and overestimation in Asia and Europe.

- L455-457: The concentration of OC were obviously underestimated by the model.

- L491-492: The highest AOD in DJF in east China is not clearly seen both in satellite and model AOD.

- Figure10: It's better to show scatter plots too, at least as a supplement figure.

- Figure11: The area and the map projection of the figures for all models should be united.

- L517-519: I'm sorry I can not understand what you want to mean here.

- L545-547: What do you want to mean here? Your model overestimated the NO2 in summer in NC and YRD regions. If you don't use the NO2* observation, the model's overestimation should become worse.

- L547-550: I can not understand what aspect of seasonal difference in NO2 column observation were reproduced by your model. You should describe more specifically on it.

- L595: Typo? respects → aspects

- Conclusions should be revised according to the modifications made to respond the reviewers comments.

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