Geoscientific
Model Development
Discussions

Interactive comment on "Decadal evaluation of regional climate, air quality, and their interactions using WRF/Chem Version 3.6.1" *by* K. Yahya et al.

Anonymous Referee #2

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The authors present for the first time a decadal regional chemistry climate simulation including a full coupling of chemistry-aerosol-radiation feedbacks. For this they use the model WRF/Chem. So far WRF/Chem was mainly used for short term studies.

The authors analyse some meteorological variable (2m temperature, 10 m wind speed and precipitation), ozone, PM 2.5 and aerosol-cloud-radiation variables and conclude that the performance of the model is good for the meteorological and chemical variables whereas the aerosol-cloud-radiation results should be improved for long-term climate simulations.

Alltogether, most of the results are not fully comprehensible as the authors provide not enough details about the procedures used.

Especially, more details should be provided about

- the re-initialisation procedure and how this interacts with the ICs/BCs from CESM/CAM5 (including a more quantitive assessment how much the re-initialising frequency changes the results) and

- about the way the statistics presented in Table 2 has been calculated. Is this really a point-to-point / date-to-date comparison?

Therefore the article is subject to major revisions from my point of view. A list of the scientific and content related questions follows as well as a list of required technical corrections.

Scientific questions and content-related remarks:

- page 6711, line 2, p. 6714, l. 20: What do you mean by "similar gas-phase chemistry and aerosol treatment"? Which are the differences if they are only "similar" and not identical? Do you still have to map species (if yes, which one and how), or are you using identical species? Please provide more details.

- page 6712, line 1-13: What are these re-initialisations good for? First of all, what are you re-initialising? Meteorology? Chemistry? The whole model?
From what you write in the paper I understand that you only re-initialise the meteorological, but not the chemical fields. Is this done in order to keep the model near the observed weather? But in this case 1 month should be much too long. Additionally, in this way the chemical and meteorological variables are not consistent any more.
Please give reasons for this procedure! Personally, I have my doubts, that you can use a model setup including such a procedure for climate applications at all. From what you say later on, the results depend on this re-initialisation frequency what just strengthens my reservations against this procedure. (Especially the

"buildup of storm systems, especially over the warm Atlantic" (page 6717, line 27-28) makes me wary.) But I think I can not really judge until I get more information about the reasons for this procedure and about how this re-initialisation works.

Additionally, I do not understand, how this re-initialisation with NCEP data fits with the statement in section 2.2 that you are using ICs/BCs from CESM/CAM5 for meteorology and chemical fields.

- page 6712, line 18-19: Why are you using a discrete and not a linear distribution of the emissions over the years? An assumption that the emissions changed linear seems to be more realistic. Especially, as for the first period the emission data is "valid" for the year before the actual period and for the last period for the last year. Only the middle period is centered around the given emission year.

- page 6712, line 24-25: The resolution of the emission is very similar to that of the model grid. Following the publications by Valari und Menut (2014) this should be assumed to be too coarse to expect really good results.

- page 6713, l. 7-8: "other RCP groups are used to approximate these emissions (Table S1)". Please be more precise: which species are approximated with which RCP group and how?

- page 6713, l. 15: Is the "simple inverse distance weighting" mass or better flux conserving? Otherwise the amount of emitted substance would be artificially modified due to your choice of model domain.

- Sect. 3.2.1 / Table 2: More information about how this statistic was calculated would be desirable.

Technical corrections:

C2029

- p. 6711, l. 15: This sentence is unclear. Maybe just a word or two are missing?

- p. 6712, l. 1: "mb"? Better use SI-Units "hPa".

- p. 6712, l. 12: add degree-sign after first 0.5

- Table S1: please use consistent annotations, i.e., if more then one species / modes are named give (Yes, No ,Group) for each individually.

- p. 6714, Eq (1): use larger brackets

- p. 6716, l. 2: consistently write "sulfate (SO_4^{2-})"

- p. 6716, l.10: "systemetic" → "systematic"

- p. 6716, l.26 "0 to $-3°$ C → $-3°$ to $0°$ C

- p. 6717, l. 22: It is unusual to start with Fig. 3d instead of Fig. 1.

- p. 6717, l. 29: "at the coast" not "in".

- p. 6719, l. 29: "Corr" not introduced.

- p. 6722 ff.: Obviously you replaced something with "AIRS-AQS", because the space in front of "AIRS-AQS" is missing everywhere.

- p. 6724, l. 3 (and below): It is very unusual to refer to Winter as JFD instead of DJF. Why are you using this notation?

- p. 6724, l. 15: AIRS in front of AQS missing.

- p. 6728, l. 10: remove "the" before isoprene.

- p. 6729, l. 14-17: reformulate this sentence. It is not understandable without thinking a long time about grammar and what you like to say.

C2030

- p. 6730, l. 10-15: repetition of p. 6729, l. 9-14 ?

- Fig. 1: What are the dots for? Is it mean bias per measurement station? Please be more precise.

- Fig. 4 - 7: in caption and y axis labels: "AIRS" missing in front of "AQS".

- Fig. 7 / 8: explain what are the dots. I assume the model results are the 2d plot and the observations are the dots, but you never write that.

- Fig. 9: Colourbar scale is not readable.

- Fig. 9: What does the "(MODIS)" below the AOD, CDNC, CWP and COT annotation mean?

Literature:
Valari, M. and Menut, L.: Does an Increase in Air Quality Models' Resolution Bring Surface Ozone Concentrations Closer to Reality?, JOURNAL OF ATMOSPHERIC AND OCEANIC TECHNOLOGY, 25, 1955–1968, doi:10.1175/2008JTECHA1123.1, 2008.

Interactive comment on Geosci. Model Dev. Discuss., 8, 6707, 2015.