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Interactive Comment

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.

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Reply to Reviewer #2

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

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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.

Reply:

We thank the reviewer for careful review of this manuscript and valuable comments to improve the quality of manuscript.

We have carefully addressed all the comments raised by the reviewer to improve the presentation quality and organization of our paper. We have also included more details about the methodology in our study. Please see below our point-by-point replies. All page and line numbers in this reply refer to those in the revised manuscript in the track mode.

Especially, more details should be provided about âĂć the re-initialisation procedure and how this interacts with the ICs/BCs from CESM/CAM5 (including a more quantitive assessment how much the reinitializing frequency changes the results) and

Reply:

Sensitivity simulations for 1 month (July 2005) have been carried out to quantify the differences in the reinitialization frequency, meteorological ICs/BCs and cumulus parameterization subroutines. The results are documented in the last part of the supplementary material. In summary, the monthly reinitialization frequency gives the highest correlation with observational data GPCP and PRISM, however, it also gives large values of normalized mean bias (NMB) and normalized mean error (NME). The use of a 5-day reinitialization helps to reduce both NMB and NME with slight to moderate improvements, it also reduces the R value. Overall, there are no substantial changes in results generated using a 5-day versus a 1-month reinitialization. More discussions regarding this have also been included in our reply in the Scientific Question part.

âĂć about the way the statistics presented in Table 2 has been calculated. Is this really

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a point-to-point / date-to-date comparison?

Reply:

This has been addressed below in our reply in the scientific question part.

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: âĂć 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.

Reply:

Both WRF/Chem and CESM use the CB05 gas-phase mechanism (Yarwood et al., 2005). However, WRF/Chem includes chlorine chemistry from Sarwar et al. (2007), while CESM_NCSU uses a modified version of CB05, the CB05 Global Extension (CB05GE) (Karamchandani et al., 2012). CB05GE includes more bromine associated chemical reactions for the stratosphere, reactions involving mercury species, and additional heterogeneous reactions on aerosol particles, cloud droplets and on polar stratospheric clouds (PSCs), which are more important for global simulations. Both WRF/Chem and CESM_NCSU also use a modal aerosol size representation, rather than a sectional size representation. MADE/VBS is used in WRF/Chem while a 7-mode prognostic Modal Aerosol Model (MAM7) (Liu et al., 2012) is used in CESM_NCSU. Both aerosol modules include sulfate, nitrate, ammonium, black carbon, organic carbon, dust, and sea salts. For gas-phase species, no species mapping are needed at all and for aerosol species only minimal mapping is require (i.e., mapping of the same species for the same aerosol modes). The above information has been added into the

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revised paper (lines 184-195 of page 9).

âĂć 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 cannot 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.

Reply:

The reviewer is correct that the reinitialization has only been done for meteorology (it has been stated explicitly in the revised paper). The reinitialization technique was recommended by the original developer of WRF/Chem at NOAA and has been used in the past extensively for both climate/air quality studies that focus on meteorology-chemistry feedbacks (e.g., Chen et al., 2013; Glotfelty et al., 2014; Penrod et al., 2014; Berg et al., 2015; Forkel et al., 2015; Ritter et al., 2013). In such studies, nudging or FDDA techniques cannot be used as they may quench the feedback effects to a large extent. The use of the reinitialization technique is to provide reasonable meteorological fields while allowing chemistry-meteorology feedbacks within the system. From

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this perspective, reinitialization technique serves similarly to the nudging technique to constrain the meteorological fields (e.g., wind fields or precipitation) from getting too large discrepancies due to the accumulation of small numerical errors over a long time period, and also to ensure more accurate meteorological fields (typically by comparing with observations) to drive the chemical calculations.

There were some confusions in our original paper regarding reinitialization. The model was reinitialized towards the bias-correct CESM/CAM5 meteorology, instead of NCEP data itself. The biases in CESM/CAM5 predicted meteorology were first corrected using the NCEP data before their use to derive initial and boundary conditions for WRF/Chem simulations (which are referred to as biased-corrected CESM/CAM5 BCs and ICs). We have clarified such confusions in the revised paper. We also added some more details regarding the bias-correction approach used in this work in lines 201 - 212 of pages 9-10, as follows: "Temperature, water vapor, geopotential height, wind, and soil moisture variables available every 6 hours from the NCEP Final Reanalyses (NCEP FNL) dataset are used to correct the ICs and BCs derived based on results of CESM_NCSU for WRF/Chem simulations. In this bias-correction approach, monthly climatological averages for ICs and BCs are first derived from both NCEP and CESM_NCSU cases. The differences between the ICs and BCs from the NCEP and CESM_NCSU climatological averages are then added onto the CESM_NCSU ICs and BCs to generate bias-corrected CESM_NCSU ICs/BCs."

From our past experience by conducting many years of simulations over various geographical locations, to run an online-couple meteorology/chemistry model freely without any reinitialization could generate erroneous meteorological fields and further deteriorate the simulation of air quality. Thus, reinitialization of meteorology is an alternative method to the commonly-used nudging technique to ensure satisfactory meteorological fields and to drive the chemical systems, which eventually make the simulation results of both meteorology/air quality credible and scientifically sound.

The reinitialization frequency may affect the simulation results. More frequent model

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reinitializations can give predictions of meteorology that are closer to the reference data that provide the ICs and BCs (however, this does not mean necessarily better predictions). We have conducted a few sensitivity simulations to further test the impacts of reinitialization frequency on the simulation results. The comparison of predicted precipitation against GPCP and PRISM shows that the 1-month reinitialization gives the best correlation coefficients (R), 0.5 and 0.7 respectively, compared to the 5-day reinitialization with R values of 0.4 and 0.3, respectively. However, the 5-day reinitialization gives lower NMB and NME compared to the 1-month reinitialization. The WRF/Chem simulation with 1-month reinitialization also gives slightly better spatial distribution of precipitation and other cloud related variables than those using the 5-day reinitialization. Therefore, we chose the 1-month reinitialization for our final production simulations. The above comparison also shows that the reinitialization frequency was not the main reason for the buildup of storm systems over the warm Atlantic as previously thought. Based on additional sensitivity simulations that we carried out, the cause of the buildup of storm systems is more likely due to the choice of cumulus parameterization scheme in our model. By comparing the sensitivity simulations using the Grell 3D (in this work) and the multi-scale Kain-Fritsch (MSKF) cumulus scheme (which is available in WRF/Chem v3.7 and later), we found that the simulations with MSKF give much lower precipitation amounts, as well as much lower NMB and NME. However, the R value is not as good as for the simulations with the Grell 3D. . In addition, the MSKF scheme does not include aqueous-phase chemistry in convective clouds, which is currently only available in the Grell cumulus parameterization scheme. The results of our sensitivity analysis for precipitation have been included in the Supplementary material (see Section A4).

Therefore, based on our sensitivity simulations and findings, we have revised our conclusions regarding the reasons for the precipitation biases in Section 3.1 in our manuscript.

âĂć page 6712, line 18-19: Why are you using a discrete and not a linear distribution of

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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.

Reply:

First, RCP emissions are discrete, and only available for the years 2000, 2005, and 2010. As we are conducting a “climatological simulation”, using the emissions from the representative years should be sufficient to represent the current state of emissions, since different years of our simulation really present more “current” or “future” years instead of a specific year. In addition, all our model evaluations are conducted based on a climatological timescale (i.e., decadal), rather than on individual years. The distribution of emissions might be more important if we are conducting air quality type of studies for a specific year, such as 2001 or 2010.

âĂć 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.

Reply:

We agree with the reviewer that emissions at a grid resolution that is very similar to that of the model grid may introduce some errors into the chemical modeling. However, the original resolution of the RCP emissions are coarse, i.e., 0.5o $\times$ 0.5o, which is the finest grid resolution for RCP emissions We have regridded the RCP emissions to our model resolution, at 36-km by 36-km. 36-km is a reasonable horizontal resolution and well used for many other regional studies over the continental U.S., which is much finer than most of other global climate/air quality applications. In this study, using a 36-km by 36-km horizontal grid resolution yields 148 $\times$ 112 grid cells and considering the multiple decadal simulations of WRF/Chem in this work, even with such a resolution, it is already extremely computationally expensive. Reducing further the horizontal grid

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space will not be feasible. The publication by Valari and Menut (2008) conducted simulations of up to 6-km by 6-km resolution, is carried out over a much smaller area as compared to the continental U.S. – over a highly urbanized area of 180-km by 180-km.

âĂć 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?

Reply:

The RCP species used to approximate the CB05 emissions have indeed been listed in Table S1 in our originally-submitted paper (now Table S2 in the revised paper). An example has been given in the previous sentence, “Some of the CB05 species are directly available in RCP; however, others are lumped into RCP groups, for example, the “other alkanals” and “hexanes and higher alkanes” in the RCP groups can be considered to approximately represent the acetaldehyde and higher aldehydes emissions required by CB05, respectively (Table S2)”.

âĂć 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.

Reply:

The “simple inverse distance weighting” method is mass-conserving.

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

Reply:

Additional details on how the statistics were calculated have been added in lines 225 to 233, pages 10-11, as follows: “For surface networks with hourly data, e.g., NCDC, the observational data are paired up with the simulated data on an hourly basis for each

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site. The observational data and simulated data are averaged out for each site. The statistics are then calculated based on the site-specific data pairs. The satellite-derived data is usually available on a monthly basis, and the simulated data are also averaged out on a monthly basis. The satellite-derived data are regridded to the same domain and the total number of grid cells is similar to that of the model outputs. The statistics are calculated based on the grid cell pairs (satellite-derived and simulated data pairs). The time dimension is removed for the climatological evaluation, the statistics are based on a site-specific average or a grid cell average."

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

Reply:

This sentence has been revised to make it more clear. The revised sentence is as follows: "The main updates include the implementation of an extended version of Carbon Bond 2005 (CB05) (Yarwood et al., 2005) gas-phase mechanism with the chlorine chemistry (Sarwar et al., 2007) and its coupling with the Modal for Aerosol Dynamics in Europe/Volatility Basis Set (MADE/VBS) (Ahmadov et al., 2012)".

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

Reply:

The unit has been changed to hPa.

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

Reply:

The degree sign has been added.

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

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Reply:

The table has been updated to keep the annotations consistent, see Table S2 (which is original Table S1).

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

Reply:

The bracket size has been changed.

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

Reply:

SO42- has been defined previously in page 11, line 240, so no change was made.

âĂć p. 6716, l.10: “systemetic” ! “systematic”

:

This typo has been corrected.

âĂć p. 6716, l.26 “0 to −3_ C ! −3_ to 0_ C

Reply:

This has been corrected.

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

Reply:

The discussion on Fig 3d has been moved back towards the end of Section 3.1, so that the discussion starts from Fig 1. now.

âĂć p. 6717, l. 29: “at the coast” not “in”.

Reply:

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This has been corrected.

âĂć p. 6719, l. 29: “Corr” not introduced.

Reply:

Corr is introduced earlier in line 222 of page 10, it has been renamed as “R”.

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

Reply:

This issue seems to be caused by the typeset of the journal since it didn’t show in in the word version of our original manuscript. We will make sure the typeset will be done correctly this time.

âĂć 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?

Reply:

We are using JFD as we are averaging January, February and December from the same year.

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

Reply:

This has been fixed.

âĂć p. 6728, l. 10: remove “the” before isoprene.

Reply:

It has been removed.

âĂć p. 6729, l. 14-17: reformulate this sentence. It is not understandable without

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thinking a long time about grammar and what you like to say.

Reply:

The sentence has been revised as follows “The MODIS AOD, however, shows slightly elevated AOD over eastern U.S., but the magnitudes are not as high as the simulated AOD over eastern U.S. MODIS-derived AOD is also higher over western U.S. compared to eastern U.S., and this trend is not found in the simulated AOD.”

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

Reply:

The second part has been removed.

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

Reply:

Yes. An additional sentence has been added to the figure caption, “Each marker represents the MB of each variable at each observational site.”

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

Reply:

“AIRS” has been added to the captions and y-axis labels.

âĂć 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.

Reply:

We have indicated in the captions that the observation is represented by markers and simulation is represented by the background in the revised paper.

âĂć Fig. 9: Colourbar scale is not readable.

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Reply:

Figure 9 has been resized to make the color bar scale more readable.

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

Reply:

It means MODIS-derived satellite data. However, the (MODIS) has been removed as there is already a header for the MODIS plots, with the exception of CWP, as the CWP is further derived by Bennartz (2007) from MODIS data.

Literatures cited in this reply:

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with respect to aerosol radiative feedbacks on meteorology and pollutant distributions, Atmos. Environ., 115, 630 – 645, 2015, doi:10.1016/j.atmosenv.2014.10.056.

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