Atmos. Chem. Phys. Discuss., 15, C3768–C3773, 2015
www.atmos-chem-phys-discuss.net/15/C3768/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can a coupled meteorology-chemistry model reproduce the historical trend in aerosol direct radiative effects over the Northern Hemisphere?" *by* J. Xing et al.

Anonymous Referee #3

Received and published: 18 June 2015

The manuscript presents an analysis of a 21-year simulation (1990-2010) of aerosol amounts over the Northern Hemisphere with the WRF-CMAQ model for the summer (JJA) season. In particular, the analysis focuses on the ability of the model to reproduce long-term trends in aerosol-related quantities such as aerosol optical depth (AOD) and clear-sky shortwave radiation, comparing model calculations with both satellite-based and ground-based observations. Clear declining trends in AOD are seen in the observations and model results for eastern North America and Europe and a notable underestimate of the aerosol-related quantities is found for regions dominated by soil dust aerosol.

The manuscript presents an impressive amount of information, with comparisons against a large number of satellite and ground-based observations. The retrieval of aerosol quantities from satellite observations is an under-constrained problem, requiring assumptions about surface characteristics and aerosol composition or microphysics. Different assumptions or methods used during the processing can lead to significant differences in the retrieved aerosol quantities. A quick glance at the different AOD estimates for eastern China (the ECH region) presented in Figure 3 is an example, with the two MODIS instruments and MISR producing significantly different estimates. To compare trends, as done here, the situation is further complicated by the combination of relatively short observational records and interannual variability. Despite the complications, several clear conclusions are drawn, some of which are summarized on Page 14050 lines 5-8:

'In general, the model captured historical AOD trends of 21 years in most regions, including the continual increasing trend in ECH [east China]; decreasing trend for EUS [eastern US], EUR [Europe] and NAT [North Atlantic], and the decreasing in 1990s but increasing trend in 2000s in SHR [Sahara and Arabian desert] and NPA [North Pacific].'

The authors characterize the trends over ECH as 'continually increasing' but the trend estimates presented in their Table 2 show AOD over ECH decreasing over the 1990s for the two satellite records available, while for the 2000s one record is slightly negative, three others are weakly positive and one (SeaWIFS) is more definitively positive. None of the satellite-derived trends are statistically significant at the p=0.05 level and the model trend is roughly four times larger than the largest positive satellite estimate. The comparison of satellite observations is considerably more coherent over EUS and EUR, both between satellites and with the model estimates, though none of the trends are significant at the p=0.05 level.

I find that the authors have done an admirable job of comparing a comprehensive set of observations against the model simulation and I am sympathetic to the desire to draw conclusions from the available observations. However, I would suggest the authors take a more rigorous approach to assessing the trend estimates, particularly the uncertainties in the trends, and avoid the desire to jump to conclusions. It is mentioned in the text (Page 14035, Lines 15-17) that the statistical significance of trends was tested and the results are noted in Table 2, but the significance is not otherwise discussed in the text of the manuscript. Not wanting to 'throw out the baby with the bathwater', which would seemingly occur if trends were only assessed at the p=0.05 level, I would suggest assessing the trends at both p=0.05 and p=0.1 (perhaps even p=0.2) levels of confidence and including a discussion of the differences in the significance of the trends in the text. The limitations of the data must be recognized and my hope here is that the clear negative trends in EUS and EUR can be assessed at the level of significance allowed by the data and that these regional trends can be contrasted against the puzzling lack of any positive trends in AOD over ECH.

My other concern is the use of the term 'non-feedback' to describe the second model experiment. It is stated that the distinction between the 'feedback' and 'non-feedback' experiments is that only the feedback case has 'aerosol direct radiative effects updated in the rapid radiative transfer model...'. Does this mean that the non-feedback case uses climatological aerosol fields in the model radiation code? Even more fundamentally, if the comparison of quantities like TOA shortwave radiation (SWR) shown in Figure 7 is taken from the output of the model and the output results from the input of a constant aerosol climatology, what is being compared? I'll note that the 'no feedback' case shown in Figure 7 produces an almost constant TOA SWR for regions where the feedback case shows significant trends. My concern here is that in the literature 'feedback' and 'no feedback' is used to distinguish between setups where the internal model chemical fields (gas-phase and/or aerosols) are, or are not, allowed to affect the dynamical fields, but in both cases the aerosol fields are calculated internally within the model. One may find systematic effects by allowing these feedbacks, but the model aerosol fields are still largely the same. Here there seems to be two different representations of the aerosols producing two fundamentally different effects on aerosol quantities like AOD. I would suggest a less ambiguous term than 'feedback'

to differentiate the experiments along with a more complete description of how the 'non-feedback' experiment is setup. Further, if the non-feedback experiment uses a climatological aerosol, is there any validity in comparing the estimates of direct radiative efficiency and correlations with observations as shown in Table 4?

Specific minor comments are given below.

Page 14031, Line 25 to Page 14032, Line3: As stated just above, a more complete description of how the non-feedback experiment is designed would help the reader.

Page 14035, Lines 14-17: That trends were estimated separately over the 1990s and 2000s eventually becomes clear, but the process of separating the decadal trends should be introduced clearly.

Page 14035, Line 23: It is here that Figure 2 is introduced. Can I suggest that regions without data be shaded grey in the panels showing trends to allow differentiation from regions with small trends? And are the trends shown in Figure 2 calculated over the full length of the record for each satellite? There is an oblique reference at Page 14036, Line 21 that suggests the trends are all calculated for the post-2000 period, but it should be clearly stated in the figure caption.

Page 14036, Line 27 to Page 14037, Line 4: It is the JJA season that is analysed here and I understand the biomass burning in south-east Asia (Myanmar, Thailand, Laos, Vietnam) peaks earlier in the year – February through April (see van der Werf et al., Global Change Biology, 9, 547-562, 2003). Are the authors certain that problems with biomass burning are the cause for the discrepancy between the modelled and observed trends over south-east China?

Page 14037, Lines 16-19: The discussion here ties the declining AOD found in the north Pacific (NPA) with the declining AOD in the eastern US (EUS). Given the west to east transport of pollutants, is it realistic to think that declining emissions (assuming the decline in AOD is directly related to emissions over the source region) in EUS contribute to a decline in aerosol amounts three-quarters of the way around the world? There has been some very interesting work tying decadal-scale trends in ozone at Mauna Loa with shifts in the large-scale circulation. I would suggest this as a more probable, but still speculative, possibility.

Page 14040, Lines 1-3: Account must be made of the fact that the Mauna Loa AERONET station sits at an elevation of 3400m. This would seem to be the reason for the very low AOD shown in Figure 5.

Page 14040, Line 26 – The text says the SWR at TOA shown in Figure 6 is for 2000 and 2010, though the text states 1990 and 2010.

Page 14047, discussion beginning at line 17: I had tremendous difficult with the discussion of how E_{t*} becomes smaller than E_t when AOD levels are higher. Given that E_t and E_{t*} are measures of the radiative efficiency and the radiative efficiency per unit AOD measured by E_t decreases at higher AOD, then E_{t*} should be larger than E_t. But I believe the reason the discussion is structured the way it is, is because the DRE is negative and so larger negative values are smaller? For example, for the SHR region shown in panel (a) of Figure 10, the E_{t*} is -18*AOD, but at values of AOD=1, the individual data points have DRE/AOD values of ~ -12. While E_{t*} is smaller than E_t numerically, the estimate of the physical effect of aerosols on DRE given by E_{t*} is larger than that given by E_t. This is my interpretation of the use of the word 'smaller', but perhaps I am not correctly following the argument? In any case, I would suggest a bit of clarification in the discussion to make it easier to follow. Perhaps defining E_t and E_{t*} using the absolute magnitude of DRE so that the numerical relationship between E_t and E_{t*} is consistent with the physical interpretation?

Page 14048, Line 8 and Line 10 – note the inconsistent use of E_{2t} and E_{t2}.

Page 14048, Lines 13-17: This argument is difficult to understand, particularly the phrase '...though the values of TOA-E_{t*} are smaller, but still slightly larger...'

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14027, 2015.