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Discussions

Interactive comment on "Assessment of multi-decadal WRF-CMAQ simulations for understanding direct aerosol effects on radiation "brightening" in the United States" *by* C.-M. Gan et al.

C.-M. Gan et al.

chuenmeei@gmail.com

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We thank the reviewer for the overall positive assessment of this study and for providing helpful comments that have helped us in improving the manuscript.

Major points 1) Section 3.2., trends in AOD and SW radiation, is particularly unclear and difficult to read with frequent changes from all sky to clear sky to direct and diffuse and back, in model and observations, and intermixed with (maybe a bit too much) speculations on why a certain feature may be seen in the observations or may not

be captured well by the model. This section in particular would benefit from a more structured, point by point discussion (model, observation, east, west) of all sky SSR, clear sky SSR, direct, and diffuse. Looking at Table 5, points that may be worthwhile addressing include: Clear sky trends (observations show stronger trends in the west than in the east, the opposite is true for the models; observations are dominated by the diffuse component, while direct and diffuse essentially compensate in the model when aerosols are present; given the different relevance of diffuse and direct in model and observation, as well as the different sensitivity of diffuse and direct to aerosols, one may wonder whether aerosols indeed play a dominant role for observed clear sky trends). These discrepancies between the model and the observations particularly the differing trends in clear-sky direct and diffuse SW are discussed in some detail in Section 3.2. All sky trends (stronger in observations than model, independent of aerosol feedback in model; much stronger than clear sky trends except for observations in the western US; the direct component dominates except for observations in the west; comparing east and west, modeled direct and diffuse are comparable without aerosols but are clearly different if aerosols are included).

Following the reviewer's suggestion, Section 3.2 is reorganized and simplified to minimize repetition of some points. The discussion points suggested by the reviewer are included in the revised version of Section 3.2.

2) The paper is, as mentioned above, a follow up of Gan et al. (2014, ACP) and Xing et al. (2015, ACP). This should be stated more clearly. Figure 1 and Table 1 are identical copies from Gan et al. (2014, ACP). This may be acceptable in order to have a self-contained manuscript, but it should be made clear. Similarly, the observational trends presented in Tables 3, 4, and 5 are identical to results given in Table 2 of Gan et al. (2014, ACP). Section 2.3. follows most closely section 2.5 in Gan et al. (2014, ACP). It may be also worthwhile to mention that site specific time series can be found in Gan et al. (2014, ACP).

The reviewer is correct that the present study builds upon Gan et al. (2014a, ACP) and

Xing et al. (2015, ACP). This has been clarified in the revised manuscript on page 3, page 5 and the caption of Figure 1.

Minor points: p. 17718, line 16: "In general ... the model output and emissions agreed well with CASTNET measurements...". One may add (in line with Xing et al. 2015) that this is not true for NO3, where even the sign of the trend is different in observations and model.

The NO3 trends shown in Figure 2 and Table 3 are directionally consistent between observations and simulations for both the Eastern and Western U.S.. As noted by the reviewer, this is indeed different from the results of the analysis of hemispheric CMAQ simulations presented by Xing et al. (2015). A likely reason for this difference in model behavior is that the present study used a different emission inventory than Xing et al. 2015 to drive the model. In particular, the Xing et al. 2015 study used the global EDGAR inventory while the present study used the 1990 – 2010 emission inventory developed by Xing et al. (2013) for North America.

p. 17719, line 24: "One of the possible reasons... such as sea salt, wild fires and underestimation of secondary constituents...". It seems not obvious how deficiencies in sea salt or wild fire emissions could affect aerosol trends. Being natural sources they are likely more or less constant, no? And how about other possibilities, e.g. aerosol properties (internal / external mixing, hygroscopicity, optical properties)?

This statement in our original manuscript referred to the underestimation of AOD, not the underestimation of aerosol or AOD trends. We have clarified this point in the revised manuscript and have included additional explanation and a discussion of other possible causes for the underestimation of AOD in the revised manuscript: Page 8 line 221 – page 9 line 237

p. 17720, line 6: "One of the possible causes... aerosol indirect effects". How about inaccurate cloud representation already in the absence of aerosols? Due to inaccurate boundary conditions, nudging, etc.?

In general, these factors may have effects in the simulation but we believe they are not the main factors in this underestimated trend. We tried to minimize these effects by using boundary conditions from a hemispheric simulation (these BCs are likely more representative of long term changes in inflow conditions) and using a moderate nudging to ensure that the model dynamical state does not diverge relative to the observations (see Hogrefe et al., 2015). This is further clarified in the revised discussion on page 10.

p. 17720, line 9: "Aerosol indirect effects have recently been included...". You mention this again a bit later, p. 17722, line 2. It may be easier for a reader if you discuss these (and other) potential short comings in one place instead of repeating them several times / scattering them throughout the text. After all, the title of your paper is 'assessment of WRF-CMAQ. Admittedly, though, this may not make sense in each case.

Yes, we agree with the reviewer's concern that the current discussion can be confusing. This section is reorganized and simplified.

p. 17720, line 25: "These anomalies are likely associated with the very strong El Nino...". To me, it is not obvious how even a strong El Nino should affect clear sky SSR in the US. Could you give a reference?

To further clarify the point, we have added the text below in the revised manuscript, page 11. These anomalies are likely associated with the very strong El Nino occurrence of 1998-1999 which had significant impact on continental US weather patterns. For example, El Nino affects (i.e. increases) the rain and snow fall, water vapor and temperature in the atmosphere. As discussed in Long et al. (2009) and Gan et al. (2014), we allow some amount of condensed water in the atmospheric column under the "clear sky" classification. Dupont et al. (2008) show that up to an optical depth of 0.15 of primarily elevated ice crystals are still typically classified as clear sky. Augustine and Dutton (2013) show using SURFRAD data that there exists a moderate correlation between ENSO, and surface air temperature and surface specific humidity at the SURFRAD sites. Their Figure 7 shows the 1998-1999 El Nino increasing the yearly average specific humidity, with Bonneville and Goodwin Creek sites exhibiting the greatest increase of almost 1 g/kg. This increased humidity likely also increased the occurrence and/or amount condensed water in the atmospheric column at levels still classified as clear-sky, yet as shown in Figure 8 had an impact on the partitioning of the downwelling clear-sky SW, significantly decreasing the downwelling direct SW while increasing the diffuse SW. In the west, the decreased direct SW anomaly is about balanced by the increased diffuse SW, but not so for the east where the decrease in the direct SW is much larger.

p. 17720, line 28: "... may also be due to errors in model representation of emissions...". If I understand correctly, you are still referring here to two years that do not match well between observations and model. Why should the emissions be wrong only for these two years?

This discussion is attempting to convey that estimates of emissions for recent years has benefitted from the availability of more detailed activity and other auxiliary data then estimates from the early 1990s. The statement "... may also be due to errors in model representation of emissions...". has been rephrased on Page 11 of the revised manuscript

p. 17721, line 15: Same question as p. 17720, line 6. Why does it have to be indirect aerosol effects and not just "misrepresentation of clouds" in general?

We agree that the representation of clouds in general is subject to considerable uncertainties, this includes the aerosol indirect effect not currently included in our model. This sentence is no longer part of the rewritten Section 3.2 in the revised manuscript.

p. 17722, line 2 and line 16: Both comments (on indirect aerosol effects and "whitening") you already had just one page earlier. Maybe no need to repeat them in the same section so frequently.

Thank you for pointing this out. The discussion is modified and the repeated portion is removed.

p. 17722, line 8: "This is further verified through the comparison of the feedback (FB) case...". The sentence seems a bit daring, given that the observed clear sky trend is dominated by the diffuse component, while the modeled trend is dominated by the direct component. Personally, I think this disagreement between model and observation (dominance of either direct or diffuse component) is among the most interesting findings of your study. It points, as you write yourself, to other relevant factors for clear sky trends beyond (simple) direct aerosol effects.

This sentence is rewritten on page 10 line 284 – page 11 line 287 One of the indications that the aerosol direct effect is contributing to the “brightening” is shown in comparison of the feedback (FB) case with the no feedback (NFB) case. As illustrated by the data tabulated in Table 5, almost no trends is apparent in the no feedback case for clear-sky total, direct and diffuse SW radiation.

p. 17722, line 27: "...in particular in the last 3 years (i.e. both of them decrease)". In the west it would be only the last 2 years. More generally, I find it doubtful to rely on only two or three years of data here.

The overall conclusion was made based on several indications and references. On page 12 line 327 – 335 of the revised manuscript, we have revised the discussion to address the reviewer’s concern as follows: For example, as a result of the increasing air traffic, ice haze layers associated with aircraft emission contrails (Hofmann et al., 1998) can potentially increase the diffuse radiation. More support for this theory was presented by Gan et al. (2014a); the pattern of US air carrier traffic (i.e. steady growth of air traffic from 1996 to 2007, followed by a decrease after 2008) agreed well with the pattern inferred in the observed clear-sky diffuse radiation especially during the last 3 years (i.e. both of them decreased). Moreover, Haywood et al. (2009) and Gerritsen (2012) illustrated that increasing contrails do increase the diffuse radiation.

This suggests that contrails or sub-visual cirrus clouds and ice haze can play a role in the increasing trend noted in the observed clear-sky diffuse SW radiation.

p. 17723, line 22: "In particular, analysis of model and observations of clear sky total SW trends... agree well...may be due to better estimates of recent emission data sets." The concrete formulation is certainly correct. However, again, personally I find the really interesting fact here the difference between diffuse / direct in model / observations (see above).

Please see our detailed response above.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 17711, 2015.