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Atmospheric Chemistry and Physics 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.

Anonymous Referee #1

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The paper investigates to what degree the WRF-CMAQ regional model (including direct aerosol effects) can reproduce increasing trends ('brightening') in surface solar radiation (SSR, under clear sky and all sky conditions) at seven sites in the eastern (four sites) and western (three sites) United States (US) from 1995 to 2010, as well as associated trends in aerosol burdens and aerosol optical depth (AOD). The paper finds overall good agreement between modeled and observed trends of the above quantities. Discrepancies between model and observations exist if direct and diffuse SSR are considered separately.

The study is a follow up to Gan et al. (2014, ACP) an Xing et al. (2015, ACP). In the former paper, most of the observational data presented here has already been presented. In the latter paper, modeled (WRF-CMAQ as well, but coarser resolution) and observed air quality trends (1990 to 2010) are compared for many more sites (in the US, but also in Europe and China) than in the current paper, while SSR is not considered.

Simultaneous, long term (1995-2010) co-located observations of both aerosols (burdens, AOD) and SSR (all sky / clear sky and even direct / diffuse component!) as presented here are scarce. They are highly attractive for model assessment, as they allow simultaneous assessment of cause (aerosol related trends) and effect (SSR trends). The insight gained from this comparison, including in particular the distinction between diffuse and direct SSR components, is clearly fit for publication in ACP. However, the presentation of the results currently lacks clarity in relevant parts, which is why I recommend major revisions.

Major points

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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 compo-

nent, 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).

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).

2) The paper is, as mentioned above, a follow up of Gan et al. (2014, ACP) an 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).

Minor points

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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.

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)?

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.?

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.

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?

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?

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?

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.

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.

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.

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).

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