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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Anthropogenic aerosol radiative forcing in Asia derived from regional models with atmospheric and aerosol data assimilation" *by* C. E. Chung et al.

Anonymous Referee #1

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This work estimates the anthropogenic aerosol radiative forcing in Asia by using 3 different kinds of regional models, a climate model for meteorological variables, a CTM for aerosol constituents, and a radiative transfer model for calculating aerosol forcing. There is no interactions or feedbacks between aerosol, meteorology, and radiation. While this work adds values to the current assessment of aerosol direct radiative forcing over Asia, significant revision is needed. My comments are given below.

1. Data uncertainties. This work assimilates MODIS and AERONET aerosol data without understanding the data limitations. In particular, the MODIS fine mode aerosol and the AERONET single-scattering albedo, both are known to have large uncertainties

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and limitations. The authors seem to just take the face value of these products with no error analysis.

2. Model simulation of aerosols. There are several issues. For example, the anthropogenic emission is fixed for the 4-year period, nitrate and secondary organic aerosols are not included, hygroscopic growth of organic aerosols are not considered. The uncertainty/error associated with these omissions should be addressed. Most of all, not much evaluations with measurements over the studied domain are given, especially in aerosol compositions.

3. Forcing. The reported forcing values are for all sky, while the assimilated AOD, SSA, etc. are for clear sky only. This is certainly not consistent. At least the clear sky forcing value should be reported. Also, the forcing values are break down to "anthropogenic forcing" and "BC forcing"; is BC not a part of anthropogenic aerosol?

Specific comments:

p. 822, first para: "high resolution" is an arbitrary term. Actually a regional model with 60-km resolution can be considered coarse, and now global models can achieve spatial resolution at 0.5 deg. I suggest delete that term.

p. 822, the end of second para: The NCAR/CCSM3 is out of context. Also, this is a very minor part in the text and should not be included in the abstract.

p. 823, line 4-8, aerosol radiative forcing: The definition here is different from IPCC, which defines forcing at the tropopause relative to the pre-industrial state. If you use different definition, you should say "aerosol radiative forcing defined here is. . ."

p. 823, line 18: In addition to AOD and SSA, aerosol phase function or asymmetry factor is needed for calculating aerosol forcing.

p. 823, line 23: This is an overstatement. How do you only retain all the advantages but get rid of all the disadvantages?

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p. 824, line 2: Why is Giorgi et al. 2002 specifically mentioned, and is it particularly relevant to this work? There have been many regional model simulations of aerosols.

p. 826, line 5: "STEM-2K1" is used here but introduced later in section 2.2.

p. 826, line 15-16: Both STEM and STEM-2K1 are used throughout the text. Should have a consistent term to use.

p. 826, line 19: Again, 60-km is not such a high resolution for regional model. Delete "high resolution".

p. 827, line 10: Can you estimate the effects of not considering anthropogenic emission changes on your results? Can you apply a growth factor for different years?

p. 827, line 21: Organic aerosol is known to grow hygroscopically. At a minimum, possible error from this omission should be estimated.

p. 828, line 8: Why use 1 deg x 1 deg MODIS cloud products while the models are running at 60-km?

p. 828, line 14: Does STM model also provide SSA and phase function?

p. 828, line 23: What is the spatial resolution of MODIS level 2 daily data?

p. 829, line 2-3: The original references of MODIS and AERONET should be sited here instead of using Chung et al., 2005 as a data reference.

p. 829, line 3-5: Problems: (a) There are only very few AERONET sites over Asia, and essentially none over the large source regions in China during the study period of 2001-2004. Please indicate what the sites are that are used in your assimilation. (b) MODIS find/coarse mode separation over land is qualitative, not quantitative. There are very large uncertainties in using the MODIS fine mode aerosol products over land, and this uncertainty should be addressed. Maybe you should discuss this with the MODIS aerosol team to have better handle on the errors.

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p. 830, line 23-24: How about other properties beyond specific extinction, e.g., size distribution, absorption, and phase function?

p. 831, line 8-9: Putting all dust and sea salt into coarse mode AOD can introduce large errors since fine mode dust and sea salt could contribute to 20-40% of dust and sea salt AOD. I don't understand why this artificial separation is necessary – you have find and coarse mode dust and sea salt simulated separately by the STEM model, why don't you just use them accordingly?

p. 831, line 14-16: here AERONET data were very limited during 2001 over Asia, either by the number of sites or by the time duration of observations. How do you adjust the AOD over the large area where no AERONET data are available?

p. 831, line 22: STEM filling the gap: How do you deal with the discontinuity between assimilated AOD and model-only AOD?

p. 831, line 25-27: The assimilation technique does not provide any vertical information and composition, as you only assimilate AOD. Therefore this information comes straight from STEM model, not from the assimilation. Also delete the word "accurately". You don't know how accurate the aerosol results are.

p. 832, line 7: can you be more quantitative than just saying "reasonably consistent?" Do they agree with observations to within 30%? 50%? A factor of 2?

p. 832, line 17: Do you know the reason of the difference between Chung et al. 2005 and the present work? Is this because of the model resolution, or emissions, or something else?

p. 832, line 23: what are included in the anthropogenic AOD? Is BC not considered as "anthropogenic"?

p. 832, line 25: Dust and open biomass burning are not evident in Figure 4.

p. 832, line 27-28: How distinguish is high BC in Asia from the rest of the world? You

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should provide some number such as the ratio of BC AOD to total AOD for Asia in contrast with that ratio over other regions. Otherwise it is hard to discern the feature based on the Figure.

p. 833, line 8: How does STEM compare with AERONET in Kanpur?

p. 833, line 14: Why do you use AERONET 2005 data but simulate 2001-2004 period? What is the period of "AERONET new" data? After 2005?

p. 834, line 11-12: Show your own comparison with AERONET and OMI. Koch et al. 2009 is not relevant here.

p. 834, line 26-27: I don't understand this sentence. How can observed vertical profile is a results of blending BL and lifted structure?

p. 834, 28-29: I don't see any BL structure from model simulation in Figure 6. The vertical profiles are very smooth, i.e. lack of any structure.

p. 835, line 2-4: The comparison with Textor et al. 2006 is not appropriate, as Textor et al 2006 showed global annual average vertical values, including land and ocean, but this work showed the profile at a specific location in the Indian Ocean during March 2006. There is no information to assess if the BC vertical profile in Figure 6 is "an improvement" over Textor et al., 2006. In addition, it is an common sense that the concentration in the boundary layer is much higher than in the free troposphere over the source regions, but the BL concentration can be lower than that aloft over transported regions such as over the Indian Ocean.

p. 836, line 5: Again, is BC anthropogenic?

p. 836, line 11: If the radiative transfer model is the same, what are the differences in aerosol properties between these two studies that lead to the difference in forcing?

p. 836, line 15: How do you use the AERONET SSAs? BC is not the only absorbing component here. Dust absorption is also very significant. How do you attribute the

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SSA to BC and dust components?

p. 836, line 22: How do you use AERONET SSAs? AERONET SSAs is not a direct measurement, but a retrieved quantity. It can only be used when AOD is above a certain value (0.3 or 0.4). Also, how do you deal with the spectral dependence of AOD, SSA, and phase function with aeronet data at only a few wavelengths?

p. 838, line 19: Comparisons with CCSM3: This comparison does not provide any helpful information as the way it is presented. Are the aerosol fields the same? What is the reason for such large differences? What is the point you try to make? I suggest either not include this comparison, or much more analysis should be done.

p. 850, Figure 2: You could mark the AERONET sites on at least one of the maps.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 821, 2010.