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Comment

Interactive comment on “AOD trends during 2001–2010 from observations and model simulations” by A. Pozzer et al.

Anonymous Referee #2

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This paper presents 10-year trends (2001–2010) of AOD in different world regions with data from satellite retrievals from MODIS, MISR, and SeaWiFS and a global model EMAC. Using the model experiments with fixed anthropogenic and biomass burning emission and information of aerosol components, the authors attribute the trends in different regions to the change of emission or meteorological conditions or both. They conclude, by examining the AOD trends over 7 selected regions, that AOD trends over the US, West Europe, East China, and South Asia are mainly controlled by the change of anthropogenic emissions, but over Sahara and Middle East the meteorology plays a major role in the increase of AOD over the past 10 years. On the other hand, over Southeast Asia, both biomass burning emission and meteorology are equally important for the decreasing AOD trend. Moreover, the authors have found that it is the

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water-soluble components that are the dominant contributors to the AOD trends over most regions mainly because of the change of water content associated with those hydrophilic aerosols.

Overall, I found some of the results are very interesting (aerosol water), some are confirmative (anthropogenic emissions responsible for the AOD trends over polluted regions), but some are rather ambiguous (role of meteorology). In any case, the analysis should be done in more depth. My major comments are listed below.

1. Aerosol water: It is striking that the change of aerosol water contributes to 70-90% of the increasing/decreasing trends in regions (a)-(g) (Table 1). To be more convincing and confident with the results, it is necessary to have some verification on the credibility of water uptake by the soluble aerosols. I suggest the authors compare with field measurements on dry and wet extinction under different RH to verify the water uptake parameter used in the EMAC. This seems a necessary step to confirm the results.

2. Trends over different regions: There are significant interannual variabilities in the 10-year period that should be take into account, such that it may not be appropriate for a linear fitting to get a “trend”, especially over biomass burning and dust dominated regions. For example, biomass burning emission in SEA has a general increasing trend from 2000 to 2006 (2006 is a high biomass burning activity year in SEA) but decreases in the later years. A linear fit is certainly misrepresenting the interannual variability. Therefore, it is important to show the time series of AOD in each region to see if the linear fitting is appropriate.

3. Comparisons with satellite data on regional trends: (a) Although the direction (positive or negative) of trends are similar between the satellite data and the EMAC model in some regions, they are very different in other regions, especially South America and southern Africa where biomass burning dominates. The authors offer no explanations of the reasons of discrepancy. I strongly encourage the authors to take a look at the time series of AOD in these regions, which will reveal the degree of agreement of AOD

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and provide information on why the trends are opposite. It is very possible that a linear fitting is a problem. (b) The model results were sampled at the satellite overpass time (with the “submodel SORBIT”) in order to have proper comparisons with the satellite data. However, it is not clear if the model and data are also matched spatially such that the regional monthly or annual averages for the model are calculated with the output that match both the time and the location of satellite observations. I wonder how much difference is there between the model-calculated regional averages with and without considering the matching time. (c) The linear fitting of trends in Figure 5 does not provide meaningful information. The points in the shaded area indicate that the model and data have different direction of change; these points require more attention to understand why. Plotting 1:1 line would be more informative to show how close the model and data are in terms of the direction and magnitude of the trends. Points of NH, SH, and GL (i, j, h) are not even discussed in the text; either remove them from the figure or add discussion in the text.

4. Role of meteorology in influencing the trends: The analysis of the role meteorology play is rather weak. When the trends cannot be explained by emission change alone, the authors give the reason that it must be “meteorology”. What meteorological variables are the determining factors? Wind strength, directions, precipitation amount, cloudiness, or something else? Can the changes (or trends) of these met variables explain the change of AOD in regions not dominated by anthropogenic aerosols? This information can be readily obtained from the model and should be presented in the paper as a part of trend analysis.

5. Comparisons with AERONET: The only model evaluation shown in this manuscript is in Figure 1 of scatter plots of AOD at all AERONET stations. At a minimum, such comparison should be stratified in various regions to show if the model has regional biases, and show regional statistics.

Specific comments:

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Page 26620, line 3: It would be more informative to include the model name here instead of just “model simulations”.

Page 26625, line 18-24: Did you just use one year dust and sea salt emission from AEROCOM, i.e., no interannual variations? Without changing dust and sea salt emission through out the 10-year simulation would prevent you from understanding the trends contributed from the emission change. I understand that not every model has the dust and sea salt emission capability, but the uncertainties associated with those should be acknowledged and discussed.

Page 26626, line 10 and 13: Personally, I think “RCP85” and “RCP00” are not good experiment names, especially RCP00 that could imply a 0 W/m² forcing scenario in RCP (equivalent to RCP85 emission scenario leading to 8.5 W/m² forcing in 2100). I would suggest “BASE” and “FIXEMI” (or something like that nature) as experiment names.

Page 26628, first paragraph in section 4, comparison with AERONET: I disagree with the statement that “the variable time coverage of AERONET data make it difficult to have robust trend comparison”. There are a number long-term AERONET sites that have small data gaps. These data can definitely be used for deducing 10-year trends.

Page 26629, line 3: What “in situ observations” do you use here? AERONET is not “in situ” measurement; the sun photometer it is a ground-based remote sensing instrument.

Page 26629, line 4: What is “correctly”? This word is subjective. Is within a factor of 2 or 5 correct or not correct? Please do not use such subjective words but be quantitative.

Page 26630, line 4: Spell out “SD”.

Page 26630, line 9: I am not sure what “a small significant decrease” means. Sounded contradictory.

Page 26630, line 15-16: It is not true that “no aerosol retrieval is possible over bright

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surfaces” from MODIS. The MODIS Deep Blue algorithm has been working for many years to get the AOD over the deserts, and it has been a part of MODIS products available in Collection 5. I strongly suggest the authors get the MODIS DB data that will definitely fill the gaps in SD and ME.

Page 26630, line 20: “Sample along satellite ORBIT”: Is figure 3 showing the model results along the Terra overpass time? OrbView-2 and Terra have different orbit and different overpass time - how do you deal with that in Figure 3?

Page 26630, line 22-25: I don’t get the “general agreement” part. There are regions where model and data are similar, but there are also regions model and data are very different. There is no generalization. I see several differences between obs and model: East Asia - model positive trend is too strong; southern Africa - opposite trends; South America - opposite trends; Arabia peninsula - trend is much weaker in model. There is no general agreement. Also, not everywhere AOD trends are “anthropogenically driven”.

Page 26632, line 15-16: “This means that the variation in the emission did not affect the AOD trend in this region” – do you mean variations in the anthropogenic and biomass burning emission? Otherwise this sentence does not make sense.

Page 26632, line 21-22: Does your model show the decrease of precipitation in that region? What is the precipitation trend in your model?

Page 26632, line 26: clarify which region is “this region”.

Page 26633, paragraph about Sahara Desert: If emission is kept the same with no interannual variability, then what is the cause of the change of AOD in that region?

Page 26633, line 21-22: What kind of status, accuracy, and cloud contamination that would affect the trends? Do you have references talking about that? Can you be more specific, not just hand-waiving?

Page 26633, line 24: “Another reason” of what, model trends different from satellite, or

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satellite trends discrepancies? Clarify.

Page 26634, line 2-3: Be more specific - does emission decreased in the last decade? What meteorological parameters change that may cause the decrease of AOD? You have a model to provide insights and explain the reason.

Page 26634, first paragraph in section 6: Please clarify what the “water soluble” components are. The text sounds like that OC and sea salt are not water soluble.

Page 26634, line 11: “aerosol water content has the largest contribution to the total AOD”: Please quantify - what % of AOD is from water? Is there any observation to verify?

Page 26634, line 16-17: Why should the water uptake efficiency, which is an intensive property, decrease when the aerosol amount decreases?

Page 26634, line 21: How and why does natural dust increase? Emission is not changed at all. Need explanation.

Page 26635, line 4: Which “meteorological factors”? Winds? Precipitation? Please be specific.

Page 26635, line 4-5: From your model results, can you tell how much “hydrophilic coating” is on dust? Or is this just a guess?

Page 26635, line 13-16: Can you be more certain about the model diagnostics of the causes for those changes? Why do you guess when you should use the model diagnostics?

Page 26635, line 19: Again, what “meteorological conditions”?

Page 26635, paragraph about SEA: What are the fractions of dust and sea salt in total AOD that would matter? Also, as I said in the beginning, biomass burning emission does not have a linear decreasing trend at all in that region.

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Page 26635, line 23: What do you mean by “directly and indirectly”?

Page 26636, line 8-9: The statement “Despite some clear differences between simulated and satellite-derived AOD trends, the general patterns of trends in the aerosol extinction are well reproduced, also on a regional level” is over optimistic. As mentioned earlier, in several regions the trends are opposite.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26619, 2014.

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