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## ***Interactive comment on “Multi-decadal variations of atmospheric aerosols from 1980 to 2009: sources and regional trends” by Mian Chin et al.***

**Anonymous Referee #1**

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This paper brings together an interesting set of long-term observations and global aerosol model simulations, to show a general consistency of the model results with these observations. The analysis presented is exhaustive and robust. The paper reads much like a model validation paper- focusing on trends. This work without doubt merits to be published in ACP, but the authors could try to condense the technical discussions somewhat, and pay more attention to improving the abstract, discussion and conclusions to bring out the key findings better and make the paper more accessible.

Specifically, I would recommend to emphasize more:

1) what are the new outcomes and progress? In my perception this work seems of higher quality and the model performing better than other studies I have seen- but the

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paper needs to demonstrate this better by referring back to older papers.

2) Currently the paper is very lengthy- which makes it sometimes difficult to understand what point the authors want to make. It would be very good if the authors could summarize at the end of each section what are the overall findings and robustness. I also suggest to construct a summary table where the main findings regarding regional trends are presented, including a quantitative and qualitative discussion.

3) where are model results in agreement with the multiple sets of observations (with uncertainty margins) and what are the aspects that we do not understand well.

4) if possible highlight where this specific model stands in a the crowd of other global model results- or with other words- what aspects can be expected to be model specific- and what aspects can be expected to more generally reflect our knowledge on changes in aerosol.

Once again I appreciate the rigor of this paper, and my comments above are merely meant to further improve the paper. In the detailed comments below, I will at some place indicate possibilities for improvement.

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Detailed comments: p. 19752: I find the title not very adequately covering the contents of the paper: there is relatively little on sources; it doesn't tell that this is a study covering the globe; it is about surface and column aerosol. Suggest: A global model analysis of regional surface and column aerosol trend variability from 1980-2009.

p. 19753 I.4 global satellite observations and ground based networks mainly covering Europe and Network, with some information elsewhere.

p. 19753 I.7 Consistent quantitative or qualitative? Would be good to give numbers.

p. 19753 I. 10-15 Does the model accurately describe the dust fluctuations?

p. 19753 I. 15 tropical North Atlantic: do you mean to say here that the dust is more

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effectively removed? Can you say something on the relatively importance of the two processes?

p. 19753 I. 19: Is it meant global-land average and global-marine average? Or just global average?

p. 19753 I. 20 In general regional assessments are useful- especially for aerosol with lifetimes of days-to one week. Global aerosol trends are mostly relevant for climate issues ('global dimming','global brightening'). I think the authors want to say something like that opposing regional trends make any estimate of global trends over the last 30 years difficult.

p. 19754-55 The introduction already contains quite some details on measured trends- I am wondering if it is not more appropriate to include details in the measurement section, and try to sketch the general picture first.

p. 19758 I. 3 Size? aerodynamic diameter?

p. 19758 Do the satellite retrievals assume a similar non-spherical treatment of dust. With other words are the model and satellite (and Aeronet) results more comparable?

p. 19759 The authors are preparing us for some issues regarding the biomass burning emissions. Likewise can they say something about this extrapolation of 2000 emissions with scenario calculations? What are the inaccuracies?

p. 19759 On my printer equation 1 didn't show well. Please check if this is general problem.

p. 19760 The description of dust scheme is interesting but detailed. Move to appendix? I think the main issue is that the dynamic source model has improved by including surface wetness. Do other models include this as well?

p. 19760 emission variability how does it compare to other published studies? For instance the Pozzoli et al. (ACP. 2011); seem to have a much larger variability of

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global mineral dust; with an approximately 10 % relative standard deviation. Further emissions were much lower than the ones in this study. Can the authors discuss why they think the emissions here are better. As a technical remark I think it is difficult to see the regional variability of the emissions- it would be great to see similar regional plots – or perhaps just make the data available in a spreadsheet for future modeling activities by other groups? Please pay attention that 2b in the ACP version will be readable (I had to enlarge it a lot in the on-line ACPD version).

p.19761: Something needs to be said about the fact that the use of of-line oxidants doesn't assume the separation of natural / current (standard) conditions, thus avoiding one source of non-linearity. But what about non-linearities in aerosol dynamics? How accurate is the attribution of the difference of the full simulation and the natural simulation to 'fossil/biomass burning'. Although I understand that the authors are not keen on performing a 3rd set of simulations (fossil/biomass) alone, I would recommend that verification of this assumption with a couple of years of sensitivity studies is needed.

p. 19762 Please explain better the difference between the two AVHRR datasets. In this section a discussion on the accuracy of the datasets is needed. The authors choose not to use ATSR-2 (Thomas et al) any reason?

p. 19764 'few' sites. How many and which ones? Figure 3 seems to suggest that there are many sites- but perhaps this overview of all AERONET sites, not of which ones were used? p. 19765 Section 4.1 gives trust in the general performance of the model (explain why 2001 was chosen?). This collection is truly impressive (make sure that it remains readable in final version). I think it is useful to have the global/ocean/land averages in the panels. It is somewhat hard to read from Figure 5a/5b the corresponding accuracy of regional averages- the plots are quite busy. While I understand there are some issues with the TOMS (over land); but what are skills of the model to reproduce variability over the period 2001-2010 when MOIDS/MISR/SeaWiFS data were available. And how to interpret the data? Is it fair to say that were various satellite datasets correspond (e.g. region 9; 13); the model is not performing very well, but

what to say about SAS and EAS; where the models seem to correspond better with MISR/SEAWIFS than with MODIS.

p. 19768/figure 5b the correspondence at northern hemispheric ocean regions is remarkable, as is the lack of correspondence in the SH. Somewhere the authors declare that the contribution of seasalt is not large, but the problems in figure 5b region 9-12 seem to suggest something else. Is the seasalt source function adequate, is the variability of the assimilated winds in the SH (driving Seasalt) enough? Or should the authors look more into DMS, which is a function of wind but also plankton variability. Please comment.

General comment: the interannual variability is strongly determined by Pinatubo eruption- and it seems accurately reproduced by the model. I wonder what picture would emerge when removing the stratospheric sulfate contribution from the model (and equally form the observations)? What fraction of the interannual variability would be reproduced?

p. 19769 l. 12-16 it is difficult to see these trends by eye- it would be good if more qualitative statements can be made- including the comparison with the emissions trends and earlier studies.

p. 19770 l. 29 Here an issue with Modis-Terra is mentioned- elsewhere issues with other instruments were mentioned. This is of course not surprising. I would however find it useful if the authors could work on a more general way to evaluate their model results. When are the retrievals and trends in retrievals robust enough to make statements on discrepancies and consistencies of the model evaluations. In this particular case a statement is missing on why an off-set would exist in SEA-SHL-SAM-ANZ and not elsewhere?

P. 19771: General comment: the choice of large ocean regions precludes the analysis of outflow regions trends (e.g. North America); but also the Arabian peninsula- where large satellite trends were observed- but somehow not so visible in this analysis. Can

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the authors look into this?

p. 19771 The two explanations offered on missing sources would probably deteriorate the model performance.

p. 19772 I. 13: Do I understand correctly that the author find a large contribution of volcanoes (Pinatubo; El Chichon) in maritime aerosol. Or do the authors want to say that AOD over marine regions in those periods is determined by stratospheric aerosol (not maritime aerosol). I would urge the authors to present an appropriate evaluation of surface seasalt aerosol – a number of years ago S. Gong has presented a rather rigorous evaluation- data could be retrieved from him?

p. 19773I. 29 Again I suggest the authors bringing this extensive and interesting evaluation to some point on what we learned on the model performance? What is the more likely case?

p. 19774 I agree with statements made on the caveats of the choice of 2 years at the beginning and end of the respective periods. As earlier indicated, why didn't the authors try to correct for stratospheric aerosol after eruption. Using model results to derive tropospheric aerosol columns for both model/satellite would be one way. Using SAGE to get measured stratospheric aerosol could also be a possibility. More realistic trends analysis would be possible.

p. 19777 I. 28 discussion on uncertainties should be elsewhere.

p. 197880-82 The authors show snapshots of a couple of stations with surface observations. While this is useful- the authors should explain why these particular stations were chosen. How valid is the site-specific discussion for a larger set of data? I understand that a comparison of a larger set of observations is analysed in Table 3. This table should mention the amount of stations and requirements regarding record length, data completeness, etc were made.

p. 19785 The relationships between emissions and AOD are interesting (Figure 9)

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especially when similar analysis can be performed for a range of models. Important factors determining these relationships are indeed local emissions and formation, long-range transport- and removal. I wonder why the authors have not utilized the simulations performed in HTAP to get a handle on the fraction of aerosol column/surface aerosol that is transported from elsewhere. Also why is the 'zero' anthropogenic emission case not included in the analysis. A sensitivity analysis with 1 meteorological year- and an high/low emission case could give insights in the sensitivity to chemistry; whereas two different meteorological years and 1 emission case could do likewise for the role of meteorology. With other words there are some opportunities to quantify the drivers behind the regional differences in these relationships.

p. 19789 Again an interesting analysis that would be almost worth an deepened analysis in a separate paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19751, 2013.

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