Atmos. Chem. Phys. Discuss., 11, C12638–C12644, 2011
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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Simulation of mineral dust aerosol with piecewise log-normal approximation (PLA) in CanAM4-PAM" *by* Y. Peng et al.

Anonymous Referee #2

Received and published: 2 December 2011

The authors present the new developed dust scheme based on the method of piecewise lognormal approximation. This scheme was implemented in the Canadian Atmospheric Global Climate Model with the PLA Aerosol Module (CanAM4-PAM) and different years were simulated for model validation. The authors compare the model output against surface concentration measurements, total deposition data and satellite retrieved aerosol optical depth (AOD). However additional datasets are available for the performance assessment of dust models that would allow to further explore the model performance. Huneeus et al. (2011) conducted an exhaustive and extensive model intercomparison of multiple models against each other and against multiple data sets. The data used in this study have been made available to the community for their

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use in model validation. The authors should include in their validation some of these datasets or at least justify why they prefer not to use them. In addition, the authors give the impression not to have given a thorough revision to the literature on the subject; some important articles relevant to this work have been neglected. Below is a list of comments explaining why this work needs major corrections before its publication.

General Comments:

The authors present the simulations conducted to validate the model briefly at the beginning of section 3 and continue to present aspects of the simulation as different data are used. It is not clear to the reader at the beginning of section 3 which years are simulated and how many, are the simulations used in section 3 the same ones used for section 4? Which will be the datasets used? etc. The text doesn't give a clear overview of the simulations conducted to validate the model. I would suggest having sections 3 and 4 as subsections of the same section and start it with a comprehensive and detailed description of the validation that will be presented in sections X.1 (surface measurements) and X.2 (satellite products). In addition, the authors should add a discussion on data quality and how it can affect the validation. Some of the data used do not coincide with the simulated year and this could explain some of the differences between model and observations.

Surface concentration: The authors compare the simulated surface concentration against two datasets; surface measurements of aerosol number size distribution in the city of Beijing and surface concentration at 21 marine sites. Although this is already very helpful to show some aspects of the model performance to reproduce certain aspects of the dust cycle, I wonder why the authors did not use the surface concentration measurements at the site of Barbados and Miami available for the year 2000? This year has already been simulated by the authors and is currently used in the study. The use of these sites would allow to assess the model performance to reproduce the transatlantic dust transport. Again, this data have been used and described in Huneeus et al. (2011) and are made available to be used. I strongly suggest the authors to include

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these data in the study. The authors should go deeper in the analysis of the model performance to simulate the surface concentrations. On one hand they only mention deposition as an explanation to the overestimation of the observations. Even though the explanation provided is coherent what about other process that could explain this feature such as transport, underestimation of the emission or vertical distribution. Specially considering that over ocean wet deposition is the dominant process (Prospero et al., 2010; Hand et al., 2004; Gao et al., 2003) and therefore total deposition is not only a function of the concentration at the surface but also the layers above. On the other hand the authors should say something on long range transport from mayor dust sources based on the model performance to simulate surface concentration. The station in Midway Island is impacted by Asian dust (Prospero et al., 2003; Su and Toon, 2011) and therefore differences between model and these observations could reveal aspects of the model performance. The same is valid with the measurements in Barbados. This station is affected by dust transported across the Atlantic from the Sahara.

Total dust deposition: The authors validate the total model deposition against measurements compiled in the DIRTMAP database. Again, why not use additional observation that is available for model validation. A three year dataset of wet and total deposition at nine station in Florida exist and has been used for model validation in Prospero et al. (2010) and Huneeus et al. (2011). In addition, Ginoux et al. (2001) presents a compilation of total deposition at different sites. Finally Mahowald et al. (2011) present a compilation of estimates of fraction of wet deposition at a number of sites. I strongly recommend the authors to include these datasets in their validation and if not justify why they decide not to use them. They would allow to explore the model performance in aspects not examined in the present version.

Aerosol optical depth: The authors use three satellite products in this study, one from MODIS, one from MISR and one combining MODIS and MISR. It is not clear why the authors actually use this last one. Furthermore, since the total AOD used in the work cannot differentiate between different aerosols why use it? how much of the differences

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are due to dust and how much due to other aerosols? Why not use AERONET data? not only provide they total AOD but also coarse mode AOD and Angström exponent which would allow to assess the model specifically with respect to dust. I would strongly suggest the authors to use this data instead.

Specific Comments:

Page 26478, line 23: remove “quite”.

Page 26479, lines 19-21: Why were the ranges given in Textor et al. (2006) and Huneeus et al (2011) not included. They give a larger range than what Zender et al (2004) gives. Cakmur et al. (2006) also gives an emission range that should be included.

Page 26480, lines 13 & 14: Authors should reformulate this statement. MODIS data exist that is valid over continents (total AOD), just not over desert dust areas as the authors mention, but over other surfaces it is. It is the fine mode AOD which is not recommended to be used over land.

Page 26486, lines 12 & 13: Textor et al. (2006) is a model intercomparison study and to my knowledge at no point the authors claim that there is a dominant process in dust removal. All the opposite, the authors clearly state in page 1792 that “For the “natural” species, there is no overall agreement among the AeroCom models on whether wet or dry deposition is the dominant removal pathway”. Authors should base statements such as this one on observation studies rather than on model ones. Results have been published that show that over ocean wet deposition is the dominant process. This issue is addressed in Huneeus et al. (2011).

Page 26487, lines 11 & 12: This is not completely true, what about the AIRS product at 10 μm? Doesn’t it allow in principle to observe only dust particles? Please complement.

Page 26489, lines 20 & 21: “The climate run is performed for five years. . .”, which years

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exactly? “The nudged run is . . .when observational data is available”. Not clear at this point which observational data the authors refer. See general comment.

Page 26490, line 17: Zender et al. (2004) do not estimate global dust emission but present the range of model emissions. However Huneeus et al. (2011) and Cakmur et al. (2006) estimate emission for certain deserts by constraining models with observation. Correct the statement. What about the emissions for Sahara and the Middle East? How are they compared to estimates given in Cakmur et al. (2006) and Huneeus et al. (2004)?

Page 26490, lines 25 & 26: “In this section,..” it is not clear to the reader which dataset the authors are talking, they should be presented before the analysis is done. See general comment.

Page 26491, line 13: Keep the same units, either nm or μm.

Page 26492, lines 4-7: Why do the authors attribute the underestimation of the submicron particles only to pollution, couldn’t it be also due to dust? Couldn’t it be that the model underestimates the emission of fine mode dust?

Page 26492, lines 18-21: The authors make a summary of the main results of this section but forget to mention the underestimation of the fine mode. It should also be mentioned, specially considering that the evidence presented does not prove that it is only due to pollution.

Page 26493, lines 5-13: Combine Figures 4 & 5 in one. Eventually even remove figure with distribution of surface concentration since the same information is already provided in the scatter plot and is easier understandable.

Page 26493, line 17: How much percent?

Page 26494, line 15: Remove potentially. An extensive literature exists on the transport of Saharan dust across the Atlantic to the Bermuda and Florida.

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Page 26495, line 12: I would recommend the authors to complement or even replace the distribution of deposition with a scatter plot. It is easier for the reader to see where the observations are over or under estimated. Data points in the scatter plot could vary in colour and shape according to the region in order to relate each point to the figure with the location of the observations.

Page 26496, line 22: To what product exactly does MOD08_M3 correspond? Is it the daily, 8 daily or monthly product? Please specify.

Page 26497, lines 16-18: “The model captures the main aerosol plumes over West Africa, Middle East and East Asia”. They capture the plumes but overestimate the magnitude over West Africa and the Middle East. Authors should be honest about this and include it in the text. I don't see in Figure 9 that the main deserts in Asia are well represented. The author should present additional evidence to state this.

Page 26497, line 23: It is not clear to me from Figure 9 that the model represents well the deserts in Chile, Peru and Australia.

Page 26498, lines 11-14: The authors compute the total and dust AOD for the year 2000 in order to compare it to other GCMs, but end up comparing it only with the AeroCom median. How are the values compared to other aerocom models? Any idea what explains the difference in dust AOD with respect to the AeroCom median?

Page 26499, line 26: Replace “rather” by something more quantitative. How much is rather?

Page 26500, line 1: The authors claim that the discrepancies between model and observations are mostly within the range of uncertainties of the observations. How big are the uncertainties of the observations? The authors at no point give these uncertainties. If they want to make this kind of statement the uncertainties of the observations need to be introduced in the analysis otherwise they should remove the statement.

Page 26500, lines 2-5: The authors present only the deposition as an explanation of the

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differences between model and observations. I believe the authors should elaborate further on this and see whether other processes could explain these differences. See general comment.

Page 26500, lines 12-16: This statement is not only incorrect but also inappropriate. The authors at no point present the results in a manner that allows a comparison with the ones presented in Huneeus et al. (2011). The authors in that paper present quantitative measures of the differences with respect to the observations that are not reproduced in this work, even though they could easily be calculated. If the authors would like to keep this statement they should not only present the results in an equivalent way to Huneeus et al. (2011) but also extend the validation to all datasets used in that study. Otherwise the statement is misleading and should be removed.

Page 26512, Figure 3: What happened with the months of October, November and December? It should be explained in the figure caption why these months have not been included. In addition, I suggest the figures be ordered chronologically.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26477, 2011.