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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

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Peng et al. describe a new size-resolved dust scheme, which they implemented as the PLA-Aerosol Module within the framework of the Canadian Atmospheric Global Climate Model (CanAM4-PAM). They evaluate their scheme by performing a climate run with prescribed SST, as well as a run where the model is nudged towards reanalysis data for specific short time intervals. The results of these runs are compared with surface concentration and deposition measurements, as well as with global AOD data from satellites.

Overall, this is an interesting and important paper on a dust model, which could eventually contribute to reducing the large uncertainty in aerosol radiative forcing due to dust.

However, there are a few issues in this paper which should be addressed before publication. The major issue is an inadequate discussion and usage of satellite products which could be used to evaluate the model's performance.

General comments

The authors state that "satellite observations have difficulty distinguishing between different aerosol species,...". This statement is too general and an oversimplification. Several satellite products exist which provide information on dust aerosol. Rather than completely discarding these products, the authors should apply some of these available datasets to their test cases and discuss potential limitations and the range of uncertainty of the observations. MODIS Deep Blue provides separate AOT for dust and fine mode/mixed aerosol, particle size information (via the Angstrom parameter), and single scattering albedo for dust. MODIS DB also remedies the limitation of MODIS retrieval over bright land areas such as deserts, which was mentioned as a concern by the authors. MISR provides (within some limitations, e.g. cloud cover issues) information on aerosol composition as well as height. The current operational MISR aerosol product (Version 22) reports the fraction of non-spherical particles retrieved over both land and water.

A recent relevant publication which discusses the application of MISR data to dust is: Olga V. Kalashnikova, Michael J. Garay, Irina N. Sokolik, David J. Diner, Ralph A. Kahn, John V. Martonchik, Jae N. Lee, Omar Torres, Weidong Yang, Alexander Marshak, Sero Kassabian and Mark Chodas, "Capabilities and limitations of MISR aerosol products in dust-laden regions", Proc. SPIE 8177, 81770O (2011); doi:10.1117/12.897773

Contrary to the authors' statement that "CALIPSO will provide layered data of aerosol properties" (p. 26501) this data is already available for some years. In addition to height-resolved information, CALIOP on CALIPSO can also measure the particulate depolarization ratio and use it to distinguish between spherical and non-spherical particles, see e.g. Yu, H., M. Chin, D. M. Winker, A. H. Omar, Z. Liu, C. Kittaka, and

T. Diehl (2010), Global view of aerosol vertical distributions from CALIPSO lidar measurements and GOCART simulations: Regional and seasonal variations, JOURNAL OF GEOPHYSICAL RESEARCH, 115, doi:10.1029/2009JD013364.

MPLNET also provides layer information for aerosols. These datasets could be used by the authors for a vertical analysis of their model results. But this aspect could also be subject of a follow-up publication.

Although it is correct that "there is no direct measurement of aerosol size distribution on a global scale" (p. 26501), there is no reason not to use size information which was derived via retrieval algorithms.

It is not clear to me why Aeronet has not been used in this paper. Aeronet (version 2) provides AOT, and via inversion techniques (under some assumptions) also size distribution, SSA, sphericity, and asymmetry factor; data on retrieval quality (for error estimation) is also provided, as well as the radiative forcing at TOA. These quantities could add a lot of insight to this study.

If the authors have specific reasons why none of these products can be used to evaluate the model results, they should discuss this in some detail. They should then also discuss why they chose the MODIS/MISR dataset from van Donkelaar et al..

Specific Comments:

p. 26478, l. 15: ADRF should also be listed for land+ocean, so it can be compared with other models.

p. 26480, l. 13: MODIS is not mainly restricted to the ocean, it is restricted to the ocean and land surfaces which are not highly reflective

p. 26482, l.1: It appears from here that the aerosols are internally mixed in the model, but on p. 26487 l. 21 the authors explain that dust and ss are externally mixed. Please clarify. Also, according to p. 26488 l. 19, an external mixture is assumed for calculating the optical properties. How are the internally mixed aerosols converted to externally

mixed ones?

p. 26483, l.2: A climatological dataset from 1850 to 2005 is used for the vegetation and bare ground fraction. Wouldn't it be better to use a climatology closer to the current time? For example, the Normalized Difference Vegetation Index (NDVI) can be used as a tool to describe the surface bareness because of its sensitivity to the vegetation cover; it is is available from MODIS or AVHRR.

p. 26484, l.8: The gusty wind is generally strongly resolution dependent. Was the model run in different resolutions (at least for a short time period) to get an estimate of this effect?

p. 26488, l.28-30: Does that mean that the change in fluxes is computed as "case_with_aerosol - case_with_zero_aerosol"?

p. 26489 l. 20: How were the emissions for BC, OC, SO2 processed for the climate run? Were they averaged over a certain time interval?

p. 26490: What threshold wind friction velocity are these two values (0.85 and 0.75) associated with?

p. 26492, l. 6: There are other reasons why submicron particles are underestimated (e.g. transport or deposition issues).

p. 26493, l. 11-12: The authors should also list the correlation coefficient, bias, error (RMS), and standard deviation.

p. 26493, l. 21: From where was the SST obtained? Which years were used to generate the climatology?

p. 26493, l. 25: The authors should also list the correlation coefficient, bias, error (RMS), and standard deviation.

p. 26497, l. 21: The plume over Indonesia might have been due to biomass burning activity.

p. 26497: Were the model results filtered in the same way as in the van Donkelaar paper?

p. 26500, l. 2: The uncertainty of observations are not provided in this paper. They are indeed an important aspect and should be included.

p. 26500, l. 15: It is not clear why the good agreement is mainly due to the size-resolved scheme of this model. Please elaborate why you think other differences between your model and other models are not as relevant.

p. 26512: The plots are out of order. Also, 200410 to 200412 are missing.

p. 26513: The numbers underneath the color bar look awkward. I suggest to use a vertical color bar.

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