Atmos. Chem. Phys. Discuss., 12, C4828–C4832, 2012
www.atmos-chem-phys-discuss.net/12/C4828/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC" *by* M. Astitha et al.

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Received and published: 19 July 2012

Review of "New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC by Astitha et al. for publication in Atmospheric Chemistry and Physics

The paper presents a parameterization of dust aerosol emissions based on soil texture data for inclusion in a global climate model. The model is run for the year 2000 with two instances of the dust source function, one in which soil properties are assumed globally uniform and the other invoking the new parameterization in which soil properties are from a gridded database. For each source function, two instance of the model are run, one where the model is run as a climate simulation and another in which the model is run with "nudged" meteorology in order to more realistically simulate the particular dynamics of the year 2000. Model surface mass concentrations, deposition, and aerosol optical thickness are compared to available observations. Overall it is found that the model reasonably simulates dust transport patterns and amounts. There is some difference between the two different versions of the dust source formulation, but larger differences between the climate and "nudged" simulations than between the source simulations.

The paper is well written, well organized, and relatively complete. I mostly have minor comments on the text. I do have a criticism in that the figures are really quite small and therefore hard to read, especially given the sort of detail one is expected to extract (the color of the dots on Figure 6, 7, 9, 12, and 13 are almost completely impossible for me to distinguish, as are the differences in the maps in Figure 4).

I'm going to recommend the paper be reconsidered after major revisions. I point out below what I think is flaw in the formulation of the soil dependent dust emissions. Either I don't understand something about what was done, or something was done that I think is flawed. If I prevail in convincing the authors of a flaw there it suggests either further simulations or else a discussion of the implications. Again, see below.

Furthermore, the significance of the work needs to be better discussed. I understand the appeal of moving toward more physically based models, but I'm not convinced that this work shows "the need to represent arid regionas individually and explicitly in global models." For one thing, a single year was simulated. Significance could be more quantitively assured by looking at more than one annual cycle. For another thing, the dust schemes are still tuned, and tuned differently from one another. So you have a tuning factor of 1e-4 for DU1 and 1e-3 for DU2. These factors are usually selected to give a desired model value: average emissions, or loading, or optical thickness. So how are these numbers chosen in this case, and would slightly adjusting one or the other improve the agreement between the two models (which isn't too bad as it is)?

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Minor comments Page 13241: In the model description, please say whether the aerosols are radiatively coupled to the model. I gather not. Additionally, somewhere in the presentation of the dust optical thickness please say something about the dust optical properties. Are other aerosols simulated and included in the computed optical thickness? It seems that other aerosol species are run, but I'm not entirely clear on this point (see page13261, line 1, which suggests poor AOT comparison to the station in the Maldives, which is likely influenced by pollution. Are you simulating this pollution?)

Page 13247, line 21: Reference to equation (10) does not belong here, as it is not unique to DU2 formulation. It appears to be used the same in both formulations.

Page 13248, line 22: You don't mean "soil size distribution" here. What you mean, and what Zender et al. are doing, is using the d'Almeida size distribution to represent the aerosol particle size distribution at the source.

Page 13249, line17: Please use "a" rather than alpha in equation (12) to be consistent with equation (10). Likewise in the following text.

Page 13250, line 11: I think there is a flaw in equation 13, but please convince me I'm wrong. This goes back to my previous comment about the d'Almeida size distribution. The horizontal flux (equation 9) should depend on the soil texture and soil particle size distribution, but it's less clear to me that the aerosol particle size distribution (equation 13) has that same dependency. Go back to Marticorena and Bergametti (1995) and Marticorena et al. (1997). You're following up on their formulation, but with global datasets of soil texture. They provide the vertical aerosol mass flux, but not the particle size distribution because there is not this clear relationship between soil particle size distribution and aerosol particle size distribution. It's up to the modeler to impose the particle size distribution of the emitted aerosol. Zender chose the d'Almeida "background dust" PSD, which is what you choose also for the DU1 simulation. Alfaro and Gomes (2001), for example, suggest that the initial particle size distribution is best represented as a function of the surface wind speeds, with different proportions of three

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size modes that depend on wind speed and not so much on soil characteristics. In the DU2 formulation you are imposing the soil particle size distribution on the emitted aerosol. But this seems wrong, since the mechanism for injection of aerosol is disaggregation of soil particles. So I presume this answers the question of why the tuning factor for DU2 is an order of magnitude greater than for DU1: your vertical mass flux must be apportioned over the four modes of the soil distribution, which may be quite large, depending on the soil type, and so most of the mass is simply not carried in the size bins you care about and need to bump up the overall emissions to get a reasonable load in the 0.2 - 20 um diameter range you care about. So, I presume the information in Figure 4 and Tables 5 - 6 pertains only to the emissions of dust in the 0.2 - 20 um diameter size range. Is that right? The some of the differences you see are not because of differences in where emissions occur (soil dependence) but because of differences in the emitted particle size distribution, reflected in different lifetimes of the DU2 versus DU1 cases, suggesting DU2 carries more mass at larger sizes.

Page 13255, line 2: I don't understand this conclusion. The year 2000 simulation with the nudged meteorology looks pretty good compared to the year 2000 observations. But that the free running model for year 2000 has a different amplitude (but same seasonality) as the climatological observations does not impugn the quality of the free running model. Is the free running model way outside the variability in the climatology of the observations? That the year 2000 observations shows this bi-modality and is lower by about 30% than the climatological peak in July suggests there is maybe significant interannual variability in the observations. The free running model is one possible realization of that variability. Running it for another year and comparing to the climatology you might reach a different conclusion.

Page 13257, line 18 (and Figure 8): You indicate in the text you are talking about the budged simulations. Please reference here and in the caption at DU1_ERA40 and DU2_ERA40.

Page 13262, line 9 (and Figure 14): Why are you showing the mass loading from

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MODIS? This is a by-product of the retrieval algorithm in MODIS, and drops out of the otpical models used to make the retrieval. It is unvalidated, so far as I know. Why not instead just compare to the optical thciness retrieved by MODIS? That at least has some validation behind it. Better yet, compare to the coarse mode optical thickness, which would tend toward removing biomass burning and Asian pollution hotspots.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13237, 2012.