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

From: Y. Shao (yshao@uni-koeln.de): "New parameterization of dust emissions in the global atmospheric chemistry-climate model EMAC, by M Astitha et al. The authors implemented two formulations of a dust emission scheme and compared the simulated dust emission and dust concentrations. Also compared are the model results with the AERONET data. Considerable efforts are made to evaluate the model results through inter-comparison and comparison with radiation measurements. Some of the problems

are listed below."

[Author reply]: We would like to thank Prof. Y. Shao for his interest in our work and the useful comments on the manuscript. His important work on dust processes is well known world-wide and he provides us the opportunity to clarify some issues that are important for the readers and the reviewers of the manuscript. We respond to the issues raised below and we will carefully implement his questions and suggestions in the revised version of the manuscript.

P13238, L14 – 15: "accurately simulated by both schemes". It is intriguing how this is possible? The particle size distribution of emitted dust, if we follow the dust scheme, must be dependent on the way the soil is specified. Then in L19, the authors claim deposition is important. If so, then there is no reason for the model to produce similar results with DU1 and DU2. If the model tests shows it does not matter much with source soil texture, then why is important again (L21): "need to represent . . . land characteristics"? On the other hand, Table 5 seems to suggest there are substantial differences.

[Author reply]: Both versions of the parameterization scheme produce similar amounts of dust in the N. African region (shown in the regional dust emissions in Table 6) on an annual basis. This occurs because the DU1 scheme produces more dust in Mauritania, Sahara and Bodele and less in Libya and Algeria compared to DU2 (Page13265, Lines13-18). This is a result of the differences in the soil size distribution of the two schemes; coarser particles in Mauritania, Sahara and Bodele from the Zobler classification used in DU2, leads to less emitted dust into the atmosphere. The outcome of the aforementioned regional differences (positive and negative) between DU1 and DU2 is the main reason for the similar dust outflow from the African coast over the Atlantic Ocean, given the fact that the deposition scheme is the same in all simulations. We do not state in the manuscript that the soil texture does not play an important role, rather the opposite. The amount of dust on an annual basis in N. Africa is similar between the two schemes, but sub-regionally, within N. Africa, the differences are important.

Furthermore, dust emitted from Asian and S. American deserts differs substantially between the 2 schemes, which lead us to conclude that each arid region must be represented individually and explicitly in global models according to their unique land characteristics and meteorological conditions.

P13240, L11 – 14: "We address (i) . . .". These are novel objectives, but I do not see how these are and can be achieved. May be the authors should state here already, what exactly are done and why is it possible to achieve these objectives. By the way, what is heterogeneity? It seems only the soil particle size is considered in this paper, what about the rest parameters, as far as heterogeneity is concerned?

[Author reply]: The objectives we describe on P13240, lines 11-14, are intended for the purpose of the modelling study as this work does not and cannot address the theoretical issues of the dust emission process which we believe is the main point raised by Prof. Shao. The first objective is that "we address the physical processes that lead to the injection of dust particles into the atmosphere". This is done by implementing a dust emission scheme that makes use of the air temperature, humidity, density and friction velocity from the direct coupling with the meteorological calculations and also calculating online the threshold friction velocity and dust fluxes without using pre-calculated tables. These elements are all part of the physical processes that regulate the emission of dust particles. The second objective is that "we address the role of the input parameters in representing the spatial heterogeneity of dust emissions", which is solely connected to the soil size distribution and texture. The spatial heterogeneity of dust emissions refers to the uniqueness of the emitted amount of dust particles depending on the region they originate from. This is affected by the soil texture and size distribution, the meteorological conditions, the terrain characteristics and the general synoptic and local circulations. We shall rephrase these sentences to emphasize the objectives of our work from the modelling perspective.

P13240, L14 – 16: Is this new? This is done in regional dust models, as well as for global models (e.g. Tanaka and Chiba, 2006), for years.

[Author reply]: The statement that Prof. Shao refers to is: "One advantage of using EMAC is the direct coupling to meteorological calculations at each time step (10min), which is expected to realistically represent the grid-scale temporal variability, e.g., compared to off-line calculations with a chemistry-transport model based on 3- or 6-hourly meteorological analyses". Of course dust models (with no ability to simulate chemical processes) are known to include the dust emission module directly coupled into the atmospheric model, whereas for atmospheric chemistry models this is not always the case. The modelling system we use is a general circulation - atmospheric chemistry model, which has the advantage of directly coupling the atmospheric physical and chemical processes. We refer to this advantage because in the modelling of dust particles we also make use of a simple sulphur chemistry mechanism to turn the dust particles from insoluble to soluble during transport. We do not refer to this model characteristic as a new idea but as an advantage in our research on modelling the dust emissions globally that also has the potential to include dust particles in other atmospheric chemical processes in the future. In follow-up studies we intend to use the full complexity of the chemical scheme to study how gas-particle partitioning processes affect the atmospheric lifetime of the dust particles.

P13246, L10 - 13: To me, the assumption for the two roughness lengths is unacceptable. It is well known, this formulation of drag partition performs poorly, and requires impossible parameters, such as separation distance between roughness elements. z0s is almost impossible to determine. Further, z0 depends on parameters such as vegetation cover and rock distribution etc. The whole point of the Marticorena and Bergametti (1995) and Marticorena et al (1997) papers is about z0 and z0s.

[Author reply]: We agree with Prof. Shao that the use of fixed values for the Aeolian and the smooth roughness lengths is not the most adequate solution for the drag partition correction. Nevertheless, it is a solution that has been published before for global models (Zender et al. 2003; Perez et al. 2011). We did not use the aerodynamic roughness length produced by the model since it would reflect processes on different scales compared to the Aeolian roughness needed for the emission of dust particles as discussed in Darmenova et al. (2009). Also, the use of a global model restricted our ability to use measured values of roughness lengths and cover the entire globe. We will pursue a more detailed representation of the two roughness lengths in our future work and we would be glad to follow any suggestions by Prof. Shao in this direction.

P13247: The title suggests there is a new dust parameterization. However, it is difficult to identify “new” aspects in terms of dust physics and the formulation of the dust emission scheme. It appears rather to be an implementation of the ingredients of some existing dust schemes.

[Author reply]: The title of the manuscript is explicitly mentioning the new parameterization implemented in the EMAC model (as opposed to the dust scheme that was in the model before) and not the formulation of a new theory and parameterization of dust emissions. We state this also in the text in Page13243, Line5 “The methodology followed in this work is based on previous dust emission schemes for regional (Perez et al., 2006; Spyrou et al., 2010; Laurent et al., 2008, 2010; Marticorena et al., 1997) and global modelling systems (Zender et al., 2003; Tegen et al.,2002).”

P13247: There is also a need to carefully check the physics before using them and to check the origin of the ideas. For example, why should c be 1? It is not 1 and it varies over a wide range. The origin of equation (9) is from where? Also, the origin of (8) is not Marticorena et al. (1997), but Kawamura (1964) and then White(1979). I tend to disagree the way papers are cited in the text. I hope the authors go to the original papers.

[Author reply]: The use of the dimensionless constant c in Equations 8 and 9 has been set to 1 after the suggestion of Darmenova et al. (2009). Even though the original value from White (1979) was 2.71, as also used in Marticorena and Bergametti (1995), the recent publication at JGR from Darmenova et al. suggested the change of c to 1 according to recent wind tunnel measurements. This value has recently been used

from other researchers like Kang et al. (2011, JGR). This is a publication where Prof. Shao is also a co-author and we hope he will agree that it is acceptable to follow the suggestion of previous published work. Again, we will be happy to reconsider the value of c in our future work depending on his suggestions. Equation 9 is found in Marticorena and Bergametti (1995) and Marticorena et al. (1997) for the calculation of the horizontal flux as a function of the particle diameter, beginning with Equation 8. In those publications there in no reference to other work. The missing reference to Kawamura (1964) and White (1979) is due to our negligence and will be corrected.

P13247: If the main difference between DU1 and DU2 is the difference between Eq (8) and Eq (9), then there is no wonder that the model outcomes of the two versions are similar. Even for a global model, there does not seem to be an urgent need to make this assumption, because the sand particle size is relative easy to estimate, by sieving for example. What is difficult to determine is the particle size distribution of fine soil particles and whether the fine soil particles break, i.e., the real problem lies in Equation (10), the coefficient a, if a universal a exists at all. If the authors really wish to emphasis the difference between (8) and (9), then they should show the differences in sand drift, before talking about dust emission.

[Author reply]: We thank Prof. Shao for pointing out this important issue, which agrees with our results. Our goal was to test the two formulations of the dust emission scheme in the global atmospheric chemistry model with the differences in the soil size distribution and study the outcome in the global dust cycle. One could anticipate a similarity between the dust fluxes based on the equations 8 and 9 but not to what specific extent that similarity would occur. The issue of the particle size distribution, as raised by Prof. Shao, is very intriguing and important and cannot be addressed by the use of a global model. We do not discuss the accuracy of the parameterization from Marticorena and Bergametti versus other very important and up-to-date dust emission schemes; our research is based on understanding and indicating the effects of soil size distribution in the global dust cycle by using the methodology described in the manuscript.

P13248, L17: D0=60 micron. Well, particles of this size saltate, but do not produce saltation bombardment (or sandblasting), because these particles are almost in the mode of modified saltation (they have little impact energy when they srike the surface). Therefore, the use of this size is inconsistent with the idea of saltation bombardment. Equation (10) is empirical and is derived by using a very old data set of Gillette (1977). There is no reason that this relationship should apply worldwide and there is no reason that this relationship applies to saltating particles with such D0.

[Author reply]: The solution of the empirical Eq.1 that calculates the threshold friction velocity indicates that the minimum threshold friction velocity occurs for particle sizes 60-70μm, in the range of sand-sized particles. Zender et al. (2003) have used Do=75μm and Spyrou et al. (2010) Do=60μm in an attempt to simplify the parameterization scheme and avoid the uncertainties of the soil size distribution assigned to different soil textures. This is the first time we hear that the saltation bombardment does not apply to sand-sized particles (even in the lower range of sand-sized particle diameters) and we wish Prof. Shao would point us to the relevant publications. Also, about the empirical Eq.10 that describes the sandblasting efficiency, we used it to be in accordance with the dust scheme of Marticorena and Bergametti (1995) and keeping in mind that this equation has been used in modeling the dust emission processes from various researchers up to now (i.e. Zender et al. 2003 JGR; Darmenova et al. 2009 JGR; Laurent et al. 2010 JGR; Perez et al. 2011 ACP; Spyrou et al. 2010 JGR; Pierre et al. 2012 JGR, among others).

P13249, (11): This equation is difficult to understand and it is difficult to see how this is related to (10). Again, where does this idea originate?

[Author reply]: Equation 11 originates from Schulz et al. (1998) as cited in the text, and was also used in other publications (Zender et al. 2003, Spyrou et al. 2010, etc). The main idea is to convert the dust mass emitted from the source modes (the particles that reside in the soil) to the transport modes (the particles that are airborne), which follow different size distributions. The standard error function shown in Eq.11 calculates the

fraction of mass from source mode i to each transport bin j. Eq.11 is not related to Eq.10. The mass fraction calculated with Eq.11 is used in the calculation of the dust vertical flux (Eqs. 12, 13).

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