

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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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.

P13238, L14 – 15: “accurately simulated by both schemes”. It is intriguing how this is
C3412

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.

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?

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.

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. z_0s is almost impossible to determine. Further, z_0 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 z_0 and z_0s .

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. 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. 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.

P13248, L17: $D_0=60$ 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 strike 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 D_0 .

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?

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