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# ***Interactive comment on “An improved dust emission model with insights into the global dust cycle’s climate sensitivity” by J. F. Kok et al.***

**Anonymous Referee #1**

Received and published: 30 May 2014

Review Kok et al.

The paper first presents in detail a new physically-based dust emission scheme that is fitted and compared with a compilation of vertical dust flux measurements from different campaigns. The new scheme is then implemented in the CESM climate model and evaluated with AERONET Sun-photometers. The authors conclude that 1) the new scheme better reproduces the vertical flux measurements than existing parameterizations, 2) its use in a climate model produces better agreement against AOD than simulations using a source erodibility function, and 3) the need for a source function is partially eliminated by the additional physics accounted by the new scheme. It is also suggested that models using source functions may have underestimated the climate sensitivity of the global dust cycle.

C2929

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## General comments

I acknowledge the authors' profound understanding of the physics of dust emission and their effort to provide an alternative physically-based dust emission scheme potentially usable in global models. The paper is excellently written and represents an original contribution that is suitable for ACP. I do not see the paper as just a model development and evaluation. The physical insights provided by the authors justify, in my opinion, its publication in ACP (instead of GMD for example). I would say that more than replacing the concept of source erodibility function, they are providing a method by which the source function is dynamic and dependent on the threshold friction velocity of the soil. This represents an advance and opens the door for future improvements of the dust cycle in climate models particularly when simulating other climates.

Nevertheless, I have several concerns about aspects of the paper in terms of format, content, and conclusions. I recommend publication if these concerns are properly addressed. I believe that the paper needs major revisions.

### Format:

I agree with the first referee that, although the paper is excellently written, it is very long and cumbersome (main text and the supplementary material) (This is why I rated the presentation quality as good although the paper is very well written). The supplementary material is 30 pages including a lot of text. The paper may be separated into 2 parts (I leave this decision to the editor). The first part could be the model description and the comparison with the dust flux measurements. Some of the tables, figures and text in the supplementary material could then fit into the main text of the first part. The second part could include the model simulations and their evaluation. This second part is, in my opinion, a bit weak and should be extended and more detailed given the strong conclusions reached by the authors. I believe that further insight on the spatial and temporal distribution of their dimensionless dust emission coefficient should be provided for example (this is what I referred to as dynamic source function).

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Content and Conclusions: I have some questions and concerns on several aspects of the paper.

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1) In my opinion some of the conclusions of the paper are overstated based on the results shown. It is true that the results show that CESM represents better the dust cycle compared to AOD measurements than previous schemes as implemented in CESM. However this cannot be generalized. The implementation of the other schemes used in the comparison is specific to CESM and the simplifications assumed are not necessarily fair or convenient for the other schemes. For example the scheme of Marticorena and Bergametti 1995 (MB95) is oversimplified as it is used here. It does not include the drag partition component, which is critical within the scheme's paradigm as shown by a number of studies. The lack of global databases cannot be a justification in this case as there are several global databases on the aerodynamic roughness length available. Also the smooth roughness length can be estimated from the FAO soil texture database that is used by the new scheme. The use in many models of a source function (interpreted as an erodibility function) partially accounted for the lack of realistic roughness length estimates in arid regions in the past. This is one of the reasons why I think that some of the discussions and part of the conclusion when comparing to other previous schemes are not well balanced. These aspects should be properly discussed and the conclusions balanced accordingly. I rated the scientific quality as fair but I believe that with a more balanced discussion the scientific quality would be excellent.

2) In relation to the previous point, I am confused about the use of friction velocity (eq. 2) and soil friction velocity (eq. 3). I understand the use of soil friction velocity in the new scheme but I do not understand how both the friction velocity and the soil friction velocity are calculated and/or used when comparing the different schemes (both when comparing the dust fluxes and within the climate model). If I understand correctly the new scheme uses the soil friction velocity but MB95 and GP88 use friction velocity. If I am correct this has several implications. First, the notation in equations 26 and 27 may be wrong as they refer to soil friction velocity when in reality it should be friction

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velocity. The same happens with the threshold friction velocity and the soil threshold friction velocity. In MB95 and GP88 it should be threshold friction velocity and not soil threshold friction velocity. Second, this may have important effects on your results. As I outlined previously, MB95 should include the drag partition scheme that accounts for the partition of the stress exerted on non-erodible roughness elements and on the bare soil. In your scheme this is implicit through the use of the soil friction velocity. However, this is treated differently in MB95. It is not surprising to observe the results of the model with MB95 in Figure 6a. The schemes features low emission fluxes in sandy regions where the clay fraction is low as the dust fluxes mostly scale with the clay fraction if the drag partition scheme is not used.

This raises some questions: - how did you calculate the soil friction velocity used by your scheme within the CESM model (how do you calculate eq 3 in the model)? - Did you use the soil friction velocity when applying MB95 and GP88? That is what I can assume if I follow your equations 26 and 27, unless it is just a typo. - What are the implications for the model simulations and for the comparison with the dust flux measurements? In particular, concerning the comparison with the dust flux measurements how did you treat MB95 (Figure S2 shows the vertical dust fluxes as a function of the soil friction velocity)?

In any case, whether there a typo and or problem or not, I ask the authors to clarify these aspects. I would like to see a discussion on the use of a drag partition scheme compared to the new formulation. In particular it would be very helpful that the authors explain the calculation of the soil friction velocity in the model. This would not only be helpful to strengthen the paper but also would help potential users of the scheme in the future.

## Detailed comments:

- Abstract: Please reformulate the abstract balancing the conclusions. I believe it is not completely fair to state that the new scheme better reproduces the measurements than

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existing parameterizations. 1) Not all existing parameterizations have been tested, 2) the new scheme was fitted with the same data that was used for the evaluation, and 3) the other parameterizations tested contained potentially harmful simplifications.

- Introduction and other parts of the paper: It is important to distinguish between the parameterization proposed by MB95 in their paper and the simplification of MB95 that is used in your model. The drag partition aspect should be clearly outlined.

Section 2.3: - Equation 19a should include the expression  $u^* > u^*t$  to be formally correct. Please specify again that  $u^*$  refers to soil friction velocity.

- Also note that your parameterization is very dependent on the clay fraction of the soil through  $f_{clay}$  and (mostly) through  $u^*t$  (which depends on  $f_{clay}$  in your model). Since the dimensionless dust emission coefficient  $C_d$  decreases with increasing  $u^*st$ , the areas with less clay fraction will show strong dust fluxes since the threshold depends on the humidity of the soil (which depends on the clay fraction through the Fecan formulation). So the smaller the clay fraction, the larger the dust flux (This is clearly seen in Figure 6d where the hot spots in North Africa appear to be in regions with low clay fraction). I would like to see an average map of your “dynamic” soil erodibility and I believe that in North Africa it will correspond to Sand and Dunes (Sand and dunes in the FAO contain low amounts of clay and large amounts of sand). Please give some insight on this issue. In my opinion your model is giving good results. Satellite data shows that hot spots in Africa coincide with sandy regions. However I expected that to be the case because of enhanced saltation and low roughness length. But that it is just a belief. Could you please develop on this?. It would be helpful that you include this discussion.

Section 3.5 -Equations 26 and 27: Is there a typo? I believe MB95 refers to friction velocity, not to soil friction velocity. Also please add  $U^* > U^*t$ . What are the implications? How this affects the comparison with dust flux measurements?

-Equation 27: I note that the formulation is wrong. Please read

<http://dust.ess.uci.edu/facts/aer/aer.pdf> from Charlie Zender (page 33-34 and equations 3.67 and 3.93). It doesn't seem to have very important implications but I still believe that you should use the correct formulation. We should stop propagating errors in our papers. What are the implications for your results?

- The same datasets used to fit key parameters of the new scheme are used to evaluate the model in comparison with other dust emission schemes. Why in figure 5 you only show the comparison with the measurements used for the fitting and not the other measurements? That would probably be the fairest comparison since any of the schemes would have been fitted with the data.

Section 4.1.1 This section needs more details: - What is the soil threshold friction velocity used (I refer to the dry)? - How the model calculates the soil friction velocity and the friction velocity? - Does MB95 use the soil friction velocity too? How is this justified? - Why not using a drag partition scheme?

Page 6393, line 17: How does Ginoux et al 2001 account for the effects of vegetation? It is a fixed mask based on topography without seasonal variation. Why not using a seasonally dependent *fbare*?

Figure 5 caption: Include the names in the caption.

Supplementary material: Please include the acronyms when presenting the database in the beginning to better link with the main text. There are some inconsistencies between the acronyms used in the supplementary material and the main text (for example ZP vs ZV)

Figure S2: What is the colour for each scheme? Please add it to the caption. Also, please include a supplementary figure without tuning. Why not all the comparisons appear in Figure 5? Please include them.

As a final comment: Please check all your equations again. I was not able to capture any other typo in your equations but it usually happens and unfortunately errors are

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propagated in the dust modeling community and models give good results for the wrong reasons!.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 6361, 2014.

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