These 2 papers can now GO into ACP once the very minors corrections below are inserted.

The 2 reviewers (including me) accept your 2 papers with very minor corrections.
Please address the following points

1/ Check in the first review that there was not an issue with one of the equations. I recall that one of the reviewer had asked you to change something about the friction velocity (if I recall well) and I did not see it in your changes. Do you recall such things?

You indeed recall this correctly. We did address this concern at length in our revisions, which is described on p. 1 and p. 4 of our reviewer response letter. The text on lines 544-560 of paper 1 discusses the issue the reviewer raised in some detail.

2/ Adress this comment:

2nd round of review of

An improved dust emission model. Part 1: Model description and 1 comparison against measurements
An improved dust emission model. Part 2: Evaluation in the Community Earth System Model, with implications for the use of dust source functions

By Kok et al.

The authors have split up the paper into two parts, addressed the comments of the 2 reviewers satisfactorily, and added an extended evaluation in Part 2. I therefore recommend the 2 papers to be published in ACP.

I only have 1 additional comment that refers to the distinction between the friction velocity and the soil friction velocity that I do not fully grasp or seems inconsistent:

In Part 1 the authors state L145-147: “Equation (5) thus accounts for the effect of wind momentum absorption by non-erodible roughness elements on aeolian transport through the wind stress on the bare soil, as captured by the soil friction velocity u*”.

The authors use fbare, which is the bare soil fraction. However bare soils can be covered by non-erodible roughness elements not related to vegetation, such as clasts, and small-scale topography. For Eq (5) to be correct fbare should be the fraction of bare "erodible" soil. Am I correct? If yes, what are the implications for your model? How would this be calculated?

This is a good point, and we have clarified the text to make it clear that f_{bare} refers to the erodible bare soil that does not have any roughness elements on it, be it vegetation, rocks, or clasts. Our definition is identical to that used by the pioneering paper of Raupach (1992), which we now specify for clarity. As such, f_{bare} is a standard variable in the field that has been used in numerous previous studies.

On the other side, since meteorological and climate models provide the friction velocity and not the soil

friction velocity, how should modelers calculate both the soil threshold friction velocity from the soil friction velocity when using your model if the information on roughness length is available?
The soil (threshold) friction velocity u_* (u_{*t}) can be obtained from the (threshold) friction velocity u_*' (u_{*t}') through the use of a drag partitioning scheme. It's indeed a good idea to mention this explicitly, which we now do in the text immediately following Eq. 5.

3/ A few minor edits from me:

Paper 1: reformat lines 538-539
which the bare soil experiences erosion. Eq.
(25) simplifies Eq. (34) in Marticorena and Bergametti (1995) by using a single value of u*t' for

to

which the bare soil experiences erosion. Eq. (25) simplifies Eq. (34) in Marticorena and Bergametti (1995) by using a single value of u*t' for …

We've corrected this a few times before, but there seems to be something in MS Word that keeps reverting it back. Hopefully it will hold this time.

Paper 2:

Abstract.
' We hypothesize that this finding explains why many dust cycle simulations are improved by using an empirical dust source function that shifts emissions towards the world's most erodible regions. '
How is your approach not a source function based approach. You might confuse the reader by stating that previous formulations where using a function and you are not.
What do you exactly mean by that?
Please clarify/and change the wording.

This is indeed a bit confusing. Our scheme actually does not use a source function, but it does contain a physically-based dust emission coefficient Cd (Eq. 18b in paper 1) that scales the dust flux. For the sake of comparing against formulations that use an empirical source function, we interpret Cd as a dynamic and physically-based source function in Figure 3 of paper 2. But it is not an empirical source function as referred to in the abstract and elsewhere. We've adjusted the wording in the abstract and elsewhere to clarify this.

Please change 'Nicholas Huneeus' to 'Nicolas Huneeus'

Done. Thank you for catching that!

Best,

Yves Balkanski