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Atmospheric Chemistry and Physics
Discussions

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 #3

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The above referenced manuscript is well written, generally well prepared, and I can find no corrections needed with it regarding language or grammar or typographical errors. However, it is overly long, and almost all parts of it can be compressed and shortened by eliminating extraneous information and writing more concisely. I still give Reviewer Question 3, "Presentation Quality", an "Excellent" rating.

The manuscript as submitted really comprises two parts: (A) which is comprised of parts 1 through 3 of the manuscript, regarding the derivation, justification, and sensitivity testing of a new dust source/emission parametrization scheme for modelling, with assessment of the new scheme's performance using a quality-controlled compilation of dust flux measurements: and (B) comprising section 4-xx of the manuscript, the usage of this new dust emission scheme in a climate model (CESM) with comparison

and assessment against AERONET observations. It is really as if it is two separate ideas/concepts/topics merged together into one. Part (A)- including sections 1 through 3 of the submitted manuscript- is not appropriate for ACP, appears to me to be outside of the scope of the journal, and should be reformatted and separately submitted to the sister journal "Geoscientific Model Development" (GMD) where it belongs- there is really no need for the journal GMD if material like this is submitted to ACP instead! The second section, centred around part 4 of the submitted manuscript (the usage of the new dust emission scheme in the CESM model and assessment against AERONET observations) is appropriate for ACP and should be submitted, perhaps simultaneously, to ACP as a separate manuscript. By doing so, it would also eliminate the problem that the full submitted single manuscript is overly long. Thus, for ACP Reviewer's Question 1, Scientific Significance, "Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?," I am forced to give it a Poor rating because much of is not "within the scope of the journal."

With regards to Question 2, "Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?" in general, Section 4 of the manuscript, comprised of the usage of the new dust emission scheme in the CESM model and comparison to AERONET observations, is of Excellent quality. However, I noted some lack of balanced consideration in the first part, the derivation and testing of the dust emission scheme, so I must give it a "Good" rating overall. I will discuss these concerns below. Firstly, there are regions, in fact some of the Earth's dust hotspots, where a significant amount of mineral aerosol is produced from sources that are nevertheless classified by geomorphologists as "supply limited" (see lines 232-233), thus the statement (line 233) that they are "probably less important in the global dust budget" represents a too-quick dismissal of these circumstances that must be more explicitly justified if it is included, and additional discussion added of how the parametrization might be changed for/applied to supply-limited environments.

While it is true (Lines 193-195) that “the threshold for fragmentation of soil dust aggregates might be the most relevant threshold for dust emission under many conditions. . .” and clearly the research of Kok and collaborators in recent years applying brittle fragmentation theory to understanding dust production is inspired, elegant and probably the greatest recent advance in wind erosion science, I do believe it is a bit overstated herein as the implicit sole significant mechanism for dust production. Again, at least regionally on Earth there are places where significant dust production comes from other mechanisms, those described here in Section 2.4 including “dust emission from crusted soils. . . and from sand particles with clay coatings.” If, as the authors state, “the parameterization’s functional form is also valid for dust emission controlled by other thresholds,” it should have been demonstrated against data set(s) for such circumstance(s), and the apparent (to me) lack of such testing raises concern. In lines 404-412 the authors state “. . .it does not account for dust emission due to saltator impacts that do not produce fragmentation but that nonetheless produce dust by ‘damaging’ the dust aggregate.... It also does not account for the lowering of an aggregate’s fragmentation threshold through the rupturing of cohesive bonds by impacting saltators. These effects might dominate for very erosion-resistant soils, such as crusted soils.... increases in ψ might not produce corresponding increases in u*st for some soils. An example of such a soil is a sandy soil or which dust emissions occurs primarily from the removal of dust coatings on sand grains, and such soils might thus be poorly captured by the present theory. . .”: the parameterization must be tested against such datasets, but if I understand section 3 of the manuscript correctly, it isn’t- and section 3 thus looks a little bit like cherry-picking. Data sets should have been included in section 3 explicitly representing dust production from crusted soils and dust production demonstrated to be from damaging of aggregates and/or removal of coatings on sand grains! If such data sets are simply not available, additional explanation and justification is needed, and the discussion of the limitations of the parameterization should be increased.

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