Geoscientific Model Development Discussions

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Comment on gmd-2021-423

Anonymous Referee #1

Referee comment on "Evaluating dust emission model performance using dichotomous satellite observations of dust emission" by Mark Hennen et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-423-RC1, 2022

In this paper the authors present an approach to verifying dust emission model performance by considering how the model(s) compares to satellite observations of dust emission point sources (DPS). For this purpose they consider dichotomous (presence or absence) comparisons of near-global DPS observations (from MODIS and SEVIRI, for North America, Northern and Southern Africa, the Middle East and Central Asia, and Australia) and simulated emissions from their albedo-based dust emission model (AEM). The model appears to overestimate the number of dust events by an order of magnitude, while the DPS database indicates that dust emission events are actually quite rare in space and time, considering the vast size of some of the deserts. The authors use this dichotomous information to understand the conditions under which the model performance is influenced by the representation of the wind fields, the assumption of infinite sediment supply, and the threshold wind friction velocity u_{*ts}. There is value to this approach to validating dust emission models using observations that represent (as closely as possible) the dust emission events that actually occurred.

I am curious about this repeated claim in the manuscript that "The aim here is to demonstrate an alternative to comparing dust emission models to atmospheric dust" (line 79). As I see it, the satellite dust emission point sources are themselves interpretations of observations of atmospheric dust, back-tracking atmospheric dust in the satellite imagery for the purpose of identifying these emission points. Hence it seems to me that the authors are themselves still comparing the dust emission models to atmospheric dust. I suppose there is a distinction here between comparisons with the retrievals of atmospheric dust *transport* (implicitly or explicitly being criticised), as described by the dust AOD, and with the derivations of dust *emission* from atmospheric observations taken shortly afterwards (apparently a more robust method). Now I actually agree that it might be more robust to try to pinpoint the dust emission sources and to compare those with the dust emission model, but I think that it would very much be worthy of clarification and further discussion on the distinction between the two types of satellite observations of atmospheric dust which are being considered here. I think it would also be worthwhile to clarify the final sentence of the abstract ("… dust emission models should not be evaluated against atmospheric dust") to indicate that this relates to the dust AOD, assuming that this is what you mean.

This points to a general criticism that I have, that while this does seem to me to be a worthwhile paper, describing a useful method of comparing models with dust emission sources identified by satellite sources, I do feel that it is let down at times by over-generalised statements such as the one described in the above paragraph. Another such statement is "These results demonstrate that there is no reasonable basis to calibrate model performance through an adjustment to a fixed global u_{*ts}" (lines 482-484). If this is an implicit criticism of some model dust emission schemes, is this not something of a straw man argument? I would have thought that it has been quite widely accepted for many years that u_{*ts} is dependent on the soil size distributions and roughness lengths, which vary worldwide. I suppose however that there is some value to confirming that this should not be done, as has been done here.

Specific comments

p.5, lines 156-157: a minor point, but as written this sentence implies to me that the red beam is associated with the 8.7 μm channel, while the green beam is associated with the 12.0 μm channel. It is of course the other way round.

p.7, Eq.6: presumably in these equations it is $(1-A_s)$ multiplied by $(1-A_f)$. As written, this equation implies to me that no snow = no dust!

Figure 3: it might make sense here to fit the y-axis to 0-1 (as has been done for the x-axis, 0-0.6), it would make it slightly more intuitive to read.

p.13, line 392: "Taklamakan (Kazakhstan)". Presumably you mean China.