**Atmospheric
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Discussions

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Interactive comment on "Predicting the mineral composition of dust aerosols – Part 1: Representing key processes" *by* J. P. Perlwitz et al.

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This article provides a detailed treatment of the partitioning of minerals into dust aerosols at emission, which is critical for capturing the many mineralogy-dependent impacts of dust, for instance on radiation, biogeochemistry, and clouds. The methodology developed in this work is described in detail, is (mostly) well justified (where possible; sometimes experimental constraints are not available), and assumptions and weaknesses of the approach are discussed in detail.

The methodology draws partly from the recent previous study of Scanza et al. (2015). In essence, it partitions minerals in the soil into suspended dust aerosols using brittle fragmentation theory for D<20 um, extended to D=50 um by the detailed measure-

ments of Kandler et al. (2009). This empirical extension is necessary because the soil mineralogy in Claquin et al. (1999) and Nickovic et al. (2012) is described in terms of clay (D<2 um) and silt (2<D<50 um). Combined with several other improvements, for instance in the treatments of iron oxide and quartz, the methodology in this study is advanced over that in Scanza et al. (2015). As such, this article makes an important step in progressing dust cycle models, and therefore makes a substantial contribution to the field.

I anticipate that I can recommend the article for publication, after the following comments have been addressed:

- The authors' description of the brittle fragmentation theory (Kok, 2011) is not quite correct. Contrary to what is stated on p.3503, this theory does not reconstruct the aggregated soil size distribution. Brittle fragmentation theory does not need (or assume) a particular undisturbed soil size distribution, and in fact hypothesizes that the emitted dust size distribution is independent of the aggregated soil size distribution. This confusion is repeated at several places in the article, and I think in the companion article as well, and it should be corrected throughout.

- If I understand the methodology correctly, the authors are assuming that, except for iron oxides, each aerosol is composed of an individual mineral, even when it is an aggregate. That is, they scale the production of aggregated silt-sized aerosols of a given mineral by the prevalence of clay-sized particles of that same mineral. I would think that, in reality, the probability that a clay-sized particle ends up in a silt-sized aggregate depends on (among many other factors) on the fraction of other small (clay-sized?) particles in the soil, not just of the same mineralogy. Why did the authors make this assumption, and how do they expect it to affect their results? Some justification needs to be provided. Does this simplification explain some of the disagreements with measurements?

I also have a few minor comments:

- Line 24-25 on p. 3500: The cohesive forces actually increase with particle size (e.g., Shao and Lu, 2000). What the authors probably mean to say here is that the cohesive forces per unit weight (or per unit surface area, which is more relevant for particle lifting) decreases with particle size.

- Line 17 on p. 3503. Sow et al. (2009) actually argue that the size distribution depends significantly on wind speed. In addition to Gillette et al. (1974), better references here would be Shao et al. (2011) and Kok (ACP, 2011). Please correct that here and in the companion article (if appropriate).

- Line 25 on p. 3503. Reconstructing the aggregated soil size distribution from the fully dispersed one is very difficult and I'm not aware of any theoretical models for this. I looked at the cited Shao (2001), but could find no reference to such a model. If this reference does present the (otherwise reasonable) assumption that the amount of aggregation scales with the clay fraction, can you provide a page number?

- I think "d" is a confusing variable name for the mass fraction, as many readers will inherently associate this with particle diameter. I suggest using a different variable name.

- Since there are no strong constraints on the global dust emission rate, I think it makes little sense to force the total emission rate equal to some somewhat arbitrary figure (2224 Tg/year in this case). For future studies, I would suggest setting the dust emission proportionality constant either by forcing the global dust AOD to some constant value (since we do have constraints on that), or by calibrating to some observational data set such as AERONET.

- I'm a bit confused by the term "accreted". Is this the same as "aggregated"? Can you give exact definitions of both terms?

- Why does this article include a comparison to data at Tinfou, Morocco when part 2 contains an extensive evaluations against measurements? I think this needs some

justification at the beginning of section 4.5.

- Figure 15: keeping the color scheme consistent for panels e-h would make the figure more readable.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3493, 2015.

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