

Interactive comment on “Study of Arabian Red Sea coastal soils as potential mineral dust sources” by P. Jish Prakash et al.

Anonymous Referee #1

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The study presents results from measurements of the mineral composition and other properties of soil, based on 13 samples at four locations in the Saudi Arabian coastal plane adjacent to the Red Sea. The region has been understudied so far, although it is an important source of wind blown dust with at least regional impact on human health, climate, and ecosystems. There is a great need for measurements of this kind, not just in the region studied here, but generally, to better understand the impact of dust aerosols as well as to have more data available, which can be used to evaluate and constrain dust aerosols in modeling studies. Thus, I very welcome this study with the new data. The manuscript is generally well written and well structured. Having said this, I see the potential for some improvement in the manuscript, which can be achieved by doing a minor revision. The study should be published after the recommendations have been taken into account.

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The authors apply a variety of measurements techniques for studying the mineral properties of the collected soil samples. This is a good approach, since it allows to study the dust mineral properties from different viewpoints. It also reveals, though, that results from the different types of measurements can vary, allowing for ambiguity in the interpretation. This is most evident in the current study where the mineral composition is investigated for the same size range, i.e., $< 38 \mu\text{m}$ particle diameter. For instance, the results from the X-ray diffraction (XRD) analysis give a quartz fraction between about 20 and 40 % and a fraction of all the phyllosilicates of not more than 10 %. In contrast, the single particle analysis, using computer controlled scanning electron microscopy, gives a quartz fraction of only up to about 10 %, whereas the phyllosilicates have the largest fraction compared to the other minerals, partially more than 50 %. Which ones of the results from the two different measurement techniques are more reliable? The authors only report these contradicting results next to each other, but a discussion of the significant differences and how to interpret them is lacking. For instance, the possibility of the presence of phyllosilicates in the form amorphous material with poor crystallization is a known source for bias, when XRD analysis is used (Leinen *et al.*, 1994; Formenti *et al.*, 2008; Kandler *et al.*, 2009). Could using this method have caused an overestimation of the quartz fraction? Knowing the answers to such questions would be necessary for properly using the data to constrain or evaluate simulations with dust models.

I recommend following modifications for improving the manuscript:

1. **Section 3, “Sampling and analysis”**: For each of the described measurement techniques applied in the study add information about known sources of bias.
2. **Sections 4.3 – 4.5, Figures 3 – 6**: Explicitly state both in the text and in the figures (at the axes or in the captions) the percentages of what variables are shown. Are these the percentages of mass, volume, or number of particles? I

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suppose it is the mass fraction in the case of the XRD analysis. It is not clear to me in the cases of the other methods.

3. **Section 5, “Discussion and Conclusions”**: Add a discussion of differences in the results from the different measurement techniques and how these differences should be interpreted. How should the data be used, when they are applied in modeling studies?
4. **Section 4.1, Page 7, line 32**: Regarding the statement about the satellite images, I suppose this refers to the two references (Jiang et al. and Kalenderski et al.) that are mentioned elsewhere in the manuscript. Please explicitly reference the two papers once more at the end of the sentence.

References

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