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Interactive comment on "Quantifying local-scale dust emission from the Arabian Red Sea coastal plain" *by* Anatolii Anisimov et al.

Anonymous Referee #3

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The authors apply the high-resolution Community Land Model versión-4 (CLM4) together with the Dust Entrainment and Deposition (DEAD) to estimate the dust emissions from the Arabian Red sea coastal plain. The emissions are estimated for a 3-year period (2009-2011) using observed high-resolution satellite land cover and vegetation dataset. The main goal of this study is to generate "new, high-resolution, multi year estimates of spatial and temporal variability of dust emissions". In addition, the authors estimate the mineralogy of the emitted dust by assuming that the composition of the emitted dust is the same as the composition of the soil where it was emitted. The meteorological fields necessary to drive CLM4 are generated with the WRF model at a 10 km x 10 km spatial resolution and the sensitivity of the estimated emitted dust to spatial resolution of the surface characteristics is examined. Results are compared against visibility from station data. The magnitude of the dust emissions is indeed

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Discussion paper

vey uncertain at present (as mentioned by the authors) and all efforts to reduce these uncertainties are very much welcome. However, I believe that important clarifications and/or modifications need to be done before this work can be published in ACP.

General comments

The main result of the work is not actually quantifying the emissions as suggested by the title and in the manuscript but distributing them in space through the use of the CLM4 model. Authors should be more consistent throughout their paper between their claims and what is actually done.

The total emitted amount is scaled in order to fit the total MERRAero emissions but the scaling factor is never provided. Models do tune their emissions to fit AOD but by doing so they implicitly take into considerations the full aerosol cycle (i.e emission, transport and deposition) and are therefor consistent. However, by simply scaling the emissions to a given model the potential usefulness of the estimate is lost since it is not an independent estimate. It is not clear if it will actually improve performance for other models. How model dependant is this estimate? Even more, how large is the model uncertainty in the emissions for this region? How much dust is emitted by other models in this region? Furthermore, what is the size distribution of the emitted dust in the MERRAero model and how does it compare to the one estimated in this study? Although only the total emission is analysed the size distribution of the emitted dust is key to determine the impact of these emissions in terms of transport and deposition. The authors should provide a discussion addressing these issues.

The authors use visibility data as a mean to validate the estimated flux and draw conclusions on the source of the dust causing this reduced visibility. Visibility is a subjective local measurement reflecting the extinction of light in a given place, but it does not provide any information on the magnitude of the source causing the reduced visibility. Therefore it cannot be concluded on the magnitude of the emission based solely on these observations whether the source is local or not, other variables such as wind direction and magnitude need to be included for this analysis or a model needs to be applied.

The authors use the Spearman correlation as a statistic to validate the emission intensity. Besides the fact mentioned above that visibility is not appropriate parameter to validate emission intensity, the correlation reflect similarity in variability (spatial and/or temporal) but does not provide any information on the difference or “distance” between the observed variable and the estimated one. The authors should include additional analysis to actually validate the emission intensity.

Specific comments

Page 1, line 26, (Abstract): Remove “The total dust emission from the coastal plain appears to be 7.5 Mt per year”. This is not a result of the study but a constrain taken form a model and therefore should not be presented as result.

Page 2, line 31: “Regional uncertainties are probably even higher”, on what evidence is this statement based? Authors should provide a reference for this.

Page 3, line 25: “Our principal objective was to obtain new. . .in order to evaluate its impact on the Red Sea”. This objective should be reformulated and made consistent with the actual work done in this study. The emissions are first of all not estimated since they are scaled and for the same reason they can’t be new. Furthermore, the impact of the dust deposition on the Red Sea is not evaluated. The work as presented does not have the tools to address this issue. I would therefore strongly recommend removing this last part or reformulating it in order to make it consistent with the work that is presented in the manuscript.

Page 4, line 4: Replace “availably” with “availability”.

Page 4, line 10: “. . .are close of those of the parent soil.” Later in the text it is said that they are the same, what is it? The same or close? Please be consistent.

Page 5, lines 3-7: How was the setup or configuration of the WRF model defined?

Please specify.

Page 6, line 10: The variable “S” should be presented as source function at this point and not on line 14 as it is at present.

Page 7, line 2-3: Please provide a reference for the assumption that the intensity of the dust source is proportional to the frequency of occurrence of atmospheric dust. On what is this based?

Page 7, lines 6-7: It is still unclear how the threshold of 1.12 was chosen. Please elaborate.

Page 9, line 3: Why these two thresholds? Please explain why these two thresholds were used. Page 10, lines 5-8: Please provide a reference for what is said in these lines.

Page 10, line 15: According to whom is it not captured?

Page 12, lines 30-31: “Yu et al. (2013) offered several explanations for this”. It is not clear to what does it refer. One would expect it refers to the previous statement, but then on the next sentence satellite data are mentioned. Please reformulate.

Page 13, lines 5-9: I do not agree with what the authors claim in these lines. Whether the data used in this study nor the analysis conducted allow to conclude on whether the dust is emitted locally or transported from elsewhere. High correlations only indicate similar variability but are not an indication of distance between observations and model. One could have high correlations but also have dust coming from elsewhere. The explanation may appear reasonable, but it is not supported (nor refuted) by evidence presented in the manuscript. I suggest either removing completely these lines or reformulating it presenting evidence to support this claim.

Page 13, line 25: Is this model skill the correlation coefficient? Or does it refer to another statistic? Please clarify.

Page 14, line 7: “provide quite realistic results”, please reformulate. How much is “quite”? Please explain better why only the FineALL case is only consider in the remaining analysis.

Page 15, line 30: Replace or eliminate “reasonably”. How much is “reasonably”?

Page 15, line 31: Although SM1 and SM2 can be identified in MERRAero, the authors should acknowledge the differences between both representations (this work and MERRAero). For instance MERRAero locates a dust source further to the north than suggested by this study.

Page 17, line 1: I do not fully agree on the statement made on the first sentence of the paragraph. Although hotspots present variability consistent with the seasonal cycle, not all features can be explained by the hot spots (hotspots show very little variability from March to August in contrast to emissions from the entire region which shows strong seasonality). The seasonal cycle of sources other than hotspots should also be included in the figure to clarify the real weight of hotspots in modulating the emissions in the area of interest.

Page 17, lines 29-30: “All quantities…”, this is actually not entirely true since figure 8b presents variability not consistent with the solar peak and this is actually described later on. Please make the analysis consistent.

Page 18, lines 15-18: Why is so little said about the diurnal cycle of the dust maximum emission? Or why is it included? Authors should spend at least the same effort in analysing it as on the other variables, otherwise I would suggest removing it. Actually, how does it contribute to the general goal of this study?

Page 19, line 1: I would suggest include “estimated” or “calculated” before “emitted mineral fraction”.

Page 19, lines 17-20: This entire paragraph should be removed from this section (it is not a conclusion of this work) and placed after the last paragraph of section 2.1.

Page 19, lines 24-26: “The results confirmed. . ..” This conclusion cannot be made based on the evidence presented in this work. See comment made before.

Page 19, lines 27-28: This is true for the case when source function is used, while when the source function is not used this is not the case as stated in lines 25-28 of page 13. Please reformulate in order to make it consistent.

Page 20, line 23: Shouldn’t it be early afternoon when referring to 12:00-14:00 UTC?

Page 20, lines 28-31: First of all the 7.5 Mt/a are not estimated but imposed. This should be corrected. Then, the fact that emissions and deposition have comparable magnitude does not allow to conclude that it is an essential source of nutrients for the Red Sea, specially if one considers that the total amount was imposed from the beginning. Although one would expect that some of the emitted dust in the coastal plain should be deposited in the Red Sea, how much of it needs to be determined by another study. I would suggest removing this sentence.

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