Anonymous Referee #3:

General comments:

1. *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.*

In this study we quantified the spatial distribution of dust emissions in the Red Sea coastal plain. The total emission estimate is obtained from the MERRAero reanalysis and we specifically emphasized this in the text. This figure is calculated by us and was previously not known. Therefore we believe it is legitimate to use the term quantification.

2. *The total emitted amount is scaled in order to fit the total MERRAero emissions but the scaling factor is never provided.*

The scaling coefficient is provided in Table 4. We have stressed the reference in the text to make it more noticeable.

3. *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.*

The reviewer is, probably, familiar with the fact that the closure of the dust mass budget requires "… to quantify precisely the amount of emitted dust, the atmospheric dust load, and the deposited mass (dry and wet). However, this budget is sufficiently constrained if, at least, two of these three terms are quantified … " (Bergametti and Forêt, 2014). Therefore, the models that tune their emissions to fit AOD (i.e. dust burden) also incorporate uncertainty and cannot provide an "independent" emission estimate. So we question the remark that this approach is more consistent if the study is aimed at assessing dust emission mass. The scaling based on AOD observations alone could still be biased due to other reasons, i.e. errors in deposition velocity or size distribution representation.

In our modeling framework, we calculate only dust emissions, scale them using the most recent and reliable reanalysis dataset (see the answer to the next remark) and verify their spatial and temporal variability using station visibility observation. To our best knowledge, this is the most consistent and straightforward approach possible at present.

4. *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?*

We are not sure if we understand the question completely. In our study, we did not compare different models, but different land cover dataset. Our results show that, certainly, using finer land cover datasets will improve spatial variability of the dust emissions and will be beneficial for the models.

The uncertainties mentioned by the reviewer are a state of the art problem. To our best knowledge, only a few estimates of dust emission made so far for the Arabian Peninsula, and most of them are done using coarse-resolution models. No specific work has been done for the Red Sea coastal plain, so our work is a pioneering attempt of this type.

The MERRAero meteorology-aerosol reanalysis is the most appropriate data source to tune the regional dust emissions. E.g., Ridley et al., (2016) reported better seasonality of dust AOD in MERRAero compared to other datasets and pointed to potentially better dust emission due to finer spatial resolution and representation of surface winds. However, we agree with the reviewer that the uncertainty estimation needs additional attention. Following the suggestion from the anonymous reviewer #2, we have complemented the manuscript with the uncertainty range estimation based on the interannual variability of dust emission in MERRAero reanalysis. Moreover, we have performed the one-month WRF-Chem simulation of dust storm event in January 2009 to compare with our off-line emissions from this study. The CLM4 results appear to be in good agreement with WRF-Chem, producing similar intensity and spatial pattern of dust emission. The text is modified to reflect this comparison.

5. *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.*

It is well known that dust size distribution is important for dust transport and deposition. However, in the current study, we only simulate dust generation and do not consider transport and deposition. Within this framework, analyzing the size distribution of emitted dust would not allow to reduce uncertainty or make any additional quantitative conclusions on its transport and deposition. To do it, one needs to simulate the full cycle of airborne dust, which is a subject of future research.

6. *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.

The reviewer, probably, refers to weather code reports when talking about the subjective character of the measurements. The visibility measurements that complement the weather code report are not subjective, as they are usually done by ASOS (Automated Surface Observing System) visibility sensor. We agree with the reviewer on his concerns regarding the limitations of visibility and weather code data. Indeed, the detailed discussions about the limitation are already present in the manuscript. However, we cannot agree that visibility measurements are not appropriate for testing dust emission models. In the absence of direct observation of emission, visibility data are the most relevant data sources for these purposes. These observations provide valuable information and may serve as a reference for qualitative comparison with modeled dust emission fluxes and determine optimal model configuration (Engelstaedter et al., 2006; Tegen, 2003). They were used in a large number of dust-related studies. For example, the present weather code reports from meteorological records have been used for evaluation of dust event frequency and dust climatology (Goudie and Middleton, 2006; Shao and Dong, 2006; Wang et al., 2011; Notaro et al., 2013; Yu et al., 2013; Cowie et al., 2014; Hamidi et al., 2014), and derive soil erodibility fields (Shao, 2008). In (Camino et al., 2015; Rezazadeh et al., 2013; Shao et al., 2003) parameterization for assessing near-surface dust concentration from visibility measurement has been proposed. Mahowald et al. (2007) stated that visibility-derived observations should better capture the temporal variability of surface dust fluxes compared to AOD measurements. In our study, we use both weather code reports and visibility measurements to evaluate the frequency and intensity of simulated dust emission.

Specific comments:

1. *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.*

Following the reviewer's comment, we have reformulated this phrase to make clear we obtained this figure from the reanalysis. This was first time calculated so it is a legitimate result of all reasonable means. There are tons of results in the literature obtained from the reanalysis data and nobody question their originality based only on that they are obtained from a reanalysis. The total dust emission estimate from the coastal plain is important for this study and, we believe, has to be clearly outlined in the abstract.

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

It could be proofed straight mathematically, as the integral of the function over the entire globe is less variable than a function itself. Huneeus et al. (2011) reported that globally averaged model estimates of dust emission, deposition and optical properties vary by a factor of 10. Apparently, these discrepancies are driven from even larger regional ones, as global models do

not simulate regional processes. Following the suggestion, we improved the text to make it more clear.

3. *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.*

We should say that the study has been motivated by that the coastal emissions are important for the Red Sea as a significant amount of this material could deposit to the Sea. We agree with the reviewer that as long as the impact on the Red Sea is not calculated directly, the statement should be removed and the objectives to be re-formulated.

4. *Page 4, line 4: Replace "availably" with "availability".*

Thanks, replaced.

5. *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.*

They are the same as in the parent soil. Changed.

6. *Page 5, lines 3-7: How was the setup or configuration of the WRF model defined? Please specify.*

WRF setup generally follows default recommendations from the user guide and is identical to that used in (Jiang et al., 2011). Following the suggestion, we have included this information in the text.

7. *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.*

Thanks, changed.

8. *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?*

The "frequency method" was first proposed by Prospero et al. (2002), and later used in a number of other studies (Ginoux et al., 2010, Ginoux et al., 2012, Schepanski et al., 2012). Following the suggestion, the text was updated with the references.

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

The choice of this threshold has been already explained in the reply to the comment #3 by anonymous referee #2.

10. *Page 9, line 3: Why these two thresholds? Please explain why these two thresholds were used.*

Please refer to the question #7 by anonymous referee #1. Depending on weather station's location, meteorological and environmental conditions, the same visibility reduction may be caused by the different level of dust loading; therefore, changing thresholds could not be avoided. These two particular thresholds were chosen empirically and are aimed to demonstrate that results are not very sensitive to a threshold value.

11. *Page 10, lines 5-8: Please provide a reference for what is said in these lines.*

Following the suggestion, the GOCART aerosol scheme description paper was repeatedly referenced in this place of the text.

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

We have also analyzed the dust emissions in the MACC reanalysis. We have updated the text to make it clearer.

13. *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.*

Thanks for the suggestion. The statement is related to issues discussed in author's own paper. The phrase was reformulated.

14. *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.*

It is not clear enough from the reviewer's remark what particular point of our claims is questioned. Following that, we have expanded the corresponding section to make the discussion clearer. The idea that dust activities in our area of interest have small spatial scales was proposed by Yu et al. (2013). The authors reported low correlations (0.1 – 0.3) between the monthly AOD observations and station dust reports in the west of Arabian Peninsula, compared to much higher ones in the central and eastern Peninsula (usually more than 0.4). This means that, for some reason, dust events reported on stations could not be detected by satellite

instruments. The authors also report that there is a large probability of observing low AOD values on dusty days. Several explanations for this contrast were proposed. Noting the shortcomings of remote sensing instruments that perform worse in the complex mountainous terrain of the western Arabian Peninsula, they claimed that this contrast might be caused by the small spatial-temporal scales of dust processes. In our manuscript, we claim our results to support these ideas and to be consistent with the proposed mechanisms. Higher correlation coefficients of station dust event time series and simulated emission fluxes compared to those reported by Yu et al. (2013) suggest that a large part of detected variability could be explained by local dust generation. Mahowald et al. (2007) also supported this conclusion suggesting that station observations should better capture the temporal variability of surface dust fluxes compared to AOD measurements.

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

Thanks for noting that, we meant the correlation coefficient here. Changed.

16. *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.*

The FineALL experiment was used in the remaining analysis as it has the highest resolution and the spatial correlation for it is the highest.

17. *Page 15, line 30: Replace or eliminate "reasonably". How much is "reasonably"?*

We consider the correspondence of dust emission patterns between the two datasets as reasonable, with regard to the coarse resolution of MERRAero. Most of the hot spot areas that are present in MERRAero are also present in CLM4. CLM4 also features smaller hot spots, that could not be resolved in MERRAero.

18. *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.*

Thanks for this suggestion. Following this and the previous comment, we have updated the text with a more detailed discussion about the location of emission hot spots in our run and MERRAero reanalysis.

19. *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.*

We thank the reviewer for this thoughtful suggestion. We agree that the corresponding statement is not fully correct. We have revised the paragraph and reformulated our claim.

20. *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.*

Thanks for this suggestion. We have made the statement more clear.

21. *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?*

We consider the maximum emission rate an important characteristic that provides the reader with a better understanding of the diurnal cycle of dust generation. Therefore, we prefer to retain the corresponding figure. Following the suggestion, we have expanded the manuscript with a more detailed discussion of the diurnal cycle of maximum dust emission rate.

22. *Page 19, line 1: I would suggest include "estimated" or "calculated" before "emitted mineral fraction".*

Thanks for the suggestion, changed.

23. *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.*

Following the suggestion, the paragraph has been shifted.

24. *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.*

Following our comment above, we suppose this conclusion should be retained.

25. *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.*

Thanks for the important suggestion. The statement has been reformulated.

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

Thanks for spotting. Changed.

27. *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.

Thanks for the suggestion. These few statements have been changed to reflect the reviewer's concerns.

References:

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