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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Evaluation of natural aerosols in CRESCENDO-ESMs: Mineral Dust" *by* Ramiro Checa-Garcia et al.

Ramiro Checa-Garcia et al.

ramiro.checa-garcia@lsce.ipsl.fr

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We thank the referee for the comments and questions. They help us to improve our manuscript and to clarify several points. Here we are indicating our answers in boxed frames after each point raised by the reviewer and our changes/actions in the manuscript within a green colour box. Informative data are given in orange boxes.

INFORMATION: The Table 6 has been double-checked by the different modelling groups. CNRM reported that, instead of our previous estimate, their diagnostics of dry deposition are not including sedimentation which means different values of total and dry deposition (without sedimentation). With this revision the CNRM-6DU model has a larger bias due to an unclosed budget but the CNRM-3DU decreases the previous bias by a factor 2. In this situation we have removed the model CNRM-6DU from the multi-model mean, but we kept the CNRM-3DU. Given scale of the differences between models and observations, the comparison of total deposition draws the same conclusions and the results are very similar. Because the dust emission scheme is not affected by the bias, we kept their results in the analysis. All the Tables and Figures has been revised, and several of them improved according to the new information.

1 General comments

In this study, the authors compare a small multi-model ensemble of mineral dust simulations to observations of mineral dust deposition, surface concentrations, and optical depth. Models perform in diverse ways against the different metrics. The comparison is complicated by the different coverage of mineral dust size distribution by the different models. This is a typical issue in mineral dust inter-comparison papers, which the authors try to work around to an extent but cannot really avoid. The paper gives an avalanche of figures and numbers and comes with a chunky supplementary document. That is not an issue in itself, but the discussion and conclusion sections should make more of an effort to summarise and add value to the analysis.

Regarding the large amount of figures/tables this is partially a consequence of the CRESCENDO approach. In this project for each model, we have a set of several simulations to be analysed and compared. This means that the number of analysis/results (therefore Figures and Tables) are multiplied by a factor 3 with respect to other comparisons, because of the three experiments: PD (present-day), PDN (present-day-nudged) and PI (pre-industrial) described in the main paper.

We have reorganized the discussion and conclusion sections providing additional information. It has been added a new final section with future research.

In my mind, the questions that the discussion should clearly answer are:

(1) Do non-dust differences dominate model disagreement? It seems to me that non-dust factors dominate – at least for emitted mass and load. The authors downplay the contribution of different wind fields, at least in a normalised sense, but highlight the contribution of the "effective" soil erodibility.

We did not try to specifically downplay the role of non "dust scheme" differences. In the introduction we explain the several processes leading to dust activation events. The data analysis and discussion of emission maps has followed a step by step analysis:

1. Prepare for each model, emission maps for both simulations: the one with nudged winds (PDN) and the other one with non-nudged-winds (PD).
2. Compare non-normalised emission maps PD & PDN, named here E and $\mathcal{E}$ respectively.
3. Compare the normalised emission maps PD & PDN, named here ϵ and ε respectively.

We observe that for each model the differences between **normalised** PD and PDN are small (Figure 7), with important differences between models. Let's write the PD normalised emission map of model m with their expected dependencies: $\epsilon_m(\mathbf{r};\mathbf{v_m},\phi_m)$ and for the PDN experiments $\varepsilon_m(\mathbf{r};\mathbf{v_{ERA}},\phi_m)$. With the definitions: $\mathbf{r}$ is the location at surface, ϕ_m represents the dust scheme parameters including soil information (for model m), $\mathbf{v_m}$ are the wind velocities for each model m, and $\mathbf{v_{ERA}}$ the ERA-Interim nudged-winds. Our results indicate that:

$$\mathrm{E}_m(\mathbf{r};\mathbf{v_m},\phi_m) \neq \mathcal{E}_m(\mathbf{r};\mathbf{v_{ERA}},\phi_m) \quad \forall \mathbf{r}, m$$

but

$$\epsilon_m(\mathbf{r};\mathbf{v_m},\phi_m) \simeq \varepsilon_m(\mathbf{r};\mathbf{v_{ERA}},\phi_m) \quad \forall \mathbf{r}, m$$

For one specific model it is possible that $\mathbf{v_m} \simeq \mathbf{v_{ERA}}$ and we can not derive a conclusion of the functional arguments from the last relation. But given that non-normalised maps are different for all models m ($E_m \neq \mathcal{E}_m$), then we consider that it is reasonable to suppose that $\mathbf{v_m} \neq \mathbf{v_{ERA}}$ are different enough, and the functional dependence in the normalised emissions maps of wind fields is less relevant than ϕ_m. In this context we have the interpretation that the comparison ϵ_m for each m is a comparison of a dust *effective* soil erodibility information (DESEI). We remark the two points implicit in our study:

A. The comparison of maps, and therefore the interpretations, are **15 years mean**.
B. Experiments PD and PDN have prescribed and identical sea-surface temperatures.

In the context of (B) a reduced diversity between the models is expected compared to fully coupled models. Because of (A) all our discussion is for our estimate of climatology emission maps. Note that we understand that the wind speed thresholds in the dust scheme are part of the DESEI, and the DESEI may also depend on the dust particle size distribution imposed during the emission process. Finally, the DESEI has still a sort of "meteorology" as far as it includes information of, for example, soil moisture.

We have clarified better the process we followed, and we have added some emphasis on the fact that each normalized emission map is calculated monthly and we presented the 15 years mean. Future research will analyse possible seasonal discrepancies.

In the discussion section we modified our previous text by:

To overcome the challenge of comparing models with different DPSD at emission, we introduced normalised emission maps, showing first (by a comparison between PD and PDN simulations) that wind fields do not substantially affect these normalised emission estimates in terms of spatial patterns when we analyse the 15 year emissions means of the PD and PDN simulations. This led us to interpret differences in regions where dust was emitted as reflecting differences in the underlying dust *effective* soil erodibility information (DESEI) among models. However, the DESEI is also including a sort of meteorological factors because the role of soil moisture in the emission process, together with specific properties of the dust scheme like the threshold in friction velocity or how the soil texture is translated into a dust size distribution. Note that the simulations compared in our study share the same sea-surface temperatures which reduces the model diversity in terms of precipitation. Nonetheless, the consistency we report between PD and PDN normalised emission maps needs further investigation at smaller spatial and temporal scales, in particular at daily and sub-daily scales.

How can model disagreement be resolved? The paper tries to explain some model differences, but the CRESCENDO simulations are not sufficient to go beyond speculations. But the results indicate where future inter-comparisons must do more: a simulation with prescribed soil properties, including moisture, is clearly required. Mass diagnostics integrated over the size distribution are clearly a barrier to understanding, so must be replaced with diagnostics for the different size modes/bin of each model, which can be remapped to common bins for comparison purposes.

We agree that the set of models used and the number of simulations (our sample size) although reasonably comprehensive of different dust scheme approaches, still is not exhaustive. We tried, when possible, to translate our study into numerical assessments, but we are cautious about directly extend our conclusions (or "ensemble values") to models/comparisons outside our analysis.

Once this premise is clear, still our analysis highlights several paths to overcome the model diversity:

- First we have proposed a simulation with prescribed soil properties by using a *benchmark reference dataset* regarding soil information, but we will clarify better this point in the discussion.

- In the CRESCENDO design of the simulations a key point has been to provide some diagnostics per-bin or per-mode. This first study includes already a large amount of analyses and comparisons. This means that a comparison per size range (and details about vertical distribution) will be incorporated into future publications. But we totally agree about the key role of analyses per bin/modes. As part of the set of software tools that we have created for this and future studies, we have included a set of methods to translate modal distribution variables into diagnostics over specific bins to perform that set of comparisons (see the following link to read the online manual with examples: FunFAN software manual).
- From the point of view of the optical properties we have shown that the loadings can be very different despite a similar dust optical depth. This points to an analysis on how the optical properties of dust are implemented to have a better convergence. First at the level of the refraction index but also the full set of hypothesis that explain the different optical parameters.

What observations can support further progress? The paper uses existing observations very well. I was struck by the absence of aircraft data, which seems to imply that all those expensive aircraft campaigns dedicated to mineral dust do not measure the quantities that are needed to improve models.

We absolutely agree about the important role of the aircraft data for mineral dust (and also other aerosols). The absence of a comparison with aircraft measurements doesn't mean, from our side, that they are not useful to improve the models. It can be actually the opposite, these measurements deserve a specific study about the representation of the vertical structure of dust in the models (with satellite soundings and aircraft measurements). Note that we also did not compare with specific measurements of wet vs dry deposition flux, or fine vs coarse optical depth. Both are important and useful, and should be part of future research.

The authors have the opportunity to say what those quantities are: size distribution, clearly – with the need to go beyond case studies and constrain the climatology. Mass extinction efficiency looks important too. Something else?

In the case of dust, and looking to aircraft measurements a key point (beyond those already commented by the reviewer) is the collection of mineralogy samples. First, it will provide information to understand the divergences in terms of optical properties beyond the size distribution. Specific minerals also produce indirect effects like those of heterogeneous chemistry, or cloud droplets formation for instance. The shape of the particles is important to quantify uncertainties in lifetime. Finally, we need more studies about the mineral fractions per size bin to understand better future paths in modelling of the largest particles.

2 Other comments

- Page 1, line 8: “uncertainty”: “diversity” would be preferable because it is unlikely that 5 models sample the full uncertainty range.

  We agree with the terminology *diversity*, we will use it along the paper. Thank you.

- Page 1, line 9: how many models in that subset?

  There are 6 different models/dust schemes, with two models including explicitly the largest particles. Therefore, this subset has 4 models (6 minus 2). We indicate it better.

- Page 1, line 10: “better consistency between models”: all models, or the subset?

  For all models in PDN experiment. We have clarified it better.

- Page 1, lines 14-17: The abstract needs to say what the conclusions of these two tasks were.

  We added: The global localization of source regions is correlated with MODIS, but the actual time-series per region has a diversity of values per model and differences with observations.

- Page 2, line 23: Could say that the estimate by Kok et al. (2017) comes from observations and models.

  Thank you for the comment, we added this information.

- Page 2, line 29: Could note that the impact of mineral dust on the phosphorus budget of the Amazon may be smaller than previously thought, based on Prospero et al. 2020

  Thank you for the comment, we added this information.

- Page 4, lines 21-22: What are the differences reported by Yu et al. 2019 due to?

  We think that the differences should be related with different dust activation processes, in the case of Taklamakan previous studies like Ge et al. (2016) proposed an important role of nocturnal low-level jets. This explains the seasonal differences in the frequency of dust events between Taklamakan and Gobi deserts.

  We have improved the text in the main paper with:
  Recently, Yu et al. (2019) reported differences in the frequency of dust events between the Gobi (very high frequency of dust events in March and April) and Taklamakan (more than half of the events from May to September) deserts, which can be interpreted by a larger role in dust activation of the nocturnal low-level jet in Taklamakan Ge et al. (2016).

- Page 6, line 21: “non-mixed”: it is more usual to say “externally mixed”

  Thank you, we agree, we have changed the "non-mixed" to “externally mixed”.

- Page 6, line 23: what experiments?

  We are speaking about Denjean et al. (2016), Ryder et al. (2018) and Ryder et al. (2019). We have improved the sentence.

- Page 7, lines 24-26: That seems to be an example of the processes mentioned in line 20, so could be moved there.

  Thank you. We have followed your comment and reordered the paragraph, and added also a reference recommended by other reviewer.

- Page 7, line 27: The information in Section 2.1 would be better described by a table of experiments.

Thank you. We have added a new table to summarise the model experiments. Here it is also shown:

CRESCENDO-ESM experiments analysed: PD (Present Day), PDN (Present Day with nudged winds), PI (Pre-Industrial aerosol and chemistry forcings). The sea-surface temperatures (SSTs) and ice cover are prescribed based on CMIP6-DECK-AMIP (Durack and Taylor, 2018). The solar forcing is using the input4MIPs dataset (Matthes et al., 2017) but NorESM uses the previous dataset. The gas and aerosol emissions are consistent with CMIP6 but depending on the complexity of the gas-phase species, ozone can be prescribed with either ozone concentrations from a previous full chemistry simulation or the input4MIPs ozone forcing dataset (Checa-Garcia et al., 2018; Hegglin et al., 2016). Wind fields used for the specified dynamics are obtained from re-analysis of ERA-Interim (Dee et al., 2011)

	PD	PDN	PI
Time Period	2000-2014	2000-2014	2000-2014
SST and ice cover	prescribed	prescribed	prescribed
Aerosol Precursors	Present-Day	Present-Day	1850
Anthropogenic Emissions	Present-Day	Present-Day	1850
Solar Forcing	Present-Day	Present-Day	Present-Day
Wind Fields	modelled	prescribed	modelled

- Page 11, Table 3: the units of MEE are given as $m2g-1$ in Table 5. It would be good to harmonise that.

We aimed to include the units of the original CRESCENDO diagnostics or derived in SI, but we have added a note to clarify this difference with Table 5. Thank you.

- Page 12, line 4: What is meant by “along the seasonal cycle”?

We have changed that by "all the months of the year". This means that the relation $\tau_{440}^{dust} > 0.5\tau_{440}^{all-aer}$ should be true for all the models, and for each model for all the months of the year.

- Page 14, lines 29-32: The low regard given by the author to Pearson correlation is surprising since that measure is used extensively throughout the paper. I suggest toning down that statement or clarifying that it only applies to specific comparisons.

It is not easy to tone down more the description about Pearson correlation as they are mathematical properties. But we have clarified that the reason to compare with another correlation estimator is that we can not show the scatter-plots of the involved variables to visually ascertain the performance of the statistic used.

Thank you for the comment. **We have changed our previous sentence by**: *Given that this statistics is not robust and only representative of linear relationships, the skill is also estimated based on the Spearman rank correlation.*

- Page 19, line 27: “being the only model” – is that CNRM-6DU?

Yes. We have been more explicit.

- Page 23, lines 5-6: But does CNRM-6DU match the Adebiyi and Kok (2020) estimates for the right reasons? Adebiyi and Kok (2020) estimate the burden of coarse-mode (larger than 5 microns) dust to be 17 Tg. Does the model also match that number?

Here we refer only to total values, we did not evaluate per-bin differences (coarse vs fine modes).

- Page 28, line 25: Is that so remarkable? The models must prescribe fairly similar soil properties.

  We agree that this property is expected, but we considered it worth to be mentioned. It is true that it seems that all models are prescribing similar soil properties in Bodélé, but not in other regions like Australia and several Asian regions. So we highlighted the agreements by indicating also those regions with fair consistency (Bodélé).

- Page 30, lines 11-13: Did the CNRM model do something specific to represent Hoggar emissions?

  Not specifically, it is a result of the dust source information implemented.

- Page 45, lines 25-30: What about the LW? It would probably be the other way around, so there should be cancellation of error in size distribution between the two spectra.

  This is a question that we asked ourselves. However, the estimate of DRE in the LW done by the RRTM (the radiative transfer model used in our calculations) is not including the LW scattering (only absorption) therefore we considered it better to not conclude about LW and SW error cancellation explicitly as we can't support it with calculations. We have added a text to explain this important point.

  We added (at the end of Section 5.1): It is important to note that the DRE shown in Table 6 is estimated without scattering in the LW (only absorption). In the case of mineral dust to neglect the LW scattering implies an underestimation of TOA-DRE-LW (Dufresne et al, 2020), mostly in cloud conditions.

- Page 44 line 31: What is the difference in terms of content between section 6 Discussion and section 7 Conclusion? They seem to both be a mix of summary

  and further discussion, so could be merged.

  Thank you. We have refactored both sections, and we have followed the advice to merge both discussion and conclusions. We have extracted the recommendations for future research to have a specific section.

- Page 45 line 6-8: Where has the discussion on effective erodibility taken place? It is the first time the paper mentions that concept.

  We have reorganized the discussion and conclusion sections of the paper and improved the cross-references in the paper. As commented before we introduced this concept to interpret the normalised emissions maps. As far as, we have identical wind-fields in the wind-nudged simulations, we consider that these maps highlight differences in terms of mixed soil erodibility properties: soil properties like texture, surface roughness length, bare soil fraction, area efficiency factors etc. We named this dust effective soil erodibility information (DESEI) as we can not separate explicitly each component.

3 Technical comments

- Page 3, caption of Table 1: extra word "of about"

  It is corrected. Thank you.

- Page 3, line 20: "indicates" -> "indicate"

  It is corrected. Thank you.

- Page 42, line 10: "correspond at" -> "correspond to"

  It is corrected. Thank you.

- Page 44, lines 14-15: What is meant in the part starting with “although with”?

  It is Corrected. Added ’,’

- Page 47, line 48: typo: “an scarcity”

  Corrected by ”a scarcity". Thank you.

- Page 47, line 12: Rephrase “which resulted to be challenging”

  We improved the sentence: resulted -> improved. Thank you.

- Page 50: Grammar of the last sentence of the acknowledgment could be improved.

  We improved the sentence. Thank you.

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