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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.

Anonymous Referee #1

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

- 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.
- How can model disagreement be resolved? The paper tries to explain some of the 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.
- 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. 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?

1 Other comments

- Page 1, line 8: "uncertainty": "diversity" would be preferable because it is unlikely that 5 models sample the full uncertainty range.
- Page 1, line 9: how many models in that subset?
- Page 1, line 10: "better consistency between models": all models, or the subset?

- Page 1, lines 14-17: The abstract needs to say what the conclusions of these two tasks were.
- Page 2, line 23: Could say that the estimate by Kok et al. (2017) comes from observations and models
- Page 2, line 29: Could note that the impact of mineral dust on the phosphorus budget of the Amazon may be smaller that previously thought, based on Prospero et al. 2020 https://doi.org/10.1029/2020GB006536
- Page 4, lines 21-22: What are the differences reported by Yu et al. 2019 due to?
- Page 6, line 21: “non-mixed”: it is more usual to say “externally mixed”
- Page 6, line 23: what experiments?
- Page 7, lines 24-26: That seems to be an example of the processes mentioned in line 20, so could be moved there.
- Page 7, line 27: The information in Section 2.1 would be better described by a table of experiments.
- Page 11, Table 3: the units of MEE are given as m^2 g^{-1} in Table 5. It would be good to harmonise that.
- Page 12, line 4: What is meant by “along the seasonal cycle”?
- 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.
- Page 19, line 27: “being the only model” – is that CNRM-6DU?

- 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?
- Page 28, line 25: Is that so remarkable? The models must prescribe fairly similar soil properties.
- Page 30, lines 11-13: Did the CNRM model do something specific to represent Hoggar emissions?
- 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.
- 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.
- Page 45 line 6-8: Where has the discussion on effective erodibility taken place? It is the first time the paper mentions that concept.

2 Technical comments

- Page 3, caption of Table 1: extra word “of about”
- Page 3, line 20: “indicates” -> “indicate”
- Page 42, line 10: “correspond at” -> “correspond to”
- Page 44, lines 14-15: What is meant in the part starting with “although with”?

- Page 47, line 48: typo: “an scarcity”
- Page 47, line 12: Rephrase “which resulted to be challenging”
- Page 50: Grammar of the last sentence of the acknowledgment could be improved.

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