Comment on *Importance of different parameterization changes for the updated dust cycle modelling in the Community Atmosphere Model (version 6.1)*, by Li et al. 2022.

General comments

This article presents multiple developments included in the dust cycle representation within the CAM6.1 model and assesses their impact on relevant variables, such as the dust surface concentration, deposition, size distribution, optical depth and direct radiative effect. The work conducted provides relevant information beyond the dust modeling community, as dust has impacts on different features of the atmospheric dynamics and chemistry, the climate and the Earth System. As such, I believe this article is well within the scope of the Geoscientific Model Development journal, it presents novel results, and it deserves publication.

However, in my view, in its current form the reader has to put in a considerable effort to follow the details of the massive amount of work presented.

The authors present nine different experiments: five defining dust as a bulk species and four experiments considering speciated dust. This involves a duplication of experiments in which one (or several) of the new developments are tested, and adds an additional variable to the analysis, making it harder to focus on the specific impact of the new aspects included in the model.

In order to lighten up the contents of the paper, I would recommend splitting the results in two different articles, one focusing on the current developments and their impact on the bulk dust cycle, and another focusing on those improvements that potentially have an impact on the mineralogy (e.g. the changes on the emission scheme).

With respect to the experiments design, the authors could better clarify the criteria used to include the new features in the tests. Instead of relying on a baseline (e.g. CAM6.1), and adding separately to that configuration the different developments (on the emission scheme, dry deposition, size, or asphericity), the authors combine multiple developments in the different experiments. I believe these combinations could hinder a clean comparison of the effect of each development (e.g. looking at Table 4 it is difficult to know which pair of experiments allows disentangling the effect of shape and deposition changes). This issue is accentuated by the fact that the experiments are referenced along the manuscript by different names or acronyms, which further complicates tracing them.

Then, I believe that a fundamental piece of this article is the variety of observations, retrievals, model-derived products and model results that are used for the model evaluation. The modelling community could greatly benefit from the effort done here to compile that information and produce a benchmark for dust properties evaluation at the global scale (in present climate). Unfortunately, these are only presented in the article in a summarized manner (through a table). I would recommend adding in the manuscript at least a discussion on the variables available, their usefulness for modelled dust evaluation and their limitations.

Finally, I would recommend modifying the organization of some of the contents, and re-writing or improving some parts of the text. Also, in some sections, the authors rely excessively on external references, making it difficult to follow the discussion with the information provided in the paper itself. My recommendation would be to restructure or adapt the article contents, such that:

(1) the previous status of the model is clearly defined and the motivation to improve or change the specific dust representation is justified.

(2) the new developments are described in the current paper in a comprehensive manner (i.e. not trusting excessively on the reader to go and check the external references).

(3) the evaluation methodology is explained before the presentation of results, for instance adapting current section 3. It would be particularly useful to identify the multiple metrics that are going to be used for the model evaluation and their purpose (i.e. regional variability, temporal variability, etc.), comment on the dust tuning methodology and its impact on the evaluation metrics (if any), as well as to merge the description of the observations with the comments on section 5 about the limitations of the datasets. Section 5 could be kept to provide an overall assessment of the observations limitations on the main conclusions of the article.

I believe that with these changes, the article would be much easier to follow and it would reach a broader audience.

Specific comments

Introduction

I believe this section could be slightly re-structured, particularly to better clarify the current model status, justify the need for improvement in the specific aspects that are dealt with in this work, and briefly explain how these are going to be approached.

2. Model descriptions

I would recommend starting by describing the aerosol representation in CAM6.1, as it affects both bulk dust, speciated dust and other aerosols simulated in the model.

Please, see my general comment above. Which is the added value of conducting two set of simulations (with bulk and speciated dust) for the purpose of this article (assessing changes due to deposition, emission, size distribution and shape)? If this is not justified, I would focus on this article in the bulk dust experiments, and present the speciated dust experiments elsewhere.

2.3. Dust optical properties and radiation flux diagnostic.

Please, take advantage of this section to explain aspects related to the calculation of optical properties and/or radiative variables that are currently explained in the results section (see my comments below on sections 4.3 onwards).

2.4.2. Dry deposition schemes.

The original dry deposition scheme is partly described here and partly in the introduction. I would use this section to describe the details on both the previous and the new proposed scheme. At least, I would include here the references to both schemes, and clarify if the empirical coefficients are updated in the new scheme.

2.4.3. Dust asphericity

Being this one of the developments listed in the article, it would be worth to include in this section at least the main characteristics of the development (e.g. factor varying according to the source region, and ranging from X to X).

Also, the authors mention the impact of the dust asphericity on optical properties (line 119). In section 4.2.3, they state that CESM2 does not include the enhancement in mass extinction efficiency due to asphericity, but that it is considered in this study (section 4.2.3). I believe the approach used to consider asphericity in the mass extinction efficiency should be clarified and described in this section (2.4.3).

2.5 Experiment design

Please, see my general comments related to the experiments' design.

I would recommend to describe first the common model configuration amongst experiments (i.e. configuration of the model components, spatial resolution, period simulated, etc.), and then identify the experiments designed to test the different developments.

3. Observational datasets for model evaluations

Please, see my general comment above.

Table 3 now includes a list of different observations, retrievals, and products using a combination of models and observations/retrievals. I believe some of the information of the table, and particularly that included in the column “Comments”, is relevant enough to be included in the main text. It would be also helpful to relate the different datasets to the limitations identified in Section 5. If possible, in the Supplement organize the observations by type, e.g. do not mix surface dust concentration with dust optical depth retrievals.

The DustCOMM reference and the models included in the Adebiyi et al. (2020) paper could be mentioned here, as they are used in section 4.1.4 to compare the model size distribution.

4. Results

Please, review and re-structure this section, see my general comment above.

I believe using the same set of experiments to discuss all the modifications (either bulk or speciated dust) would help. In addition, a discussion focusing on the different variables, combining the multiple datasets used as a reference, rather than a separate explanation for each comparison could be of benefit. Another strategy to make easier the discussion for the reader, could be to “qualify” the sites / observations by their characteristic trait when explaining the details, e.g. source region, remote station, etc., rather than leaving it to the reader to figure out where the site is or its characteristics.

4.1.1. Dust emissions

Why compare the total emission burden with model estimates that go beyond CAM6.1 simulated size range? I believe it would be useful to include comparisons with models that use the same range (e.g. some of the AEROCOM phase I models, Huneeus et al. 2011).

4.1.2. Climatology annual means of [...]

The discussion here will greatly benefit from a previous definition of the statistics, metrics, and evaluation, which I would suggest including in Section 3. In that way, the authors could make the discussion in this section lighter.

The authors mention the tuning as a factor affecting the comparison of modelled DOD to MODIS and Ridley et al. (2016) products; however, this is not taken into consideration when AERONET information is used as a target. Could the tuning also have an effect on those results?

Does the dust wet vs. dry deposition balance in their model change with the improvements on size distribution? Could this partly be explained by an overestimation of the finer dust fractions? Or is the representation of modal internal mixtures more relevant to this process?

4.1.4. Size distribution of transported dust

Why is the comparison with AERONET presented in the supplement?

4.2.1. Dust emission schemes

Please, avoid relying on excessively on external references to explain features observed among the experiments (e.g. lines 561 to 563), summarize them directly here.

What is the impact of the dust tuning on the results? According to section 2.5, both EXP06 (MINE_BASE) and EXP07 (MINE_EMIS) were tuned to match a global DOD of around 0.03. Was that not the case? What does the re-scaling of the DOD mentioned on line 591 refer to?

4.2.3. Dust asphericity

The authors state that the dust asphericity could mediate the overestimated dust emission from source regions, is this shown in their experiments? Does the asphericity factor affect differently fine vs coarse particles?

4.2.4. Dust size representation

This section is difficult to follow, please, revise.

4.3. Dust direct ratiative effect.

Details such as the LW increase by 51% could be explained in section 2.3. I would only mention this again here if the approach used in the different experiments would differ, and thus affect the comparison.

4.3.1. Dust direct radiative effect efficiency.

Please, use also section 2.3 to define the net DRE efficiency.

What is the metric used here to define the model performance?

The difference between the experiments with speciated and bulk dust is not exclusively dependent on the developments presented here, but, as the authors mention, attributed to the resulting optical properties for the different representation on the dust.

Does the model diagnose all sky or clear sky DRE (line 730)? Please, clarify this in section 2.3.

Conclusions.

The authors mention the effect of dust asphericity on mass extinction efficiency as one of the aspects that produces a larger change in the results, as mentioned above, it is unclear to the reader which is the approach followed to introduce this in the model and/or if it's introduced at all.

I believe it would be useful to include a brief discussion on the implications of reverting the standard deviation changes in the coarse mode for the stratospheric aerosols. If the change was initially introduced to better accommodate those, which would be the recommendation of the authors for the model version to be issued?

The authors comment on potential ways of improving further the dust cycle, however, it is unclear for the reader if those stem from the work performed in this article. I would recommend to highlight the weaknesses detected in this study concerning the dust cycle representation (even after all the improvements included), and link to the appropriate suggested next step to solve that issue.

Technical corrections

Please, find below a list of technical corrections that could be applied to the current manuscript version.

L19. Either refer to the CAM6 model in the abstract (as it is in the article title) or change the title to include the CESM model.

L23-24. If possible, outline the main changes included in the different parameterizations (emission, dry deposition, size distribution and dust particle shape).

L26-27. Is it the effect of the size distribution change as large as the change in the dust emission scheme?

L46. Is shape also a factor affecting the uncertainty in dust direct radiative effect?

L63-64. Is it necessary to mention the previous CAM and CESM versions?

L71. Why do the authors mention now the Community Land Model version 5 (CAM6.1/CLM5)? Please, use the same acronym/naming convention all along the article, either CAM6.1 or CAM6.1/CLM5, or at least, mention the full name the first time it appears and explain that from then on it will be referenced as CAM6.1.

L102 (Table 1 caption): MAM4 is mentioned for the first time. Why use two abbreviations for the standard deviation, remove extra dot after CAM6.1 in L103.

L108: Homogenize the naming of the sections, either Sect. or Section.

L109: I would substitute semi-observation by more specific term(s).

L117: Is it CESM2.1 or CESM2? Please, keep consistency in the naming of the model versions along the document.

L125: Why is the iron solubility mentioned here?

L126: I would state in the introduction that the tests are to be conducted under present climate conditions, this will already justify using observations for the same period and then the clarification on the pre-industrial will not be needed here.

L138: Please, change “models” by model.

L139: Please, remove “generally”.

L141: CESM2, CESM2.1? CLM?.

L153: Please, rephrase to specify the variable that is independent of the friction velocity (rather than the theory itself).

L154-155: As it is expressed now, the improvement in CAM4 size distribution is not informative to the reader. Please, either remove the part about the improvements or to briefly explain the difference between the approaches in previous CAM4 PSD and that derived from Kok (2011).

L156: Please, remove “other”, and “of aerosols”.

L179: Please, remove “the so-called”.

L178: As mentioned above, please, select just one acronym for the standard deviation.

L189: Please, change “their ranges”, by “its ranges”.

L195: The reference to Scanza et al. (2015) was already included.

L221: Is the vertical transport modified per se? Or is it indirectly affected by changes in emission/size?

L231: What do the authors mean by “although even dust modeling with BRIFT can be improved if optimized against observations”, is that optimization relevant for this specific study?

L328: Please, avoid repeating references unnecessarily (e.g. remove described in Sect. 2.2).

L333: There are two references for Kok et al. (2021), please, specify a or b.

L338: Please, change “could change”, by the appropriate: does or does not change the model performance?

L338-343. May not be necessary to explain again the content of each sub-section.

L358: Please, explain what the binned method is.

L466 (and other locations in the text): Please, refer to the different *experiments* as such, instead of mentioning the *models.* If preferred by the authors, they could use *model versions*.

L369: Please, identify the reference with a or b.

L432: Typo: averages.

L439: Change “to the low” by “to the lower”.

L475: Have the authors information on the precipitation evaluation for their own model?

L524: Please, include the coordinates of both stations or none.

L543: Does the super coarse dust start at 10 um? or larger diameters?

L614: Hematite and illite have a high iron content, feldspars not much. The sentence could be rephrased as “, including hematite and illite, and feldspar”

L636: I believe the increase is in wet deposition (not dry), please, verify.

L655: Please, include the full reference and then in parenthesis the values.

L683: Why is the calculation explicitly included there? It makes the text more difficult to read. I would avoid it (here and in other locations in the text below).

L699: The sentence “where the dust emission occurs in transport” is difficult to understand, please, clarify.

L869: Substitute “new model” by the appropriate model version name.

Supplement:

The caption in Figure S2 mentions the sub-regions being defined in Figure S3, but Figure S3 only lists the names of the regions, and those do not correspond to the legend in Figure S2. Please, review.

Tables and figures

Table 1: I would order the modes from smaller to larger in size. I believe this table could be included in the supplement and leave in the text exclusively the default and new configuration for the coarse mode.

Table 4: Why is the dust SSA for NEW_EMIS_SIZE missing?

Please, homogenize the naming convention for the different experiments, here tagged in Table 4 as NEW_EMIS, NEW_EMIS_SIZE, etc. In Table 2 and sections 2.1 and 2.2 they were listed also as EXP01, EXP02, etc. In Table 4 caption CAM6S5 and CAM6S6 are mentioned, which were not identified nor described before.

Table 5: Could the locations be represented in a map, together with the other observations location?

Figure 1: Which is the metric used to define the improvement (+) or worsening (-) of the comparison? Remove the comment on Figure S3 from the caption, and if needed, clarify in the text (line 392) the information presented in main paper and in the supplement.

Figure 2: Could the re-scaling factors now explained in the caption be included also in the figure legend (e.g. above each map)?

Figure 5: Please, review the caption: remove “and” in the third line, remove “for the abbreviation for other models”, either explain them there or leave just the reference, specify what do we understand by semi-observations. Please, do not refer to other figures in figure captions unless they are needed to understand the figure contents.

Figure 6: What do the maps represent? Is it the ratio? Or the differences over the reference?

Figure 7: Please, use the same naming convention for the different experiments along the manuscript, otherwise is very confusing.

Figure 8: Homogenize the experiment names with the rest of the document, review the seasons listed in the caption, the inserted map below is not shown in this document version.