25/04/2022

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

In this paper, Li *et al.* investigate the sensitivity of dust in the CESM2-CAM6.1 climate model to various parameterised processes: the emissions scheme, the dry deposition scheme, the fixed geometric width of the coarse mode, and the assumption of spherical/aspherical particles. Using a wealth of validatory observations and many simulations, they find that changing dust emissions and the coarse mode width have the greatest impact on the dust metrics, followed by the dry deposition scheme and then asphericity. They also propose a new version of CAM (CAM6.α) which improves on many dust metrics relative to CAM6.1 and incorporates some of the listed process changes.

The paper is well written and contains a wealth of useful information, including the most comprehensive database of dust observations yet (Table 3). The introduction is highly readable, and the conclusions are generally supported by the analysis. However, this paper rather feels like 3 independent studies convoluted together, namely, (1) a new and improved version of the dust scheme in CAM (CAM6.1 versus CAM6.α), (2) a study of the sensitivity of simulated dust to certain processes, and (3) a study of the merits of separating dust into its mineralogical components in CAM. I think the paper would benefit from being split into 2 or 3 separate papers, which I expand on below in the General Comments. In short, I think that the study needs a redesign before it is published, which may require major revisions (i.e., new simulations and a re-write) and/or splitting into separate papers.

General comments

Firstly, I think that the simulation design is incorrect for exploring the sensitivity of dust to the altered processes. For example, the new dry deposition scheme is only tested in conjunction with the other altered processes (CAM6.α and CAM6.α_MINE) and never on its own. Conversely, the new emissions scheme is tested by itself for both BULK and MINE dust models, whilst the size and shape of the particles are tested in conjunction with the new emissions scheme but using BULK and MINE dust respectively. In short, it's very difficult to attribute the impacts on the dust metrics to the individual processes.

I would suggest concentrating on either the BULK dust scheme or the MINE dust scheme, unless you plan to directly compare them. The study would be much cleaner if the processes were tested in isolation using either BULK or MINE and then compared to CAM6.1 (see Table below). In its current form, it is very difficult to disentangle which dust impacts emanate from which altered process.

Suggested simulations:

Simulation	Name	Description
1	CAM6.1	Standard model
2	NEW_EMISS	CAM6.1 with BRIFT emissions
3	NEW_SIZE_S5	CAM6.1 with CAM5 size assumptions
4	NEW_SIZE_S6σ5	CAM6.1 with CAM5 assumptions except coarse σ from CAM6.1
5	NEW_DRYDEP	CAM6.1 with PZ10 dry deposition
6	NEW_SHAPE	CAM6.1 with aspherical dust
7	CAM6.1_MINE	Equivalent to MINE_BASE but may use CAM6.α as BASE simulation
8	CAM6.α	CAM6.1 with all of the relevant model changes

Another issue that I had with the simulation design was the arbitrary tuning of dust optical depth (DOD) to 0.03 in some simulations but not in others (L289). This made it very difficult to quantify the impact of the altered processes and forced the authors to add caveats throughout the text e.g., L590 *"differences between the global annual mean dust deposition in BRIFT and DEAD would become smaller, if we rescaled the value according to the same DOD criteria"*. I suggest only tuning CAM6.1 and CAM6.α to 0.03 and using the tuned CAM6.1 as the BASE model in which to add the different processes incrementally. I see no need to rescale DOD in the sensitivity simulations and it would be interesting to see the impact of the different processes on the global-mean DOD as a derived product of the models. Tuning to 0.03 is arbitrary and also misses the fact that much of the dust mass is in the super coarse mode which is missing from the model, and therefore the model may be wrongly tuned to 0.03.

It is also confusing for the reader that some simulations have emissions scaled by 1/f_clay whilst others have the scaling as 1, and so the impact of this change is difficult to disentangle using the current suite of simulations. It would be better if this factor is consistent across the simulations or tested in isolation.

I gather from the text (L649) that the impact of asphericity on the dust mass extinction efficiency (MEE) is represented in *all* of these simulations. This is rather confusing, as it suggests some representation of asphericity is incorporated even when dust is assumed to be spherical (?). Please clarify this for the reader. In particular, please state whether the impact of asphericity on MEE is only applied in the simulation with dust asphericity or in all simulations (which seems inherently wrong). Really these details should be included in the Methods (L98, L224) and not in the result section.

In terms of the presentation of the results, I thought that comparing CAM6.α with CAM6.1 before looking at the individual processes was confusing, as much of the analysis of the impacts of individual processes could have been used to explain differences between the dust metrics in CAM6.1 and CAM6.α. Additionally, the authors say the following in Section 2.5:

"It is worth noting that dust burdens and deposition fluxes would be comparable, if the bulk and speciated dust models have similar DOD. But the dust optical properties (e.g., single scattering albedo) in the bulk and speciated dust simulations differ, resulting in considerably different dust direct radiative effects and direct radiative effect efficiencies. Therefore, we state the difference in the dust DRE and DRE efficiency estimate in Sect. 6, but do not document the comparison of dust loadings/deposition/DOD between the bulk and speciated dust simulations."

Given that DOD is tuned to be similar in these simulations, I do not see why the differences in optical properties should be used as an excuse not to compare BULK with MINE. This would be a very interesting study in its own right, and possibly the authors should omit MINE simulations in this paper as without comparing BULK with MINE, it is difficult to understand why MINE is used at all. Is the additional mineralogical detail in MINE useful for a better dust simulation? What is the additional computational expense of MINE over BULK? Is MINE being considered for inclusion in a future of CAM or is this rather an interesting pedagogical study? Currently, MINE is frivolously used in this study and is unnecessary without further analysis and comparison.

In summary, I would highly recommend that the authors run further simulations with each of the processes applied separately as the current simulation design is not conducive or particularly supportive of the results presented in the manuscript.

Specific comments

[L75] Is it worth introducing the DEAD and BRIFT acronyms here?

[L84] The fine mode is described as d < 1um whilst the coarse mode is d > 5um. Normally, the coarse mode is adjacent to the fine mode so I wonder what the authors would define the intermediate aerosol (1 < d< 5um) as?

[L91] *"one of the changes from CAM5 to CAM6.1 was replacing the size distribution of aerosols in the coarse mode in CAM5 with the one that has a much narrower width in CAM6.1"*- this seems nonsensical to me, or completely without consideration for actual coarse mode dust widths (e.g., Ryder et al, 2013, 2018, 2019 suggest $\sigma \in [1.6, 2]$ rather than 1.2). Why was it decided to favour stratospheric sulfate over tropospheric mineral dust when sulfate is more episodic (e.g. volcanic eruptions) and has less of an impact over tropospheric climate? Also, the authors seem to recommend that the coarse mode width be reverted to 1.8 as in CAM5 (I agree), but do not comment on the impact of resetting the coarse mode width on stratospheric sulfate. Seeing as this was the initial motivation for contracting σ, I think that some comment is appropriate.

[Table 1] I think that GMD should be labelled as "initialisation GMD" as this is more descriptive. Or is the initial GMD at source calculated online? It is difficult to tell from the text what the initial GMS is. This also refers to L179.

[Table 1] Why is the order of the modes Accumulation, Aitken, Coarse, then Primary? Surely it should be in ascending size order: Primary, Aitken, Accumulation then Coarse

[Table 1] Why was the accumulation mode width changed in CAM6.1? What are the impacts of reverting it? I can't see this detail in the text

[L109] The term 'semi-observation' is undefined and is confusing

[L115] *"show the final summarization in Section 7"*. This is an unusual way to say "Discussion and conclusions are provided in Section 7" or something to that effect

[L120] This is one of the places in the text where it is unclear as to: (1) whether the impact of dust asphericity on MEE is represented at all, (2) if it is represented then in what way (methods), and (3) which simulations include it?

[L137] Sentence beginning *"We consider the default DEAD scheme"* should explicitly acknowledge that it refers to emissions

[L143] How confident are the authors in the critical LAI threshold? Should this assumption be discussed in the Discussion section?

[L152] The mass is distributed as 0.1 %, 1 % and 98.9 % between the Aitken, accumulation, and coarse modes. Surely these ratios should change depending on the assumed coarse mode width?

[L160] Many dust schemes treat dust as initially insoluble and then permitted to age via coagulation and condensation wherein it becomes soluble and internally mixed (e.g., dust in UKESM1). The authors should comment on their assumption of internally mixing dust, which may artificially enhance dust deposition near source regions? Would you expect similar results if dust is assumed to be insoluble?

[L165] The Neale et al (2010) reference is an internal document, which I can't find online. Can the authors please provide a URL for downloading the report, or alternatively, relevant peer-reviewed papers with the same information.

[L172] *"The wet deposition rate thus depends on the hygroscopicity of dust (=0.068; Scanza et al., 2015) as CCN/INPs and the prescribed scavenging coefficient (=0.1; Neale et al., 2010), both of which are currently constant with respect to the dust size (and composition for speciated dust) in CAM6.1."* – I assume the hygroscopicity of dust will evolve as dust is transported through the atmosphere so I question the use of a single spatially uniform constant for this parameter. The below cloud scavenging coefficient (0.1), if it is in units of s^{-1}, seems 2 orders of magnitude too high (Wang et al., 2010, doi:10.5194/acp-10-5685-2010). Wang et al (2010) for instance, suggest it's somewhere between 10^{-6} for accumulation mode aerosol and 10^{-3} for coarse mode aerosol depending on scavenging rate. The authors should comment more on the assumptions made in the model and the implications of those assumptions.

[L180] *"Note that the current default CAM6.1 employs a narrow coarse-mode size distribution but a broad boundary width (high bound minus low bound), likely resulting in the GMD bounds less in effect, compared to that in CAM5".* – what are the impacts of changing the coarse mode width on sea-salt emissions and sea-salt AOD? Surely this change will impact more than dust alone, which may be confounding other results presented in the study (e.g., the DRE).

[L210] *"The wet size due to growth of aerosol particles by adsorbing water vapor follows the κ-Kohler theory with a time-invariant hygroscopicity for each aerosol species (Petters and Kreidenwei, 2007)".* – is it worth listing these hygroscopicity parameters to aid in the replicability of the simulations?

[L215] *"here and hereafter unless stated otherwise"* – this phrase, in parentheses, doesn't seem to apply to anything or make sense

[L224] This is another place in the text where the impact of asphericity on the MEE is tantalisingly hinted at without further detail as to whether its on and how its incorporated

[L276] "In addition, the meteorology field (horizontal wind, air temperature T, and relative humidity) was nudged" – the results will obviously be changed if the model is free running then. For instance, the coarse dust will absorb LW radiation, warming the surface and destabilising the atmosphere. Perhaps this assumption (fixed meteorology) should be discussed in the Discussion section

[L285] *"Therefore, we state the difference in the dust DRE and DRE efficiency estimate in Sect. 6, but do not document the comparison of dust loadings/deposition/DOD between the bulk and speciated dust simulations."* – Avoiding comparing BULK and MINE seems like a massive oversight and is one of the first things I'd query as a reader. Does speciation between minerals improve the simulation compared to assuming dust as a bulk quantity? Simply saying that as the dust properties are different (of course they will be), this reduces comparability, is a little bit absurd and a bit of a cop out. I think this comparison should be made in a follow-on paper. To be honest, it doesn't seem worth including the MINE simulations if they not appropriately analysed.

[L289] Choosing to tune some models to DOD = 0.03 but not others is very peculiar. The authors say *"Dust tuning was not applied to EXP03 and EXP04 (bulk dust simulations), in which the* *dust emission was identical to EXP02, in order to see how changes in the transported dust size distribution affects the DOD calculation".* – Well surely all of the individual sensitivity simulations (emissions, dry deposition, asphericity) would have benefitted from the same analysis? I guess that some parameters in the emissions and dry deposition algorithm need to tuned in some way (so using DOD might be a reasonable approach) as the parameters have a huge degree of uncertainty, but the asphericity probably did not need changing.

[L289] My other issue with this paragraph is that the tuning is not described in any detail. Which parameters were tuned and what are their values in the baseline simulation? How was tuning conducted and why was global-mean DOD chosen as the target? Simply saying 'tuned the model following Albani et al (2014)' is not sufficient, and it would be impossible to replicate these simulations without further detail

[Table 3] This table seems very large, and I'm not sure whether the list of acronyms should be at then end of the table or in the caption. Would it be better to have 1 table for each metric?

[Results] The difference between CAM6.1 and CAM6.α i.e., the control and the simulation with all changes added (except mineralogy) comes before the dissection of impacts of individual processes. Why is this? Surely it would be better to investigate the impacts of the individual processes and then use them to explain why CAM6. α a is different to CAM6.1?

[L378] *"CAM6.1 may overestimate the contribution of high-latitude dust emissions to the global dust total (8.0%)."* – is this referring to the dust burden? It's rather ambiguous as is

[L391] *"Overall, all models reproduced the climatology of DOD from AERONET retrievals, the surface concentration, and deposition within a factor of ten (Fig. 1 and Fig. S3)"* – this doesn't seem to be the case from looking at Fig. 1 b, c, e, f, h, and i. It seems that both models exhibit at least one measurement outside the range of 1/10x and 10x.

[Fig. 2] Why is the new dust emissions scheme smoother in terms of emissions, rather than the delta function (almost) in DEAD? I couldn't easily find this information in the text

[Fig. 3] Isn't the Ridley et al (2016) DOD dataset constrained by MODIS (either through assimilation or using it as a baseline? If so, aren't Figs 3a and 3b effectively showing the same results?

[L436] capture -> captures

[L437] Taklamakan (as in the desert) is spelt wrong throughout

[L440] *"Comparing with both datasets suggests that the new model may overestimate the regional DOD over North Africa and Middle East within a factor of two."* – this is especially odd considering the omission of dust mass for super-coarse particles from the model. Incidentally, would you expect the super-coarse dust to contribute much to 550nm DOD?

[Fig. 4] Great figure

[L498] S5i -> S5e

[Fig. 5] This plot, especially Fig. 5a, is very confusing. There are too many colours and it is difficult to pick out the CAM models. It may be worth plotting a non-CAM multi-model mean with max/min as shaded in grey, and then have just the CAM models in colour

[L542] Why is the size distribution for the fine dust fraction better captured by CAM6.α?

[L548] Sentence beginning *"Overall, CAM6.α better reproduced the size distribution"*. It would be worth adding the caveat here that the Otto et al and Ryder et al measurements are from single campaigns or flights and thus may not reflect the long-term mean dust properties at those altitudes, locations, and times

[L558] Section 4.2.1 – why are the mineralogy experiments used to test BRIFT vs DEAD rather than the BULK simulations? There doesn't appear to be any reasoning behind this

[L559] MIINE_NEW_EMIS -> MINE_NEW_EMIS

[L646] Paragraph on asphericity – I'm still confused even after reading the text as to whether the assumption of asphericity is applied to the dust MEE in every simulation run here or just the MINE_NEW_EMIS_SHAPE simulation?

[L683] *"(0.030-0.019)/0.030*100)"* – I don't think this formula needs to be written. See also L686 and L759

[L693] Paragraph beginning *"The lifetime of dust"*. Should this paragraph be in Section 4.2.4? It doesn't seem to mention asphericity or apply to the MINE_NEW_EMIS_SHAPE simulation

[L705] Why is MINE_NEW_EMIS referred to as the reference case? It's a sensitivity simulation, isn't it? Surely the only reference cases are CAM6.1 and possibly MINE_BASE?

[L733] *"NEW_EMIS_SIZE"* -> MINE_NEW_EMIS_SIZE. Also, this paragraph seems to be the only place where BULK and MINE are explicitly compared. I think the comparison should extend to all the dust metrics

[L798] *"Overall, replacing the size distribution of dust aerosol and the dust emission scheme with new ones (PZ10 and BRIFT, respectively)"* – replacing the size distribution is referred to here as PZ10 but this is the dry deposition scheme

[L821] The term "space volume" is ambiguous. Possibly "colocation in space"?

[L833] *"which can get mixed with dust aerosol particles during the transport and may not be completely excluded in the measurements."* This seems a little lazy, do you have any estimates of how much contamination leads to errors in measuring dust? At the moment, this point isn't backed up by evidence.

[L859] *"… followed by the enhanced dust mass extinction efficiency at the visible band by ~30% to account for the enhancement by dust asphericity"* – the asphericity applied to the MEE has not been shown to be the second most important change affected. Rather Fig. 10 shows that asphericity has a negligible impact on dust. Or is the asphericity in the MEE applied separately to the asphericity in the deposition rate? This is very confusing.

[L869] "Overall, the new model can:" – is the new model, referred to in this sentence, CAM6.α? If so, why has CAM6.α_MINE been neglected? The addition of MINE to this study makes little sense as it is peripheral. Additionally, is this "new model" already adopted for the next revision of CAM6 or is this the plan for the future?