

# ***Interactive comment on “Description and evaluation of aerosol in UKESM1 and HadGEM3-GC3.1 CMIP6 historical simulations” by Jane P. Mulcahy et al.***

## **Anonymous Referee #2**

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Aerosol modeling is a major challenge, in particular aerosol-cloud interactions. This work evaluates the performance of the GLOMAP-mode modal aerosol scheme when implemented in the GCM HadGEM3-GC3.1, and the larger ESM UKESM1 which is built on top of that GCM. The authors provide a comprehensive description of relevant differences between the two models. Using an ensemble of results from each model, they describe relevant differences in the aerosol simulations by each model when using GLOMAP, performing extensive comparison to observations. They also evaluate the effect of implementing a parameterization for marine primary organic aerosol emissions into UKESM1.

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The central question of this work is interesting – in particular the question of how much additional skill in aerosol representation is achieved by using a more comprehensive earth system model. The approach taken to address this question is to compare the results of ensemble simulations with GC3.1 to those with UKESM1. The authors’ conclusions – that the more complex UKESM1 does indeed produce a more accurate simulation of aerosol burdens, or at least does not produce a worse simulation - are mostly supported by their data, with some caveats.

The comparisons that the authors perform are pertinent and thorough, looking in detail at the quantities relevant to the evaluations in question. Their investigation of the effects of a new marine primary organic aerosol source, for example, is appropriate, and the observational sets used to evaluate the models are fitting. However, the paper suffers from two significant drawbacks.

Firstly, the analysis appears to mostly rely on comparison of completed ensembles, with few examples of dedicated simulations which isolate a specific process present in one model but not the other. This makes it difficult to evaluate the true contribution of individual differences between the UKESM1 and GC3.1 to the observed differences. This undermines the authors’ claims regarding the value of having a “traceable hierarchy”. For example, it is difficult to fully interpret differences in sulphate loading (or to evaluate the accuracy of the authors’ interpretations) because the two models use different approaches to distribute sulphur emissions. To truly take advantage of the “traceable hierarchy”, more sensitivity simulations – such as the one performed here for evaluation of the effect of the primary marine organic aerosol emissions parameterization – would be needed, and would have significantly improved the paper. Without them, it is difficult to say why exactly one model is more accurate than the other. This mostly constitutes a missed opportunity and a reduction in the paper’s utility.

Secondly, the comparisons made between the two models are mostly qualitative and subjective. In spite of an abundance of numerical output, the majority of comparisons made by the authors – both between the two models and against observations – are

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qualitative. This seems unnecessary, and I strongly recommend that the authors revisit their results and discussion sections to make them more quantitative. Differences are frequently said to be “clear”, “small”, “large”, “high”, “low”, and so on, with comparisons said to be “good” or “excellent”, when for all such cases a quantitative assessment should be straightforward. Statements about agreement are also often (albeit not always) vague and qualitative, such as: “BC and OM emissions are in good agreement with other models” (P19, L34); “Mineral dust emissions initially appear high in UKESM1 compared to other models” (P20, L10); “the low bias in the OM concentrations is clearly improved” (P37, L17); “the global dust burden. . . compares well with other models” (P20, L17); and so on. This last example is strange since it was immediately preceded by a statement that comparison is methodologically difficult. The last paragraph of page 20 is similarly problematic, claiming that “BC burdens compare extremely well” but then making no quantitative assessment. Similar statements such as that the model “compares remarkably well with the observations” (P42, L15-16) do not add to the discussion and seem more like assertions than scientific evaluations. Throughout the results and discussions sections, the numerous qualitative comparisons should be replaced with quantitative comparisons. This will enable the readers to evaluate the quality of the comparison for themselves, rather than relying on the authors’ opinion.

These issues are by no means fatal to the paper, and it is worth noting that there are still many quantitative comparisons made. If the authors can generally replace the more qualitative or subjective comparisons with quantitative ones, it will significantly improve the paper while also maximizing its scientific value. Additional sensitivity simulations to isolate the specific contributions of each individual difference between the two models would also help to elevate the paper.

Other major comments

Further to the broader need for quantitative comparisons, several comparisons are made to the AeroCom medians (e.g. P20 L21), but it would be more informative to also

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include the AeroCom range if possible.

How much of the difference in observed outcomes for sulphate can be explained by the different sulfur dioxide emission approaches? In particular, the lifetime of sulphate aerosol is ~20% longer in UKESM1 than in GC3.1. Could this be related?

P19, L8-10: The statement that Mann has a much shorter sulphate aerosol lifetime “due to a combination of lower burden and higher production rates” is backwards. This is just the definition of a shorter lifetime, and does not actually explain why the lifetime is shorter.

P2, L5: The wording “Finally, UKESM1 includes for the first time a representation of a primary marine organic aerosol source” is ambiguous. Previous global atmospheric models have included online estimates of primary marine organic aerosol emissions (e.g. Gantt et al 2015), so this novelty is for the UKESM1 system only. However, this wording is unclear, and makes it sound like UKESM1 is the first global model to include this source. The wording should be modified to make it clear that this is novel only for UKESM1, and not for geoscientific modeling generally.

P10, L16-26: The quantification and comparison to literature for BVOC emissions is insufficient, but seems like it could be easily improved. Please make this comparison quantitative, as “reasonably good agreement” is not a meaningful statement. Estimates of annual global emissions from the comparison studies should be provided. The total monoterpene emissions flux for GC3.1 should also be stated explicitly. Furthermore, it is not clear why isoprene emissions are not quantified.

While the focus of the model is on tropospheric aerosols, one of the advantages discussed of the UKESM1 model is that it incorporates a full stratospheric-tropospheric chemistry mechanism. I was therefore surprised to see no explicit description of how stratospheric aerosols are represented. Is the same GLOMAP scheme used throughout? If so, how are polar stratospheric clouds represented? If not, where and how does the aerosol representation in UKESM1 transition from GLOMAP-mode to some strato-

spheric scheme? Archibald et al (2019) states that PSC treatments were “recently expanded in UKCA”, but that “these improvements did not make it into the UKESM1 version of UKCA discussed here”. It would therefore be very useful to have an explicit (if brief) clarification of how stratospheric aerosols are handled, including any transition between schemes.

The authors state that the lack of (eg) nitrate aerosol is part of the reason for underestimation of aerosol mass by the model. However, no description is given in the paper of what components are included in each of the four aerosol “species”. An explicit description is needed as to what chemical species are included in each of the four. In particular, the presence or absence of ammonium and nitrate in the “sulphate” aerosol; and what composition is assumed for “sea salt”.

Section 2.4.1 states that sea salt emissions are calculated based on Gong (2003). However, this equation is non-linear with respect to surface wind speed, meaning that changes in horizontal resolution can change total emissions. Gong (2003) appears to be assessed using the same setup as Gong et al (2002), in which simulations were performed at a horizontal resolution of  $\sim 3.75$  degrees compared to the  $\sim 1$  degree resolution used here. Is any tuning applied to the sea salt emission calculation to account for this? This is particularly important given that marine POA is emitted proportional to sea salt (equation 3 on page 9).

“UKESM1 is believed to be one of the most comprehensively coupled models” (P42 L32) needs to be justified, or it is simply an assertion.

Minor comments, including formatting and typographic errors

P4, L1: ERF not defined on first use.

P7, L1-5: Are all 15 of the competition sub-steps performed together, once per 60-minute time step? Or are 1/3 performed after each 20-minute advection step? Please clarify.

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P8, Eq (1): This formatting is confusing and unconventional. I recommend the authors change this to a more conventional stacked format, with the DMS = a form on top and the DMS =b[log... form on the bottom.

P10, L1: Commas are incorrectly placed (one is needed after “monoterpenes” and should be removed after “iBVOC”)

P16, L9: “diagnositcs” should be “diagnostics”

P20, L18: It seems strange for a burden to be reported in “Tg/yr” – I assume this is a typo.

P32, L2: “simplier” such be “simpler”

P42, L25: “an underestimations” should be “underestimations” (or “an underestimation”, possibly)

## References

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