Geoscientific Model Development Discussions

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Reply on RC1

Jane P. Mulcahy et al.

Author comment on "UKESM1.1: development and evaluation of an updated configuration of the UK Earth System Model" by Jane P. Mulcahy et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-113-AC1, 2023

Response to Review by RC1, Dr. Andrew Gettleman.

We thank Dr. Gettleman for taking the time to review our manuscript and for his constructive comments on our paper. We respond to each point below, where the reviewer comments are in **bold** and our responses are in regular text with revised/new text from revised manuscript in *italics***.** Line numbers (denoted by **LXXX**) refer to Line Numbers in the revised manuscript accompanying this resubmission.

RC = Reviewer Comment

AR = Authors Response

RC: This manuscript is a comprehensive assessment of updates to the UKESM1, documenting a new version of a major Earth System Model. The manuscript is well written and should be acceptable for publication in Geoscientific Model Development with minor revisions. One general comment is that it's still unclear to me how you are ascribing sensitivity of the model to SO2 and SO4 partitioning, and the processes that are acting. You talk a lot about deposition, but very little about oxidation rates. See detailed comments below. This could be better clarified in the manuscript. Otherwise this is an excellent and comprehensive treatment of model evaluation.

AR: Thank you for your positive comments on our manuscript. We address your specific comment on SO2 and SO4 partitioning in your specific comments below.

Specific comments are below.

RC: Page 1, L12: note this is a reduction in magnitude of aerosol ERF (which is negative)

AR: We have reworded this sentence as follows:

L11:"*Changes to the aerosol and related cloud properties are a driver of the improved GMST simulation despite only a modest reduction in the magnitude of the negative aerosol*

ERF (which increases by +0.08 Wm-2)."

RC: Page 3, L59-63: This discussion is confusing. I read it 3 times and it still doesn't really make sense. Is the problem SO2 deposition or SO2 oxidation? It seems to be both, but you just say it's SO2 deposition I think. Please clarify.

AR: Thank you for highlighting the lack of clarity here. It is indeed the point that both deposition and oxidation processes are uncertain in the model. We hypothesise in the previous paragraph that too little removal of SO2 (via potentially both deposition and oxidation processes) close to source leads to excess transport of SO2 to remote regions where it is eventually oxidised to SO4, leading to a potential positive bias in remote SO4 aerosol. Given the remote marine location this has a large effect on the aerosol forcing. This work implements updates to the dry deposition only and we hope to investigate oxidation processes in the future, so the intention was just to highlight oxidation processes as another source of uncertainty here.

We have reworded this paragraph to make this more clear.

L55-64: "*Hardacre et al (2021) examine the impact of an updated parameterization for the dry deposition of SO2 on the surface SO2 concentration bias in UKESM1. The new parameterization considers whether the surface vegetation is wet or dry when calculating the surface resistance to species uptake. Due to the high solubility of SO2, the wetter and more humid at the surface the higher the uptake of SO2. The new parameterization leads to a significant improvement (of the order of 50%) in the positive SO2 bias against ground-based observations in the above study. Despite this improvement in the simulation of surface SO2, the reductions of SO2 close to source further degrade the pre-existing low bias in SO4 aerosol (Hardacre et al. 2021, Mulcahy et al. 2020) and so model process deficiencies in the oxidation of SO2 to SO4 also likely exist.Interestingly, Hardacre et al. (2021) show a larger relative reduction in surface SO2 and SO4 remote from source (e.g. over the North Atlantic region) than over the source regions supporting the above assertion that excess SO2 close to source regions drives remote aerosol loading and subsequent aerosol forcing."*

RC: Page 3, L70: where is section 3?

AR: Many thanks for highlighting this omission. Section 3 is now referenced in this paragraph.

L70: "….Section 3 details the model simulations conducted as part of this study."

RC: Page 3, L80: So is GA 7.1 also part of HadGEM3-GC3.1? The terminology is a bit confusing.

AR: Yes, GA7.1 is the Global Atmosphere component of GC3.1 and UKESM1. We apologise for the confusion. We have modified the text to clarify that both models have largely the same atmosphere:

L82: *"The physical atmosphere component (including aerosol) of UKESM1 (and GC3.1) is the Global Atmosphere 7.1 science configuration of the Unified Model ….."*

RC: Page 5, Table 1: How is DMSO + OH —> 0.6SO2 a balanced chemical reaction for S? Also, might note you have neglected things in these reactions (eg. DMS + OH —> SO2). Maybe better to have the whole reactions here?

AR: Thank you for your comment. Technically speaking the reaction should be DMSO+OH ->0.6SO2+0.4MSA (Pham et al. 1995). MSA is an inert tracer in UKESM1 and represents an effective sulfur sink. You are correct that the DMS chemistry (even with this new change) in the model is a gross over-simplification of the actual (highly complex) DMS chemistry taking place in the atmosphere. Many intermediary products are neglected (e.g. the potentially important role of halogens). This change seeks to make the DMS chemistry consistent with the physical model and reintroduces the important DMSO intermediary (Chen et al.,2018, Pham et al. 1995), which is a tracer in the UKCA-StratTrop model anyway and undergoes wet and dry deposition. We have corrected the reaction in Table 1 of the updated manuscript and highlighted the general simplicity of the scheme in the text:

L136: "*Currently, in the gas phase, DMS is oxidised by OH via an abstraction and addition pathway. The addition pathway, favoured at lower temperatures, neglects the formation of the intermediary product, dimethyl sulfoxide (DMSO), despite this being a transported tracer which undergoes wet and dry deposition in the StratTrop scheme. In UKESM1.1, the DMS chemistry is updated to include the formation of DMSO as shown in Table 1 and is now consistent with GC3.1. The DMS chemistry in UKESM1.1 remains a highly simplified scheme, Revell et al. (2019) investigated the impacts of more complex DMS chemistry on SO4 aerosol production and found a notable impact on cloud droplet number concentrations in the Southern Ocean."*

RC: Page 5, L141: does the SO4 go into a different mode?

AR: The bug led to sulphuric acid tendencies from the chemistry scheme not being updated on the shorter microphysical timesteps controlling nucleation, condensation and coagulation processes. This resulted in too high an initial H2SO4 concentration at the start of the chemistry timestep and subsequent excessive number concentration of small particles being produced by the nucleation process. The bugfix distributes this concentration correctly across the smaller substeps reducing the amount of H2SO4 initially available for nucleation, so nucleation mode number concentration decreases while there is a small increase in the condensation sink onto other modes. The bugfix and impacts of fix are documented in Ranjithkumar et al. 2021.

RC: Page 6, L152: maybe add a sentence on how these values were derived from AMIP runs? What was the methodology in brief?

AR: We have now added the following:

L158: *"Here, numerous AMIP simulations were conducted with the parameters of interest independently adjusted and outputs evaluated against observations. "*

RC: Page 7, L164: 'snow metric' is strange. Just call it the TOA outgoing clearsky SW flux over land…

AR: The terminology is consistent with what was used in Sellar et al. 2019, but we are happy to change it here if it is clearer:

L170: *"When evaluated in present-day simulations however, this tuning appears to lead to an excessive burial by snow and results in a net positive bias in the DJF top-of-atmosphere clear-sky outgoing SW radiation between 30N and 60N (see Table 2 Sellar et al. 2019)."*

RC: Page 7, L172: so the tuning darkens the present so it is warmer and does not change as much in the future?

AR: Yes, the positive albedo feedback is smaller.

RC: Page 7, L174: Capitalize Dust Optical Depth

AR: Now corrected (L182)

RC: Page 8, L199: What does it mean that 'tuning was omitted'? A parameter value came from somewhere. Is it that the protocol suggested that sub-grid gravity wave flux be adjusted to get the right period of the QBO? Or was this found after UKESM1 was released? Please be a bit more descriptive of the process.

AR: The *USSP_launch_factor* represents the generation of vertically propagating gravity waves by tropospheric convection and is sensitive to both model resolution and science configuration. It generally requires retuning for a change in model resolution and / or new science developments. By "omitted" we mean that when developing the N96ORCA1 configuration of GC3.1 (and subsequently UKESM1.0 at the same resolution) we neglected to tune this parameter and inherited the value from the higher-resolution N216ORC025 model. We have changed the text to make this clear:

L206: *The parameter (USSP_launch_factor) controlling the flux of sub-grid gravity waves generated by non-orographic sources is sensitive to both model resolution and science configuration and generally requires retuning when changing model resolution or implementing new science (Walters et al. 2014). This retuning was erroneously neglected during the development of UKESM1 which subsequently inherited the value of USSP_launch_factor used in the higher resolution physical model. As a consequence the period of the tropical quasi-biennial oscillation (QBO) was found to be too low in UKESM1 when compared against reanalyses (Richter et al., 2020)."*

RC: Page 8, L204: mean QBO period…..(also line 205)

AR: Now corrected (L214/215).

RC: Page 8, L211: What does the parameter do? I guess the tuning controls the LWP and the SW cloud radiative effect?

AR: The parameter, *two_d_fsd_factor*, describes the assumed sub-grid scale cloud water inhomogeneity. Larger values assume a greater sub-grid inhomogeneity of grid box mean cloud water and thus a less reflective cloud (vice versa for decreased values of this parameter). Development of the *two_d_fsd_factor* parameter is detailed in Hill et al. (2015). Due to this parameter being extremely poorly constrained by observations and its impact on TOA SW fluxes it is often used as a final tuning term for achieving a balanced TOA budget. We have amended the text as follows:

L221: *"Increasing the parameter value translates to a greater assumed sub-grid inhomogeneity of grid box mean cloud water and thus a less reflective cloud (and vice versa for decreased values of this parameter) although only a small retuning of this parameter - from a value of 1.48 to 1.49 - was required here."*

RC: Page 9, L229: what are the SSTs in the piClim-control

AR: SSTs and sea-ice in the piClim-control are taken from a 30 year period of the fully coupled piControl simulation (and represent the mean over the 30 year period). We have clarified this in the text:

L243: *"This configuration follows the Radiative Forcing Model Intercomparison Project (RFMIP, Pincus et al. 2016) protocol and takes simulated SST, sea-ice fields as well as the other climatological forcing fields described above from the piControl simulation. All other prescribed forcing data is also from 1850"*

RC: Page 9, L234: what is the second piClim-control-2014 experiment called?

AR: This experiment is called *piClim-anthro* – this has now been clarified in the text (**L246**).

RC: Page 20, L401: what is 'mean Nd' averaged over within the column? It's given as a concentration per unit mass, so it's not column. Averaged over cloud layers?

AR: For this analysis we diagnose the Nd at cloud top to enable a more accurate comparison with satellite retrievals. We have clarified this in the text , see **L367/368 and L425, Figure 9 Caption** while the Caption of **Figure 6** now includes the following sentence:

"Both cloud droplet number concentration and effective radius represent cloud-top values"

RC: Page 21, L435: Does the improvement when below 700m is included indicate that OHC is not partitioned at the right layers in UKESM?

AR: Yes, in the historical simulations the vertical distribution of the additional heat in the ocean is not in full agreement with observations. In the period after 1991, there is too much heat stored in the 0-700 m layer, and not enough in the layers deeper than 700m. A much more detailed analysis of the historical ocean heat content changes in UKESM1.0 is

given in Kuhlbrodt et al. (2022). We have adjusted the sentence to be more clear:

L447: *"This implies that in the ocean layers below 700m the uptake of heat is too small during this period, compensating for the overly strong increase above 700m."*

RC: Page 23, L465: what is the mechanism by which weaker aerosol forcing lowers AMOC? That does not seem trivial or obvious. Please explain how this is 'consistent'

AR: The mechanisms were proposed by Menary et al. (2013) based on a study using HadGEM2-ES, the predecessor model of UKESM1. Aerosol forcing induces atmospheric circulation changes over the North Atlantic and Arctic, which ultimately lead to an increase in the salinity in the North Atlantic, which decreases the stability of the water column, driving an increase in the overturning circulation. The increase in salinity seems to come from a combination of a decrease in ice transport through the Fram Straits, increased evaporation over the subpolar gyre and a positive ocean circulation feedback (stronger overturning brings more saline water northwards from the subtropics). Menary et al 2020 (see also Robson et al. 2020) have subsequently attributed the strengthening of the AMOC over the historical period (1850-1980) to the magnitude of the aerosol forcing across several CMIP6 models. They developed a metric for aerosol forcing of the AMOC that is proportional to the hemispheric gradient of net downward SW at TOA. With increased anthropogenic aerosol loading the northern hemisphere becomes increasingly more reflective than SHEM so net downward SW at TOA in the NHEM decreases while the SHEM stays largely unaffected. This energy imbalance seems (in models) to be balanced by a shift in the ITCZ or by a change in AMOC strength (Marshall et al. 2014). The strength of the AMOC is systematically weaker in UKESM1.1 (by a small amount, <10%) and this is consistent with a less negative (weaker) aerosol forcing. For completeness we have added the Menary et al. (2013) and Robson et al. (2020) references to the revised manuscript (see **L476**) and have also changed the language from "a weaker aerosol forcing" to "a less negative aerosol forcing" on **L478**.

RC: Page 25, L496: For the Antarctic sea ice you state there is no significant difference between UKESM1 and UKESM1.1. But is there an increase or decrease over time, or no change? And how does that compare to observations.

AR: Thank you for your comment. We have now included additional detail on the model performance in the Antarctic:

L510: *"Both models simulate a flat trend in both extent and volume up until the late 1970s after which the extent and volume decrease at similar rates in both models. Observations of sea ice extent from 1979 show a small positive trend which is not captured by the models."*

RC: Page 27, L527: does 'stronger' mean less negative? If so, awkward. It's actually a reduction in magnitude. Please clarify. Also the 'increases' is a decrease in magnitude right? (Less negative). Might be more clear to use magnitude.

AR: We apologise for the confusion in terminology. By 'stronger' we mean more negative and by 'weaker' we mean a less negative aerosol forcing. We agree this is confusing and so have adjusted the language to refer to "a less negative ERF" or "more positive" or "a more negative ERF" etc in this section (see **L538, 541, 542, 561, 562)**

RC: Page 27, L529: why is the aerosol effect positive over China and India?

AR: This has been documented in O'Connor et al (2021) and is due to the strong absorption by BC in UKESM1 resulting in regional positive forcings. O'Connor et al. (2021) show how this comes through the instantaneous radiative forcing as the SW and LW cloud adjustments were found to cancel (see also Johnson et al. 2019).

RC: Page 35, L658: do you want to comment on TCR being high as well? Also you might note that the cold temperature bias does not seem to be related to high ECS, since changing it did not alter ECS.

AR: Thank you for this suggestion. We have adjusted this sentence to state :

L684: "*While the ECS and TCR remain at the upper range of CMIP6 models….*"

We have also added the following sentence to the Discussion section of the similarity between feedback parameters of the two model at:

L666: *"Furthermore, the similarity in the effective climate sensitivity and transient climate response demonstrates that, in this model at least, the effective climate sensitivity does not seem to be related to the magnitude of the aerosol effective radiative forcing or the magnitude of the historical cold temperature bias."*

References:

Chen, Q., Sherwen, T., Evans, M., and Alexander, B.: DMS oxidation and sulfur aerosol formation in the marine troposphere: a focus on reactive halogen and multiphase chemistry, Atmos. Chem. Phys., 18, 13617–13637, https://doi.org/10.5194/acp-18-13617-2018, 2018.

Pham, M., Müller, J.-F., Brasseur, G. P., Granier, C., and Mégie, G.: A three-dimensional study of the tropospheric sulfur cycle, J. Geophys. Res., 100, 26061–26092, https://doi.org/10.1029/95jd02095, 1995.

Johnson, B. T., Haywood, J. M., and Hawcroft, M. K.: Are changes in atmospheric circulation important for black carbon aerosol impacts on clouds, precipitation, and radiation? J. Geophys. Res.-Atmos., 124, 7930–7950, https://doi.org/10.1029/2019JD030568, 2019.

Marshall, J., Donohoe, A., Ferreira, D., & McGee, D. (2014). The ocean's role in setting the mean position of the Inter-Tropical Convergence Zone. *Climate Dynamics*, **42**(7), 1967–1979. https://doi.org/10.1007/s00382-013-1767-z

Menary, M. B., Roberts, C. D., Palmer, M. D., Halloran, P. R., Jackson, L., Wood, R. A., Müller, W. A., Matei, D., & Lee, S.-K. (2013). Mechanisms of aerosol-forced AMOC variability in a state of the art climate model. *Journal of Geophysical Research: Oceans*, **118**, 2087–2096. https://doi.org/10.1002/jgrc.20178

Menary, M. B., Robson, J., Allan, R. P., Booth, B. B. B., Cassou, C., & Gastineau, G., et al. (2020). Aerosol-forced AMOC changes in CMIP6 historical simulations. *Geophysical*

Research Letters, 47, e2020GL088166. https://doi.org/10.1029/2020GL088166

O'Connor, F. M., Abraham, N. L., Dalvi, M., Folberth, G. A., Griffiths, P. T., Hardacre, C., Johnson, B. T., Kahana, R., Keeble, J., Kim, B., Morgenstern, O., Mulcahy, J. P., Richardson, M., Robertson, E., Seo, J., Shim, S., Teixeira, J. C., Turnock, S. T., Williams, J., Wiltshire, A. J., Woodward, S., and Zeng, G.: Assessment of pre-industrial to present-day anthropogenic climate forcing in UKESM1, *Atmos. Chem. Phys.*, 21, 1211–1243, https://doi.org/10.5194/acp-21-1211-2021, 2021.

Ranjithkumar, A., Gordon, H., Williamson, C., Rollins, A., Pringle, K., Kupc, A., Abraham, N. L., Brock, C., and Carslaw, K.: Constraints on global aerosol number concentration, SO_2 and condensation sink in UKESM1 using ATom measurements, *Atmos. Chem. Phys.*, 21, 4979–5014, https://doi.org/10.5194/acp-21-4979-2021, 2021

Revell, L. E., Kremser, S., Hartery, S., Harvey, M., Mulcahy, J. P., Williams, J., Morgenstern, O., McDonald, A. J., Varma, V., Bird, L., and Schuddeboom, A.: The sensitivity of Southern Ocean aerosols and cloud microphysics to sea spray and sulfate aerosol production in the HadGEM3-GA7.1 chemistry–climate model, *Atmos. Chem. Phys.*, 19, 15447–15466, https://doi.org/10.5194/acp-19-15447-2019, 2019.

Robson, J., Aksenov, Y., Bracegirdle, T. J., Dimdore-Miles, O., Griffiths, P. T., & Grosvenor, D. P., et al. (2020). The evaluation of the North Atlantic climate system in UKESM1 historical simulations for CMIP6. *Journal of Advances in Modeling Earth Systems*, 12, e2020MS002126. https://doi.org/10.1029/2020MS002126

Kuhlbrodt, T., Voldoire, A., Palmer, M. D., Geoffroy, O., and Killick, R. E. (2022). Historical Ocean Heat Uptake in Two Pairs of CMIP6 Models: Global and Regional Perspectives. *Journal of Climate*, https://doi.org/10.1175/JCLI-D-22-0468.1