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Reply on RC2

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-AC2, 2023

Response to Review by RC2

We thank Reviewer RC2 for their helpful and constructive comments on our manuscript. We respond to each point below, where the reviewer comments are in **black** and our responses are in regular text with updated/new text in the revised manuscript denoted 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 is an important manuscript that describes the changes made between UKESM1 and UKESM1.1 in order to improve the simulation of the historical surface temperature in the second half of the 20th century. A number of changes and bug fixes were made, but the key change appears to be a reduction in the magnitude of the aerosol ERF as a result of a reduction is sulphate.

The discussion focuses on one specific model but the problem of an overly cold late 20th century is present in other climate models as well. Therefore the manuscript should be relevant to a broad audience and well suited for GMD.

While I believe that the conclusions are very likely correct, thie analysis does not currently provide sufficient evidence to support them. The reported change in ERF is small difficult to attribute to aerosol only (see major comment below).

I recommend performing additional simulations (time evolving ERF calculations) to better support the conclusion that the primary reason for the improvement in surface temperature is due to a reduction in the aerosol forcing.

AR: We thank the reviewer for making some highly relevant points in their review and hope we have satisfactorily address the Major Comments below.

RC: Major comment

The magnitude of the change in total ERF (+0.08 W/m2) appears small compared to the actual change in surface temperature (Fig 3a).

Using a simple back-of-the-envelope calculation (see Shindell 2014, doi:10.1038/nclimate2136), we can estimate the warming for a given forcing and TCR as:

dT = TCR/F2xCO2 ERF_tot

where TCR is the transient climate response, F2xCO2 the 2xCO2 forcing, ERF_tot the total anthropogenic forcing. Let's assume F2xCO2 = 3.6 W/m2 for both UKESM1 and 1.1 (based on Figure 18c) and estimate dT for both models:

dT_UKESM1 = 2.76 / 3.60 * 1.76 = 1.35 K

dt_UKESM1.1 = 2.64 / 3.60 * 1.84 = 1.35 K

Based on that simple calculation, both models would yield about the same level of warming due to a compensation between an increase in total forcing and a reduction in TCR. While that is not the case, it does make it difficult to simply conclude the all the changes arise from ERF while the TCR remains "essentially unchanged" (line 14).

Another way to look at this to calculate how large a temperature change one might expect given the change in ERF (0.08 W/m2):

dT = 2.64 / 3.60 * 0.08 = 0.06 K

This value is very small compared the actual temperature difference between the models (Fig 3a).

The most likely explanation for this discrepancy is that the difference in ERF is much larger during the period 1960-1990 than the value of 0.08 W/m2 reported for 2014.

Similarly, the forcing values presented in Table 3 are not very convincing. The total anthropogenic forcing is indeed larger in UKESM1.1 (+1.84 W/m2) than UKESM1 (+1.76 W/m2). However, summing the components yields a smaller forcing for UKESM1.1 (+1.61 W/m2) than UKESM1 (+1.65 W/m2), and both of them are off by more than the difference between models. I don't think this data supports the conclusion that the change is aerosol forcing is key. Having comparable values for the period 1960-1990 would likely help.

I would recommend to perform additional simulations to estimate ERF for different periods more relevant to the cold bias. The best would be to follow RFMIP experiments for diagnosing time-evolving ERF. Due to the need for additional simulations, I recommend that the manuscript be returned for major revisions.

Author Response (AR): We thank the reviewer for their comments and agree that the change in the aerosol ERF in UKESM1.1 relative to UKESM1 appears modest (at +0.08Wm-2) and the reviewer rightly questions how this small net change in the aerosol ERF can be the main driver of the improved historical surface temperature evolution in UKESM1.1. Following the reviewer's recommendation, we have now conducted a series of additional aerosol ERF timeslice experiments at different time points during the historical period where we perturb the anthropogenic aerosol emissions to 1900, 1920, 1950 and 1980 conditions respectively, in addition to the 2014 timeslice already conducted. Each timeslice is run for 45 years with the aerosol ERF calculated for the final 30 years as outlined in the manuscript. The aerosol ERF timeseries (for net all-sky but also clear-sky fluxes at the top-of-atmosphere) is shown below (and is included in the manuscript as new Figure 21) and has also been now added to the main text. It shows that post 1920 when anthropogenic SO2 emissions start to rapidly increase the aerosol ERF in UKESM1.1 is consistently less negative in magnitude, with the *change* in aerosol ERF increasing from +0.01Wm-2 in 1920 to a maximum difference of +0.2Wm-2 in 1980. Hence, we agree with the reviewer that the difference in aerosol ERF is bigger between the models at this crucial time-period than was indicated by simply considering the change in ERF between pre-industrial and a 2014 timeslice. We thank the reviewer for the suggestion to investigate this further.

The change in aerosol ERF is predominantly coming from the Northern Hemisphere which shows larger changes in aerosol ERF (changing by +0.1Wm-2 in 1920 to +0.31Wm-2 in 1980) than the Southern Hemisphere. In addition to the aerosol ERF timeseries figure we now also include a plot of the interhemispheric difference in the aerosol ERF for each timeslice (new Fig 21b and shown also in the attached supplement to this response)**.** The smaller interhemispheric gradient in the forcing for all years assessed points to differences in the spatial inhomogeneity of the aerosol forcing in UKESM1.1 which could lead to changes in the transient sensitivity of the aerosol forcing. The role of inhomogeneous forcing is highlighted as important and incorporated in Shindell et al (2014) through their ratio, E, where the calculation of dT above becomes dT=(TCR/F_2xCO2) x (Fghg+E*(Faer+Fo3+Flu)).

In addition, we believe the different PI background states of UKESM1 and UKESM1.1 is of importance for both the aerosol ERF and also the historical temperature response to forcing. The UKESM1.1 PI climate in the *piClim-Control* simulation is more positive than UKESM1 by +0.56Wm-2. The change is driven by a combination of the large reduction in the outgoing SW (-1.57Wm-2) being offset by an increase in OLR (+0.97 Wm-2). Natural marine sources of DMS dominate the PI distributions of SO2 and subsequent sulphate aerosol. Therefore, changes to the DMS chemistry, SO2 dry deposition parameterization and the bugfixes to the H2SO4 updating and vertical profile of cloud droplet number concentration calculation all alter the PI background state. These changes result in a notable less negative SW cloud forcing in UKESM1.1 PI climate (-43.80 Wm-2 versus -45.02Wm-2). This change in PI climate translates to the fully coupled historical simulations driving a more positive net TOA radiation and subsequent warmer temperatures. We have additionally now included the historical timeseries of the absolute values of the net TOA radiation and its components as well as the SW cloud forcing and cloud fraction to highlight the different base climates more clearly in these two configurations.

In summary, we believe the dT calculated by the reviewer above does not take into account the spatial inhomogeneity of the aerosol forcing and how that may change between UKESM1 and UKESM1.1. Also, it is calculated for 2014 relative to 1850, while the period where the largest change in dT occurs (as the reviewer points out) is closer to 1980 minus 1900. During this period, the anthropogenic aerosol forcing will play a relatively larger role than the homogeneous forcing from GHGs and so will have a stronger relative impact on surface temperature. We hypothesize that the sustained less negative aerosol forcing over the historical period imposed on a warmer PI background climate is an important factor in the improved simulation of historical surface temperature in UKESM1.1. We thank the reviewer for prompting us to carry out this additional analysis which improves the paper and our conclusions. A full transient aerosol ERF simulation is planned for future work and for this manuscript we have modified the text and conclusions in the manuscript to reflect this additional analysis **as follows:**

L535 (start of Sect 4.3): *"As noted earlier, the warmer climate in UKESM1.1 and improved representation of the historical surface temperature is believed to be largely due to a weaker anthropogenic aerosol forcing driven predominantly by lower sulphur dioxide and hence sulphate aerosol burdens in the updated configuration"* is changed to : *" We now compare the key effective radiative forcings between UKESM1.1 and UKESM1 and examine their potential role in the improved simulation of historical surface temperature."*

L537 now states: *"The total anthropogenic ERF is more positive in UKESM1.1 increasing from 1.76Wm-2 to 1.84Wm-2. This is in part due to a less negative aerosol ERF……"*

We have made significant changes to the Discussion and Conclusions section with the main change being the inclusion of a new paragraph and a new figure (Figure 21) of the aerosol ERF timeseries and interhemispheric difference in the aerosol ERF.

L648: *"The relatively modest change in global-mean aerosol ERF between UKESM1 and UKESM1.1, calculated for 2014 anthropogenic aerosol conditions relative to pre-industrial, is unlikely to be the sole explanation of the improved historical temperature response. To get a better indication of how the aerosol ERF changed throughout the historical period we have conducted additional aerosol ERF simulations (piClim-aer) for 1900, 1920, 1950 and 1980, in addition to the 2014 simulation. The resulting aerosol ERF timeseries (Figure 21) shows that post 1920 when anthropogenic SO2 emissions start to rapidly increase the aerosol ERF in UKESM1.1 is consistently less negative in magnitude than UKESM1, with the change in aerosol ERF increasing from +0.01Wm-2 in 1920 to a maximum difference of +0.2Wm-2 in 1980. This change in aerosol ERF is predominantly coming from the Northern Hemisphere which shows larger changes in aerosol ERF (changing by +0.1Wm-2 in 1920 to +0.31Wm-2 in 1980) than the Southern Hemisphere leading to a weaker interhemispheric gradient in the aerosol ERF in UKESM1.1 (Figure 21). This change in the regional pattern of aerosol ERF between UKESM1 and UKESM1.1 could imply that the transient sensitivity to aerosol forcing has changed in UKESM1.1 (Shindell, 2014). The dependence of the transient sensitivity on the forcing is often described as an efficacy (Hansen et al., 2005), and in CMIP6 models the radiative feedbacks in response to aerosol forcing have been found to more amplifying (transient sensitivity higher) than that to greenhouse gas forcing (Salvi et al., 2022). We hypothesize that the less negative aerosol forcing over the historical period imposed on a warmer background climate state, which has a less negative SW cloud forcing, is an important factor in the improved simulation of historical surface temperature in UKESM1.1. Our comparison of the effective climate sensitivity and TCR in UKESM1.1 and UKESM1 from the 4xCO2 and 1%CO2 runs shows the long term-response to CO2 is similar between model configurations but is unable to test for a change in transient sensitivity to aerosol forcing. This would require dedicated historical aerosol-only simulations, which is planned in future work."*

RC (copied again from above): Similarly, the forcing values presented in Table 3 are not very convincing. The total anthropogenic forcing is indeed larger in UKESM1.1 (+1.84 W/m2) than UKESM1 (+1.76 W/m2). However, summing the components yields a smaller forcing for UKESM1.1 (+1.61 W/m2) than UKESM1 (+1.65 W/m2), and both of them are off by more than the difference between models. I don't think this data supports the conclusion that the change is aerosol forcing is key. Having comparable values for the period 1960-1990 would likely help.

AR: The component ERFs making up the total anthropogenic ERF in the fully coupled UKESM do not add up linearly as has been shown in a detailed assessment by O'Connor et al, 2021. This is due to the non-linearity in aerosol-cloud interactions but also aerosol-chemistry interactions (O'Connor et al 2021, O'Connor et al 2022). Changes to ozone precursors and CH4 have been shown to lead to an additional indirect aerosol forcing that would not be captured in an aerosol only ERF experiment set-up. An in-depth study of all the components of the total anthropogenic forcing is outside the scope of this model documentation paper, we acknowledge however that this would be an interesting study to make in the future. In its absence for this manuscript, we have altered the text to as outlined above in our more general response to the reviewers Major Comment and have conducted additional aerosol ERF simulations which support the role of the less negative aerosol ERF in the more positive total anthropogenic forcing and the subsequent improved historical aerosol performance.

Minor comments:

RC: Lines 122-127: paragraph requires clarification. If I understand correctly, r_c was set to 10 sm-1 in GC3.1, then mistakenly to 148.9 sm-1 in UKESM1 and then to 1 sm-1 in UKESM1.1. Clarify the motivation for using 1 sm-1 instead of 10 as in GC3.1? Insufficient SO2 dry deposition?

AR: The motivation comes from the literature already cited in the text (Garland,1978; Erisman et al. 1994; Zhang et al., 2003, Hardacre et al. 2021). SO2 is a highly soluble species and as such readily dissolves in water, supporting a low resistance value. Different ranges from 0.004 to 20 s m-1 appear in the literature, so 10 sm-1 as such isn't an incorrect value but lower values are also acceptable and further increase the SO2 dry deposition in the model. We have modified the text to make this clearer:

L127: *"Numerous studies (eg: Garland (1978), Erisman et al. (1994), Zhang et al. (2003)) indicate the resistance to SO2 deposition over the open ocean is minimal with reported resistance values ranging from 0.004 to 20.0 s m-1."*

RC: Lines 207-211: What is the impact on net TOA radiation?

AR: This is shown in the top panel of Figure 1 (P11) where the net TOA of the final tuned piControl is approximately 0.04Wm-2.

RC: Line 271: any reason for stopping at 462 years and not the recommended 500 years for DECK piControl?

AR: No. UKESM1.1 is well spun-up having been initialised after 111 years of the UKESM1 piControl plus running an additional 613 years in the model development cycle and another 70 years of the final frozen UKESM1.1 configuration. We ran the UKESM1.1 piControl long enough to cover our historical ensemble but felt it was unnecessary to run longer as these were not designed to be official CMIP simulations.

RC: Lines 275-276: "later period". Chosen because of the smaller drift or for other reason?

AR: The UKESM1.1 piControl was initialised from a later period in the UKESM1 piControl (see previous answer) than that used in Sellar et al. (2019). We use this later period in our analysis of the two models.

RC: Figure 3: HadCRUT5 reports SST over ice-free ocean, and surface air temperature over land and ice covered ocean. Was the same calculation done for the model output?

AR: We have used 'tas' or temperature at 1.5m everywhere in our calculation. Following the reviewers suggestion we have checked the impact of using just 'tas' versus a blended dataset of 'tas' and SST but find minimal difference (see plots compared in the attached supplement). We did discover in the process that in the original submission the 6 UKESM1 historical ensemble members used in Figure 3 were inconsistent with the 6 used elsewhere in the paper. This has now been corrected in the revised manuscript and so while the UKESM1 values are slightly altered to previous it does not impact on the paper's findings or conclusions.

RC: Line 319: 1900 □ 1901 for consistency with the figures. Similar on line 324.

AR: Now corrected (**L329-L350**)

RC: Lines 355-356: explain how globally averaged Nd and r_eff were calculated.

AR: These are 2d fields diagnosed at cloud-top output by the model. We have now 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: Figure 6: how different are the starting values?

AR: We have now included an additional figure in the Supplementary Information (Figures S12) showing the historical timeseries of the absolute values of these aerosol and cloud variables. These figures show how the AOD is systematically higher in UKESM1.1 while the Nd is systematically lower. Differences for both variables are between 10-20%. We have referred to this figure **on L368** in the revised manuscript**.**

RC: Lines 365-372: are Nd anomalies really relevant if the clear-sky OSW anomalies are driving the surface temperature change?

AR: We contend that it is still relevant to show that the Nd response to historical forcing is smaller than in UKESM1 as it is the very different Nd climate in the PI that prevents a weaker ACI during the historical period, but it demonstrates that the model developments implemented in UKESM1.1 reduce the Nd response to anthropogenic forcings.

RC: Figure 9: explain how vertically averaged Nd was calculated.

AR: We have not plotted the vertically averaged Nd in this figure, it is Nd diagnosed at cloud-top. This is more comparable to the satellite derived quantity. We have made this now clear in the Figure 9 caption in the revised manuscript.

RC: Line 418: is the detrending actually needed? piControl looks stable in Figure 1b.

AR: Whether we can call the piControl "stable" depends on the quantity considered. For the ocean component, Figure 2 shows that both UKESM1.0 and UKESM1.1 display substantial centennial, internal variability. By applying detrending we have chosen to subtract most of this internal variability from the analysis of the historical simulations. Of course, other choices could have been made here. With our choice we ensure comparability with historical simulations from other models, as published in the literature.

RC: Line 425:"relatively large climate sensitivity" here, and "outside of the CMIP6 5-95% ranges"on line 552.

AR: This comment isn't very clear but we assume the reviewer is saying the 2 phrases are perhaps inconsistent. For clarity we have rephrased the former from "relatively large" to "high" **(L439).**

RC: Table 4: use a consistent number of decimals and verify that the net adds up, or explain why.

AR: Thank you for bringing this to our attention. We have now corrected the Table 4 to have a consistent number of decimal places. We also realised there was a typo in the "ACI cloud absorption" term for UKESM1, this should read -0.003 and not +0.003 and is now corrected. The components now add up to the net value.

RC: Line 602: "thesimulated" □ "the simulated"

AR: Now corrected **(L616)**

RC: Lines 610-611: Table 3 currently doesn't support this assertion. See major comment above.

AR: We have now rephrased this sentence and paragraph, see response to Major Comment above.

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