

Interactive comment on “The Met Office Unified Model Global Atmosphere 7.0/7.1 and JULES Global Land 7.0 configurations” by David Walters et al.

David Walters et al.

david.walters@metoffice.gov.uk

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1 Response to general comments

We thank the referee for reviewing our manuscript and for their insightful comments. The most significant point raised is the lack of detail in the description of GA7.1. The authors fully recognise the importance of these developments, and for this reason, a detailed description of the rationale behind these is being documented in full in an additional paper (already referenced in the paper under review as Mulcahy et al. (in prep.b) – hereafter Mulcahy et al.). Mulcahy et al. will address comments 2(a-c), documenting

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more fully the aerosol ERFs of both GA7.0 and GA7.1. It will also document in full the relative importance of each change in terms of impact on ERF (addressing Comments 2(b and c)). The authors feel that to address these issues here would go beyond the scope of this paper and would significantly detract from the importance of that highly relevant work.

The authors propose to raise the awareness of the Mulcahy et al. paper at the start Sect. 5 by including the following sentence: “... whilst maintaining a present-day simulation similar to that from GA7.0. The aerosol ERF and subsequent development of the GA7.1 configuration as well as the scientific justification for those developments are fully documented in Mulcahy et al. (in prep.b).” In response to comment 9, however, this does mean that we will need to request that the final publication of this paper is delayed until a doi is available for Mulcahy et al.

We hope that this addresses the main concerns of the referee; we address the individual points raised in more detail below.

2 Responses to specific questions

1. “Given that GA7.1 will be be the UK contribution to CMIP6, ideally GA7.1 would have been the model described ... it would have been better to have seen the GA7.1 results consistently through the paper.”

Whilst GA7.1 will be the GA configuration used in the UK submissions to CMIP6, we try to make clear in the paper that this is still seen as a “branch” to the GA “trunk” and that the latest release of that trunk is still GA7.0. Aside from CMIP6, GA7.0 will still be used as the basis for a number of applications including operational NWP and the GloSea seasonal forecasting system. Furthermore, GA7.0 and not GA7.1 forms the baseline for GA8 development and the eventual documentation of a GA8.0 configuration will be made by comparison with GA7.0.

2. (a) *“The paper reports ERFs for GA7.0 but not GA7.1.”*

As discussed in the response to general comments, this is out of scope of the current publication, but will be documented in full in Mulcahy et al.

(b) *“Provide references to justify the 70% increase in marine DMS emissions”*

The increase in marine DMS emissions is a first attempt to represent a marine organic aerosol source in the model, guided by the observations in McCoy et al. (2015) and work of O’Dowd et al. (2004). Again, this work and the motivation for this change is documented in detail in Mulcahy et al.

(c) *“Provide some indication of the relative importance of the changes designed to reduce the magnitude of the aerosol ERF”*

As discussed above, a quantitative response to this will be included in Mulcahy et al. However, for a more qualitative response, we are also happy to list the changes in Sect. 5 in order of importance. The list will therefore read:

1. Liu cloud droplet spectral dispersion
 2. Aerosol absorption update
 3. RADAER lookup changes
 4. Scaling of DMS
 5. UKCA Activate
 6. Lana
 7. Retuning
3. *“Even if the changes discussed on p. 60, which can be viewed as ERF tunings, are empirically and physically justified, an important point emerges. . . . To this reviewer, this should impel research to understand more robustly the magnitude of the aerosol ERF.”*

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Again, we agree with the referee's comments that this highlights an important point of general interest to the climate modelling community. Further research is required to more robustly understand the magnitude of the aerosol ERF across global climate models. However, once again, we feel these discussions go well beyond the scope of the current paper. The implications of the need to develop a GA7.1 configuration will be discussed in Mulcahy et al. and the larger scientific questions raised as a result of this research are already the focus of further investigations with the UKESM and HadGEM3-GC3.1 models.

4. *“Some parameterizations seem designed primarily to reduce model bias with structural designs difficult to justify otherwise. The particular example here is the parameterization for cirrus spreading (p. 23).”*

Whilst this does sometimes occur in the development of new parametrisations, it is something that we try hard to avoid, to be clear about when this has been done and to address in the longer term by replacing these with more physically motivated approaches.

The cirrus spreading term highlighted by the reviewer is a good example of this. It was first introduced in GA4.0 as a late tuning, with an acknowledgement from the developers that this was poorly motivated. Because of this, when the package of cloud, radiation and microphysics changes for GA7.0 was first proposed, the parametrisation developers proposed removing this altogether by setting the spreading rate to a vanishingly small value. In the later development of more complete GA7.0 test configurations, comparisons with observations showed that in general this was a good thing, but that reducing it to the extent originally proposed pushed the biases too far. For this reason, we have had to leave this in the final configuration, albeit with a significantly reduced rate compared to GA6.0, but have acknowledged in the documentation that this is a tuning and that there is still a desire to eventually remove this term altogether.

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5. *“Figs. 7, 13, 14, 15, 16, 17, 18, 24, and 25: Include summary statistics”*

We will consider this ahead of producing plots for the final publication.

6. *“Indicate the number of moments in the microphysics parameterization.”*

Our current microphysics scheme is single moment, holding prognostic variables for the mass of a number of independent species (i.e. cloud liquid water, cloud ice water and liquid rain). We have altered the text to make this explicit in the description in Sect. 2.4.

7. *“Nudging toward climatology presumably does not take place with interactive vegetation. This is somewhat unclear.”*

Agreed. The lack of clarity on this point was also picked up by the first reviewer in reviewer comments RC1. We have added a comment to clarify this.

8. *“Quantify the magnitude of the radiation improvements.”*

Some evidence for this will come through the statistics added in response to Question 5. In line with our response to Question 1, however, we would like to reiterate that simulations presented here are designed to be indicative of the performance of the configuration in general and not to represent its final performance in any particular target system or application. For this reason, we believe that a detailed quantitative discussion of the magnitude of improvements in one particular simulation is beyond the scope of this paper; instead this is for discussion in subsequent papers describing particular applications.

9. *“Numerous important references are described as in preparation.”*

We agree that a final resubmission should not contain “in prep” references. The most important of these, which is needed to address questions 1–3 of this review,

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is Mulcahy et. al (in prep.b). Good progress is being made on that paper, but we will make a request to the editor of this paper that its publication is held back until a doi is available for Mulcahy et al.

10. *“N50 concentrations are described as in reasonable agreement with modeled results. . .”*

The word “reasonable” was used here because whilst we agree that it would be an overstatement to say that this “agrees well” or “is in good agreement”, we do believe that the agreement is reasonable given the current expectations for physical climate models and the limitations of both our modelling systems and the observational datasets. In the paper, we highlight good performance in the clean air regions, but acknowledge the underestimation in the polluted regions of North America, Europe and Asia. The observations used in this comparison have their limitations, as these are from specific campaigns at particular points in time, compared to a free-running 20 year model climatology. In many instances, these campaigns deliberately track the polluted plumes and so the observations are likely to be weighted towards highly polluted events (e.g. see Reddington et al., 2017; Schutgens et al., 2017). These papers highlight the inherent uncertainty in model-to-obs comparisons due to spatio-temporal sampling and note that this can lead to an overestimation of high pollution episodes and underestimation of clean episodes in the observations.

We propose adding the following sentence to the paper to address this: *“It is a point of ongoing discussion whether the current approach of targeting high-pollution events with observational campaigns leads to biases in comparisons of models to observations for these events (Reddington et al., 2017; Schutgens et al., 2017).”*

11. *“Tunings to improve the coupled simulation are described. How much do these tunings change the global, annual-mean top-of-atmosphere OLR, SW, and net*

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radiative fluxes if imposed in the uncoupled model?"

As discussed in Sect. 3.11, the most significant tuning aimed at improving the simulation in the coupled model was the tuning of the `r_det` parameter. This was primarily motivated by its impact on local SSTs rather than any global mean parameters, so the impact on global OLR, SW and net radiative fluxes in the uncoupled simulations was less than 0.1 Wm^{-2} .

12. Additional suggested corrections:

We agree with all 5 of these suggestions and have updated the manuscript appropriately.

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