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Geoscientific Model Development Discussions

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.

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1 Response to general comments

We would like to thank the reviewer for their thorough and complementary review. The list of minor comments is appreciated as addressing these has led to improvements to the paper. We respond to these in turn below.

2 Responses to specific questions

1. *"The thing that bugged me most in this paper was that the model timestep and target horizontal resolutions were never described . . . "*

   Because the GA configuration is designed for use across a range of horizontal resolutions, we do not really have the concept of a "target resolution". Instead, we expect the documentation for each system using a GA configuration to specify their own target resolutions. The fact that the time step information is only included as supplementary information, however, is an oversight; we've added this in a table in Sect. 2.2.

2. *"p2 line 1-3: citations in this sentence seem like they're for v7 of the model . . . "*

   The Unified Model is a very general code base that can be used is a large number of ways; the Global Atmosphere configuration described in the paper is a single science configuration of this model. The citations in these lines are for general descriptions of the models, rather than the configurations described in this paper. We have tried a number of different ways of citing the general model descriptions and the structure used in lines 1–3 is the best we can come up with.

3. *"section 2.1: I'm curious why UKMO continues to use a lat/lon grid instead of a cubed-sphere grid. Also, are polar filters used?"*

   We originally chose a lat/lon grid for the UM (rather than taking, say, a spectral approach) because this allowed us to use the same code base for a global models and limited area models. The move to semi-Lagrangian advection in the early 2000s meant that the cost of these grids was not too great and a more recent upgrade to use the ENDGame dynamical core has improved the scalability to be suitable for current HPC architectures. With the move to increased parallelism, the UM will suffer from scalability issues due to the incredibly fine resolution as

one approaches the pole and it has been known for some time that this approach is not suitable for Exascale HPCs. The next-generation model (due to be operational in the mid-2020s) will run on a cubed-sphere grid, but with the aim of the majority of code being agnostic to the grid, which will make it easier to swap to alternative grid structures later on, if required.

Since the introduction of ENDGame in GA6, the GA configurations of the UM have run without using a polar filter. Whilst this improves the model's scalability, it is not without problems as it does expose the noise at the pole discussed on p16 of the paper and alleviated by ticket #135. We have not ruled out the reintroduction of some very limited polar filtering in future releases, although we do not want to do this without first understanding the source of the noise in the wind field.

4. *"I'm curious about model performance. How many cores do you typically use and what is your typical throughput on the machine you use most?"*

   As the reviewer suggests, we do see this as out of scope of the paper. However, here are some very rough numbers for completeness. An N96 atmosphere/land-only climate simulation run on about 450 cores of our Cray XC40 HPC completes just over 3 years of simulation in 1 day (ignoring any queuing time between monthly resubmissions). At the other end of the spectrum, an operational N1280 NWP simulation uses 530 nodes ($\approx$19,000 cores) and completes a 7+ day forecast in 40-45 minutes. In research mode, we tend to run on fewer nodes, which increases runtime, but allows us to run a larger number of simulations in parallel.

5. *"section 2.2: Using parallel splitting creates opportunities for water and aerosol species to go negative. . . "*

   Although this is possible in principle for moist species, there are checks within the code to ensure not only positivity, but internal consistency between differ-

ent moist/cloud prognostics. No aerosol processing takes place with the parallel physics.

6. *"page 4 line 13: "When using the prognostic aerosol scheme, this is included. . ." is awkward*"

   Agreed. We have made the change suggested.

7. *"page 7 line 12: "We also make use of. . ." sentence is awkward."*

   Agreed. We have restructured the sentence and split it in two.

8. *"It seems odd to talk about Large-scale precipitation (sect 2.4) before large-scale cloud (sect 2.5)"*

   Yes, we can see the reviewer's point. However, the ordering of physics schemes in Sect. 2 is based on their order in the UM time step. In the UM, the LSP scheme is run from the start of the time step, whilst the PC2 cloud scheme is distributed through the time step, so we have decided to include this afterwards.

9. *". . . Wouldn't it be better to just reserve a subcolumn for convection?"*

   Yes, we could allocate one column for convective cloud amount. In the current scheme, this does implicitly set the convective cloud fraction as 1/(number of sub-columns), so we would need some sensible way of working out its weight. Related to this, but of equal priority in the current scheme, is further investigation of how best to determine the amount of convective cloud water.

10. *"section 2.8: if your PBL scheme is really a turbulence scheme, why not call the section and the parameterization "turbulence scheme" instead of PBL scheme?"*

    Most of this turbulence diagnosed by the scheme is in the boundary layer, so this is still its main role. In addition, there is also plenty of turbulence in and

around cumulus clouds which this scheme does not handle and so we find this a helpful distinction. Finally, we want the paper to be consistent with our internal documentation and code naming, in which this is labelled the BL scheme.

11. *"p. 9 lines 16-22: I found this explanation to be unclear."*

    We have updated the text to make this clearer: *"The existence and depth of unstable layers is diagnosed initially by two moist adiabatic parcels, one released from the surface, the other from cloud-top. The top of the K profile for surface sources and the base of that for cloud-top sources are then adjusted to ensure that, from the resultant buoyancy flux, the magnitude of the buoyancy consumption of turbulence kinetic energy is limited to a specified fraction of buoyancy production, integrated across the boundary layer."*

12. *"p. 9, line 28: . . . is stable stability redundant?"*

    By stable stability dependence, we mean the stability function used in stable boundary layers. We have altered the text to make this clearer.

13. *"what is the "sharp" function?"*

    We have added a reference in which this is described.

14. *"p. 10, line 9: "surface layer is conditionally unstable"?"*

    By unstable we mean where the surface buoyancy flux is positive and so have now written that instead.

15. *"p. 11, lines 28-end: I don't understand how you use the canopy model to simulate lake and urban surfaces."*

    Here, we take advantage of the capability of the "vegetation canopy" code to model a layer above the top soil layer with an additional heat capacity that is

    coupled radiatively to the soil. All other aspects of the vegetation canopy code is switched off on these surface tiles. This is described in the references included."

16. *"p. 12, line 8: it seems like you'd only want to nudge to climatology for weather simulations"*

    Agreed. The lack of clarity on this point was also picked up by the second reviewer in reviewer comments RC2. We have added a comment to clarify this.

17. *"p. 12, line 10: does the canopy affect LW emission?"*

    No, the emission is based on the surface temperature and the emissivity of the surface type.

18. *"18. p. 12, line 22: how is "Excess water" defined?"*

    For clarity, we have changed this to "Outflow at inland basin points with saturated soils is distributed. . . ". This is required, as otherwise the outflow would go into runoff, which would go into the rivers and then turn back into outflow.

19. *"When you say "system dependent", I think of computer system (e.g. a particular intel KNL machine) but I think you mean "model configuration"."*

    As with the response to Question 1, this is complicated because the GA configuration is not a model in itself, but a specific configuration of a more flexible model. For this reason, we have come up with the definitions below:
    Model (e.g. The Unified Model): The underlying code base used for modelling one component (i.e. the atmosphere in the case of the UM). Major iterations of this code base are described as model "versions".
    Science configuration: A set of scientific options and settings used to "configure" a model simulation. The Global Atmosphere configuration is essentially a set of options designed for use at resolutions requiring parametrised convection.

Major iterations of these configurations are described as “releases” (i.e. GA7 is the 7th GA release, not the 7th GA version).
System: The use of the word system is quite common in describing short-range prediction tools. e.g. the Met Office global NWP system uses a GA/GL configuration of the UM/JULES for its deterministic forecast. Similarly, it is quite common to describe an ensemble prediction system or a seasonal forecasting system. It is less common to describe a production climate model as a system, but by analogy, it is, with its system design essentially dictated by the experimental design of the activity for which it is being used.

20. *“. . . could you make solver tolerance a function of vertical level. . . ?”*

    This would be more difficult in the UM than in some other global models, because its dynamical core is non-hydrostatic. We have also had discussions about making the solver tolerance a more localised function of latitude, although this would also lead to load balancing issues. In line to our response about polar filtering, we would still like to understand the root cause of the noise in the wind fields at the poles.

21. *“p. 17 line 2-3: I don’t think there should be a paragraph break here.”*

    Agreed.

22. *“p. 20 line 1: “by either” sounds awkward.”*

23. *“how is the fraction of autoconverting cloud different than the cloud fraction?”*

    “It also seems awkward that you avoid advecting rain by doing something special when rain advects into a grid cell. . .”

    As a joint response to points 22–23, we have rewritten this paragraph, which hopefully makes this clearer. The quantity that we don’t want to advect is the

“rain fraction” rather than the rain mass mixing ratio, which we do advect. The rewritten paragraph is: *“We also improve the parametrisation of sub-grid rain fraction, i.e. the fraction of the grid box in which rain is held. When rain is created by autoconversion, this is set to be equal to the grid box liquid cloud fraction and when rain is created by melting snow this is set to be equal to the grid box ice cloud fraction. When rain is advected horizontally into a previously rain-free column, the rain fraction is set to the fraction of cloud directly above it. This avoids the requirement to advect a rain fraction in addition to the rain amount.”*

24. *“p. 21, line 19: “are scaled down before being combined. . .” How are they scaled down?”*

    Now clarified in the text.

25. *“p. 22, line 29 or so: what fraction of the time is RHcrit at its max or min value?”*

    We don’t have a measure of how often this is at its max or min value within a model run, but we have previously looked at what the model would have diagnosed using offline calculations, which suggests that these max and min values will be being activated, but not too often. Another piece of evidence that the impact of these on the model cloud is not large comes from some diagnostics we have looked at (but not shown here) showing histograms of cloud amount vs RH. At GA6, these show clear clustering on the old RHcrit curve, even though RHcrit is only being used for initiation from zero cloud and erosion from a fully cloudy state. In GA7 simulations there is no evidence of any unsmooth peaks, suggesting that the impact of the parametrisation is itself relatively smooth.

26. *“p. 25 top: what is the % reduction in energy fixer magnitude from GA ticket #87?”*

    This is discussed in the results section 4.3.

27. *"p. 25 line 25: is enhanced thinning of stratocumulus a good thing in your model?"* You raise a good point, that here, the word "enhanced" could be misread as "improved", so we will replace this with "additional". However, in GA6, the diurnal cycle of our SCu LWP was previously too weak and got neither thin enough by day nor thick enough by night. This change (GA:#13) and the warm rain microphysics change (GA:#52) do both act to improve this.

28. *"p. 26-27: I must admit I still don't understand how convection interacts with the turbulence scheme after reading this section . . . "*

    The reviewer's understanding of this (based on their comments) is correct, but we have updated the text to make this clearer.

29. *"p. 30: the relationship between CAPE timescale and resolution would make a good topic for a standalone paper"*

    We have decided not to write up the relationship between the CAPE timescale and resolution, as any closure based on this assumption will break down when going to the convective "grey zone". The development of a grey zone convection scheme for the Unified Model is currently an area of active research, which will itself require a new approach to the convective closure.

30. *"p. 42 line 25: "own/independent analyses" this sounds awkward. "*

    This was originally trying to clarify that we verify both against the model's own analyses (as generated through a cycling data assimilation system) and independent analyses. As it's difficult to express this elegantly, we have simply replaced this with "analyses".

31. *"I'm surprised there's no mention of land model tuning?"*

    The tuning section only describes tuning performed between GA6.0/GL6.0 and GA7.0/GL7.0, which did not include any *new* tuning of the land surface model.

32. *"p. 45, line 14: Isn't it customary to include a table where it is first mentioned?"*

    The main reason for including this table is to meet the requirements of the journal for "Details of the simulations performed", so it is included at that point. Given that this table is there, however, it is worth referencing at the start of the results section. We have clarified this a bit in the text.

33. *"p. 45 line 15: in section 4.1-4.3 you only talk about N96 and N216. You should mention both of these and NOT mention N768 until section 4.4. Also, it would be nice to get the rough dx values in km here for N96 and N216."*

    Agreed.

34. *"p. 47 line 2: I think you mean "returning" rather than "retuning""*

    Yes, thank you.

35. *"section 4.1-4.3: how long are the simulations you discuss in the climate section?"*

    We have clarified that these are 27 year simulations at the start of the section.

36. *"Fig. 12: it's really hard to tell orange from red dots."*

    The majority of figures in the paper have been created using a standard 8 class paired colour palette, so I have replaced the orange with the "light orange" colour in that palette.

37. *"Fig 16 top left panel: why have OLR color levels start at 0 W/m2 so the plot basically just looks like a red box? I think some other figures could benefit from this change too."*

    The aim of this is for Figs 15, 16 and 25 to use the same colour scales (as part of a budget). As the main points of discussion are in the differences, we would rather leave this as it is, unless the reviewer/editor feels very strongly about this.

38. *"Figure 21: 3rd from last line of caption: I think you mean blue instead of purple?"*

    Comment from the lead author: This comment made me chuckle because whilst this colour was deliberately chosen to have the same tone as the green arrows, exactly what colour it actually is has caused great debate! A quick straw poll of those around me has led to "purple", "blue" and "purple/blue", so I've decided to leave this as it is.

39. *"p. 57 line 7: I think you mean "OUR own analysis"?"*

    Verifying model changes against an analysis created using that model is tricky to describe, because the change in the model leads to changes in the analysis. For this reason, "own analysis" is a bit of a standard phrase, although I agree it does not read well. We've replaced this with "its own analysis".

40. *"I'm surprised you don't evaluate the land model skill at all."*

    We routinely perform a rudimentary analysis of the land surface model, but improved assessment of the GL configuration is something in our future development plans. This should include routinely assessing the performance of the land surface component in JULES-only simulations as well as coupled UM/JULES ones.

41. *"p. 59 end of line 9: "prep.b)" typo?"*

    This is standard BibTeX behaviour because "in prep." is used as the year. We need to wait for a doi for the "a" and "b" papers before final resubmission (see our response to reviewer's comments RC2), so this will go away then.

42. *"p. 60: differences between GA7.1 and 7.0 bullets 2-4: does CMIP6 specify these input datasets? More generally, you don't really say anywhere whether or not you follow the CMIP6 specifications for input data."*

    No, CMIP6 specifies the emissions of the aerosol precursors, not the optical properties of the aerosols themselves. Whilst DMS emissions over land are included, marine emissions from natural sources are dependent on the model. The emissions used in our assessment simulations are outlined in Table 1, which are mostly based on CMIP5; the CMIP6 protocols had not been published at the point that these simulations were first performed. The performance of the UKESM1 and HadGEM3-GC3.1 climate models with CMIP6 emissions will be the subject of subsequent publications.

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