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Geoscientific Model Development Discussions
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Interactive comment on "The Met Office Unified Model Global Atmosphere 4.0 and JULES Global Land 4.0 configurations" *by* D. N. Walters et al.

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1 Reply to general comments

We thank the referee for their thorough review and their support for our continued documentation of the Global Atmosphere and Global Land configurations. We appreciate their comments regarding the density of the text; we have reviewed the document and amended where necessary, but will also pay more attention to this when documenting future configurations. We address the specific points raised in turn below.

2 Responses to specific requests for clarification

2.1 Section 2.6

"'Momentum deposited when they break in the upper stratosphere and mesosphere drives a global circulation ...' doesn't quite make sense to me.":

We have replaced the introduction to the section with: "Non-orographic sources, such as convection, fronts and jets, can force gravity waves with non-zero phase-speed. These waves break in the upper stratosphere and mesosphere, depositing momentum, which contributes to driving the zonal mean wind and temperature structures away from radiative equilibrium. Waves on scales too small for the model to sustain explicitly are represented by a spectral sub-grid parametrization scheme (Scaife et al.,2002), which by contributing to this mechanism leads to a more realistic tropical quasi-biennial oscillation. The scheme, described in more detail in . . . ".

2.2 Section 2.7

"Use of punctuations would improve the readability of this sentence: 'For stable boundary layers . . . by the "MES-tail" function . . . ":

We have replaced this with: "For stable boundary layers and in the free troposphere, we use a local Richardson number scheme based on Smith (1990). Its stable stability dependence is given by the "sharp" function over sea and by the "MES-tail" function over land . . . "

2.3 Section 3.3

"I don't understand the description of Fig.1":

GMDD

6, C1933–C1941, 2013

We have updated this paragraph to read: “Figure 1 shows the impact of this change on the droplet size distribution for three rain mixing ratios. For the smallest mixing ratio, the intercept of the particle size distribution is at least ten times greater in GA4.0 than GA3.0, whilst for the highest mixing ratio, the two lines are very similar.”.

“[Is] it possible to draw a connection between this particle size distribution change and the slight worsening of the tropical-subtropical rainfall biases in GA4.0?”:

Clean tests of this change suggest that it may contribute a small proportion of the signal over the ocean, but the majority of the increase in tropical-subtropical rainfall biases has been attributed to the changes to the convection scheme.

2.4 Section 3.4 *(i)*

“‘Morcrette and Petch (2010) found a feedback in the model caused by ...’ Was this feedback spurious? Please, explain.”:

Yes, the feedback was spurious. We have rephrased this as : “By studying the prognostic cloud scheme’s cloud tendency terms, Morcrette and Petch (2010) found a spurious feedback caused by an explicit link between the rate of sub-grid homogenization in the cloud erosion parametrization and the relative humidity. For this reason, an alternative way . . . ”

2.5 Section 3.4 *(ii)*

“Shouldn’t the cloud fraction be reduced because of the departing ice? Please, clarify.”:

“The large scale precipitation scheme calculates the rate with which frozen condensate falls from one layer to the next. In addition, the PC2 cloud scheme represents how the

GMDD
6, C1933–C1941, 2013

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falling ice will increase the ice cloud fraction in the layer it enters. In the absence of shear, the ice cloud fraction is advected downwards using the ice fall velocity. Any ice cloud fraction falling into clear air is then rescaled in the horizontal by assuming it has filled the grid box in the vertical.

"In the presence of shear, the falling ice cloud fraction can be displaced laterally as it falls, making it less likely to fall into the vertically contiguous cloud below and more likely to fall into clear air. In Wilson et al. (2008a) and GA3.0, this source term was calculated assuming that the vertical wind-shear had a globally constant value of $1.5 \times 10^{-4}\,\mathrm{s}^{-1}$ and without taking account of the size of the model grid box. In GA4.0, the wind-shear term is calculated from the vertical shear of the model's horizontal wind and the potential increase in ice cloud fraction due to the lateral displacement of the ice cloud as it falls is related to the size of the model grid box (which varies between systems using different horizontal resolutions as well as with latitude in any one system due to the model's latitude-longitude grid).

"This falling ice parametrization assumes that although ice may fall out of a layer and increase the ice cloud fraction in the layers below, the ice cloud fraction remains constant in the layer the ice is falling from. These layers lose mass as ice falls out, but their lateral extent remains constant. This represents the ice cloud becoming optically thinner whilst maintaining its lateral extent.

"Wilson et al. (2008b) describe how the temperature..."

2.6 Section 3.4 *(iii)*

"[The change to the wind-shear term] appears to be a significant improvement over the 'constant shear' assumption in GA3.0. Is the impact of this improvement on weather/climate simulation known?":

Yes, we have seen positive impacts on the model cloud fields in regions where there is

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6, C1933–C1941, 2013

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a lot of shear. We have added the following to highlight this in this discussion: “From a theoretical point of view, this is a significant improvement over the constant-shear assumption. Preliminary tests (not shown) suggested that the use of the model shear improved the representation of cloud in the mid-latitude storm tracks, particularly over the Southern Ocean.”

2.7 Section 4.1 *(i)*

“By the metric presented in Fig. 6, the performance of GA4.0 appears to be slightly worse than the GA3.0 . . . contrary to the conclusion that both models perform comparably.”: The method of assessment outlined in Walters et al. (2011) is to make a qualitative overall assessment based on a large basket of quantitative measures. Figure 6 shows that for the global tropospheric assessment area, there are 14 fields where GA4.0 has a detrimental impact (marked as red) and 11 fields where the impact is beneficial (the 9 marked as amber and 2 of the points marked as green, where the zero line is outside the range of observational uncertainty). The largest detriment is in tropical precipitation over land, which we highlight in our conclusions. In our overall assessment, however, we believe that this is balanced by the improvements to the global cloud and radiation budget (not included in Fig. 6) and the middle atmosphere (which only contributes to a fraction of the zonal mean points in Fig. 6).

2.8 Section 4.1 *(ii)*

“This sentence doesn’t make much sense to me; should be rewritten to clarify.”:

We have rewritten this as “The N96-AL_clim GA3.0/GL3.0 control did not exhibit such a large error in this region, where the new configuration now shows a greater similarity between N96-AOIL_clim and N96-AL_clim”.

GMDD
6, C1933–C1941, 2013

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2.9 Section 4.4

"This comparison between the climate and NWP simulations involving the stratospheric warm bias reduction is . . . not a clean comparison.":

This is true. As discussed in Sec. 3.9 and Table 5, the specification of ancillary data in general, and of the ozone ancillary in particular, is one area where the GA3.0 configuration was not used consistently between different systems. This was addressed in GA4.0 by the use of the SPARC-II ozone dataset in all systems.

The reason why only the June/July verification is shown is that the impact of the move from the Li and Shine (1995) dataset is largely seen in the summer hemisphere and we preferred showing results from the northern hemisphere due to the higher density of the radiosonde network there. To address this, we will supplement this figure with the southern hemisphere verification in Jan/Feb. We have attached this combined plot to this reply and will change the discussion of this text to: "There is also an improvement in upper level temperatures in NWP simulations, which Fig. 14 shows to be particularly large in the summer hemisphere. Whilst this is due partly to the improvements seen in N96-AOIL_clim, there is a larger contribution from the replacement of the Li and Shine (1995) O_3 dataset with the SPARC-II dataset as described in Sect. 3.9."

2.10 Figures 1 and 2

The dotted lines in these figures have been replaced with a dashed line and a dot-dashed line to make them clearer. These updated figures are attached to this reply.

Interactive comment on Geosci. Model Dev. Discuss., 6, 2813, 2013.

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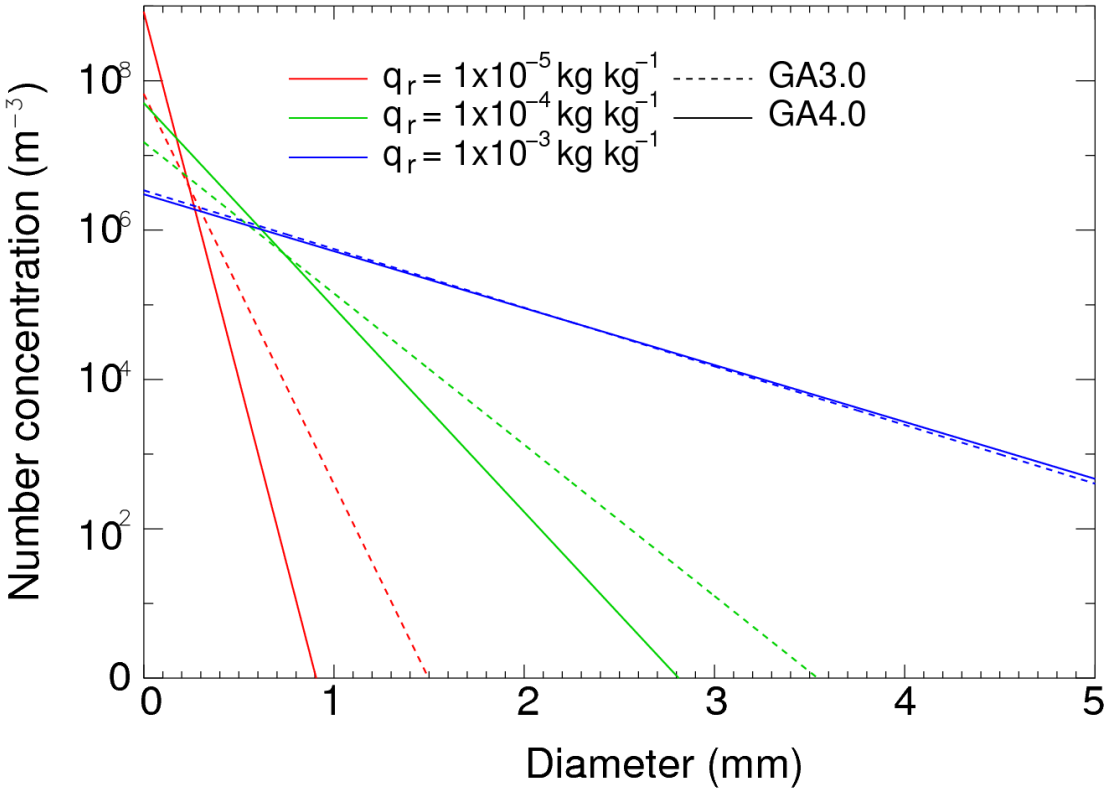

Fig. 1. Edited version of figure 1.

GMDD
6, C1933–C1941, 2013

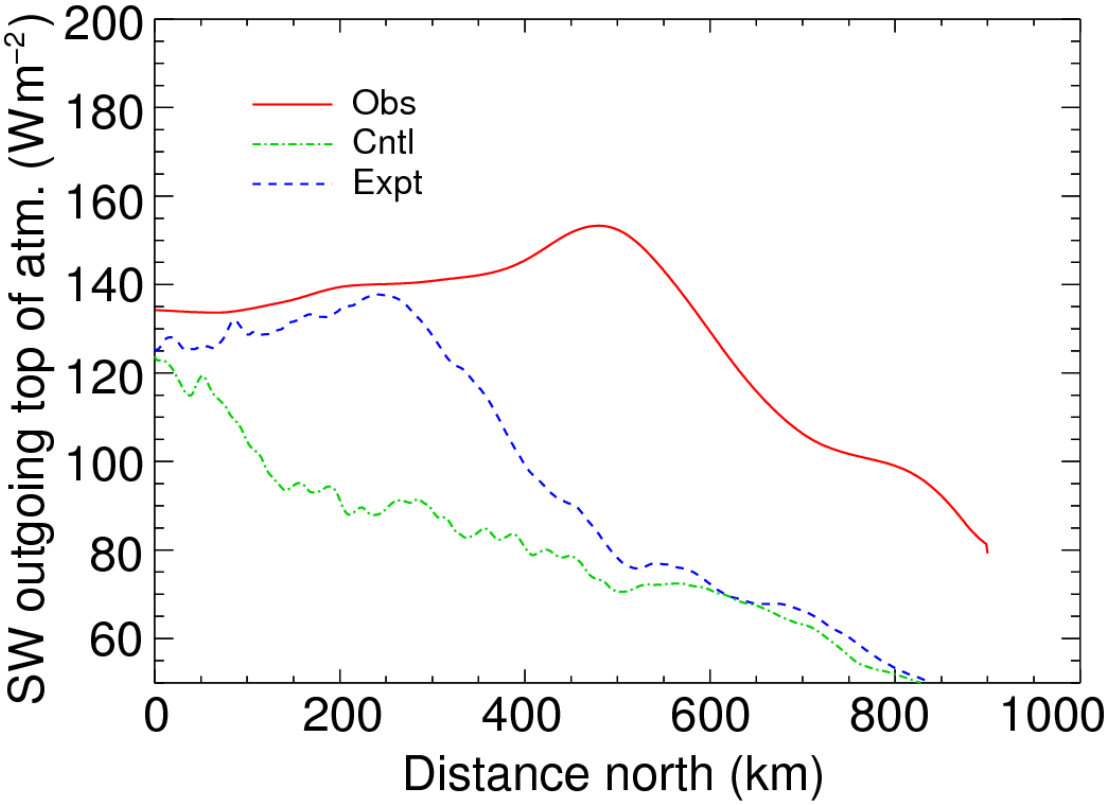

Fig. 2. Edited version of figure 2.

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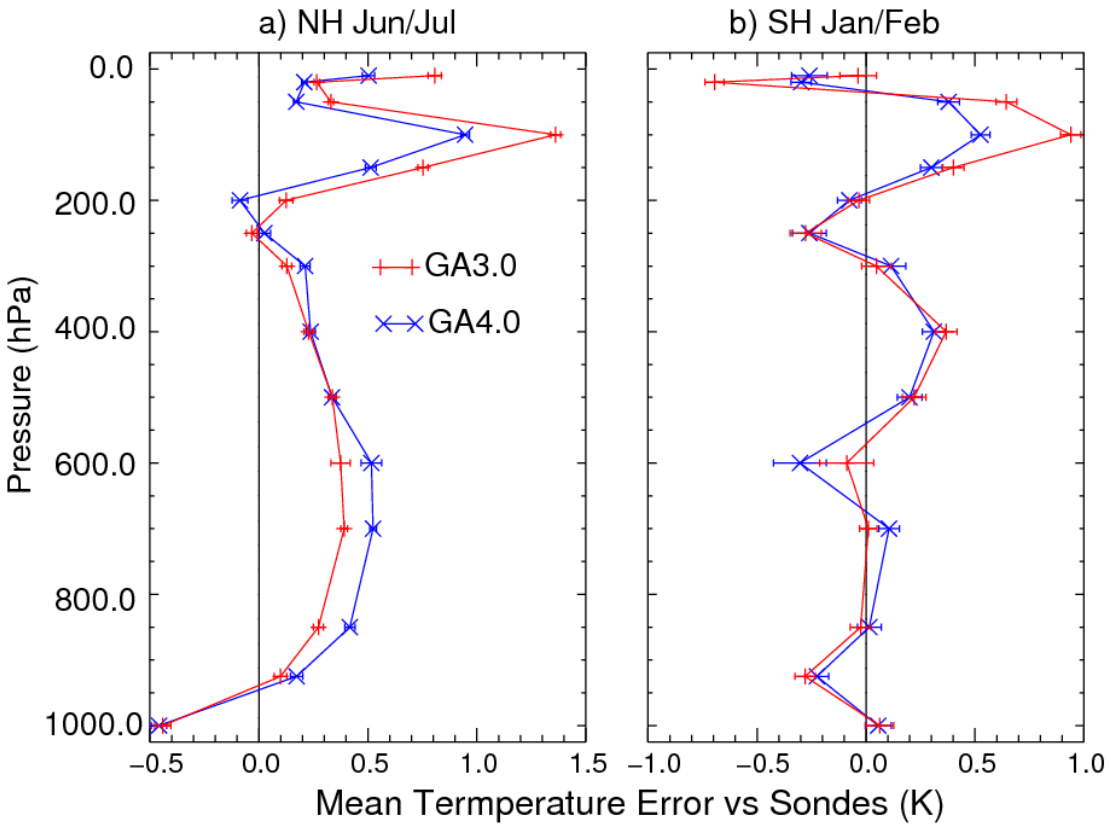

Fig. 3. Edited version of figure 14.

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