

## ***Interactive comment on “Development and evaluation of an Earth-system model – HadGEM2” by W. J. Collins et al.***

**W. J. Collins et al.**

bill.collins@metoffice.gov.uk

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We thank this reviewer for their valuable comments. We have tried to improve the coherence of the manuscript throughout. The level of technical language was chosen to make the paper understandable to anyone involved in Earth system modelling in general, rather than experts in a specific component of an Earth system model. However we appreciate the suggestions to tighten up the language in places.

The aim of the paper is to describe the model, its development and performance, rather than present new scientific results. The recommendations to describe more fully the comparisons with data are well taken. A figure comparing modelled and measured methane concentrations has been added.

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We have improved the quality of the figures and adopted a consistent colour scheme. The figures are chosen to illustrate specific model processes, particularly processes that were foci for development during the model construction. Different processes are important for the different model components. For example methane had previously been implemented in the radiation scheme as a global mean number. In the Earth system model the methane varies considerably (by a factor of 10) in the vertical dimension and figure 2 was chosen to illustrate this. For CO<sub>2</sub> the vertical variation is less important than understanding the balances between photosynthesis and respiration. So to evaluate the CO<sub>2</sub> sources and sinks the CO<sub>2</sub> seasonal cycles are compared in figure 21.

Specific comments:

Abstract. Line 16: We agree that such a figure would be useful, and have added a comparison of surface temperatures to section 5.

Page 999, line 1: The artificial correction terms were to the ocean heat fluxes. This is reworded.

Page 999, lines 13-16: We agree that "online" is jargon.

Page 999, lines 22-25, page 1001, line 24: We have adopted the suggested wording changes.

Page 1003, line 14: We have re-worded the description of the handling of lakes in the model. Lake fraction has no implications for the precipitation which all feeds into the soil moisture.

Page 1004, lines 11-20: The soil properties within a grid square are not tiled in HadGEM2, therefore there is no concept of soil carbon from the non-crop fraction. In the scheme used to represent the terrestrial carbon cycle the carbon in the vegetation removed by land clearance is added to the most labile soil pool rather than being directly emitted into the atmosphere. The inability to use the soil carbon to drive the

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methane emissions does indeed mean that we miss an important feedback. We are grateful to the reviewer for providing references to this.

Page 1004, lines 21 to 26: The ice shelf calving rate is specified at a value that maintains the balance between precipitation and loss processes in the control simulation (pre-industrial conditions). Therefore if precipitation were to increase or decrease in a future climate the nominal ice mass would increase or decrease, with an opposing effect on sea level. In itself this is physically realistic since the calving rates are likely to take millenia to response to increased mass. The process that is missing is that there is no change in this rate with climate (for instance to reflect warmer sea temperatures or lubrication from meltwater).

Figure 1: This figure is for pre-industrial conditions. The numbers within the boxes reflect the changes in the water stores over the 10 year period and therefore any imbalance in the water fluxes. The most variable component is the sea ice where input has exceeded output by 3% over this period. All other fluxes are in equilibrium to within much less than 1%.

Page 1006. Line 19: Suggested wording change adopted.

Page 1007, lines 13 to 15. The expression "preferential source term" was jargon and has been removed. This refers to only allowing dust emissions from topographic depressions.

Section 2.6: We agree that more description is needed for the methane budgets. In this section we are focussing on processes internal to the model (wetland emissions, stratospheric removal, dry deposition). The external data (including "natural" emissions from termites, ruminants etc.) can be chosen to fit the experimental setup. Similarly the methane lifetime is not an intrinsic feature of the model, but depends on the emissions and climate. We have supplied more information on the external forcing used to spin up the methane in section 3.2 along with the budgets and lifetimes in that setup.

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Figure 2: Data with fixed surface methane had been used here. This has been updated. The colour scale has been changed to emphasise the hemispheric differences. More details on the methane emissions are included here and in section 3.2. The validation of the chemistry in section 4.3 has been extended to include methane.

Page 1009. Lines 6-7. This description of the anthropogenic land use changes has been improved to make it clearer. Indeed, in areas where the agricultural mask is applied C3 and C4 grasses are allowed to grow. By land-management we meant no irrigation or harvesting, and have clarified this.

Page 1010. Line 3: The calculation for NEE still applies, even when land use is changed since the vegetation carbon is transferred to the most labile soil pool. This will naturally increase the respiration leading to a decreased NEE. Fires are not included in the HadGEM2 model. We have therefore removed the discussion of land use change and fires from this sentence.

Page 1011. Line 1: This sentence has be reworded.

Section 2.8: Iron is indeed a 3D tracer in the ocean, as are nitrogen, carbon and silicon. Text now clarified

Page 1012. Line 5: Sentence has been reworded.

Page 1012. Lines 26-27: The reviewer is correct that this sentence only applied to trace gases and aerosols.

Page 1013. Line 7: CLAW has been expanded.

Page 1013. Line 27: As the reviewer points out, it is difficult to assess whether the model had a soil moisture "bias". So we have rephrased this as "low soil moisture". It was not simply a precipitation bias, but also caused by larger evaporation than in previous models. This aspect was discussed more fully in Martin et al. (2010).

Page 1014. Lines 2-3: This phrase has been reworded.

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Page 1014: Lines 14-17: The different timescales involved in the spun up components are all discussed here so we don't think that lumping them together will cause any confusion.

Page 1016: Lines 1-6: We have improved the description of the methane spin up here, including the sources and sinks. The main control used fixed surface methane, so the interactive methane was spun up separately. Figure 2 has been changed to use the results from the interactive methane simulation rather than fixed surface concentrations. It now simulates an interhemispheric gradient.

Section 4.1 We agree that the inundation fraction is not well simulated and have now added text to comment on the differences.

Page 1017, lines 1-2: The large-scale hydrology scheme uses the subgrid topography ("topographic index" in Gedney and Cox 2003) to calculate the inundated fraction. This data may not have been accurately derived over the Amazon.

Figure 7: We have tried the log-log scatter plot, but decided that it was important to show the geographic information. We have changed the colour scale to give more emphasis to the larger rivers.

Page 1019, line 1: This has been clarified to read "anthropogenic emissions of ozone precursors".

Page 1019, lines 19-20: This sentence has been reworded. The Matthews and Fung data is used as the prior for the inversion.

Page 1020: Section 4.4: This section has been reworded. The point we wish to make is that we did not run 140 year historical simulations after each parameter change during the model tuning in order to compare the model with present day observations. Hence the tuning was all done with a pre-industrial climate and pre-industrial CO2 levels. Once the ultimate parameter sets were chosen, the model was finally run over the historical period to produce the output shown.

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Page 1020: Line 21: We have clarified that the term "climate correction" refers to ocean heat flux adjustment.

Page 1021. Lines 13-14: We think the visual comparisons of the broad spatial patterns between the vegetation distributions are more informative than statistical correlations. Note that TRIFFID tends to produce more extreme values (nearly zero or nearly 1) than the IGBP distribution.

Page 1023: Line 23: Suggested changes to text made.

Page 1025. Lines 1-9: The non-technical language has been removed. Both the observed and model fluxes were for decadal averages for the 1990s.

Page 1028. Lines 9-10: This sentence has been reworded

Section 6. Conclusions: The suggested wording change has been adopted

Table 2. ISLSCP has been replaced by "Potsdam"

Figure 1. As explained above, the fluxes are close to equilibrium even for a ten year period.

Figure 8. References now added. The Ayres et al. reference did not provide a range for the observations.

Figures 17-19. Suggested changes made.

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