

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

**Anonymous Referee #2**

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### General comments

While it is important to document models, especially the extremely comprehensive Earth system models, it is a difficult task. The authors have done a good job in documenting HadGEM2 but there remain a number of items that require clarification. The difficult task of writing an overview multi-author model documentation paper requires that all co-authors contribute somewhat equally, at least, in terms of their responsible sections. However, this synergy is difficult to obtain in practice and unfortunately this is reflected in the quality of the final text. Some of the sections are written in a relatively poor and seemingly incomplete manner compared to others. In addition, several of the statements seem to be written in a very non-technical manner. As a consequence, the overall manuscript does not appear significantly coherent. In addition, as a reviewer,

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who has not reviewed a manuscript before for Geo-scientific Model Development, I am somewhat unsure if the purpose of the journal is to document models without significant emphasis on the results or are the results equally important. As the other reviewer (Rachel Law) notes discussion of results, even if they are not well simulated, is important. Equally important, is the fact, that the description of the model should be complete since this is the overarching purpose of a paper that is being submitted for publication in GMD.

Overall, the manuscript also does not appear consistent. Averages are reported over different time periods for different quantities. While the manuscript shows latitude versus height plot of CH<sub>4</sub> (Figure 2) for the present-day, it is somewhat surprising it does not show zonally-averaged distribution of more important CO<sub>2</sub>.

### Specific comments

Abstract. Line 16. “. . . but the impacts on the overall simulation of present-day climate are slight.” This statement is based on the discussion in section 5. However, the section 5 itself does not discuss any figures which would compare say, for example, zonal distribution of simulated temperature and precipitation from the model with and without earth system components. It would be useful to have some figures to support this statement.

Page 999. Line 1. “. . . used artificial correction terms to keep the model state from drifting”. Correction terms of what, heat and water over ocean (or over land as well), I suppose. Please reword this sentence to be more explicit.

Page 999. Lines 13-16. Please consider rewording the phrase “online consistent calculation” as well as this whole sentence which is somewhat unclear.

Page 999. Lines 22-25. I think it is more appropriate to say here that ESMs have the ability to be driven with either concentrations or emissions of CO<sub>2</sub>.

Page 1001. Line 24. Replace “surface exchange scheme” with “Land surface exchange

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scheme” to be more explicit. Also, make this change for the title of section 2.2.

Page 1003. Line 14. “Lakes are not modeled interactively, but have a fixed extent”. This sentence seems to imply that interactive lakes will have varying extent, but I do not think that’s what the authors mean to imply. In addition, while this paragraph discusses how evaporation is handled over lakes, it makes no mention of precipitation. As a reader, I am wondering how precipitation over lakes is handled.

Page 1004. Lines 11 to 20. This paragraph discusses that a soil carbon climatology is used for calculating methane emissions from wetlands because land-use changes yield unrealistic changes in wetlands CH<sub>4</sub> emissions. I do not follow this argument exactly. Can’t the model just use simulated soil carbon from the non-crop fraction of the grid cell. In fact, recent modeling and empirical wetlands CH<sub>4</sub> studies (Spahni et al., *Biogeosciences*, 8, 1643-1665 and *Nature*, doi:10.1038/nature10176 ) have found that CH<sub>4</sub> emissions do increase over time due to more soil carbon and/or higher NPP (which provides higher root exudates) both associated with CO<sub>2</sub> fertilization effect. So using a soil carbon climatology implies that feedbacks associated with changes in soil carbon are not captured.

Page 1004. Lines 21 to 26. This paragraph discusses the methodology used to account for calving ice shelves. To account for absence of calving ice shelves a time invariant freshwater flux is applied to the ocean. However, the snow falling on the ice sheets is expected to change with climate. Doesn’t this imply that the global water budget will not be perfectly closed? This seems unfortunate given that so much effort is put to make sure that evaporation from lakes is conserved.

Figure 1. The manuscript does not mention over what time period is the 10 year average taken for freshwater fluxes. Are these 10 years from an 1850 control simulation? In addition, 10-year seems too short a period for averaging fluxes.

Page 1006. Line 19. Please replace “online oxidants” with “interactively simulated oxidants” or something along these lines.

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Page 1007. Lines 13 to 15. I am not sure what does “preferential source term” means in this context. Please explain.

Section 2.6. Tropospheric chemistry. The discussion about methane emissions from wetlands seems incomplete. For the tropospheric chemistry to work properly CH<sub>4</sub> emissions from other sources must also be taken into account. Clearly, while CH<sub>4</sub> wetland emissions are the biggest natural source they contribute only 20 to 25% to the total CH<sub>4</sub> emissions. What about emissions from termites, landfills, ruminants, rice paddies etc. And, what about the soil sink of CH<sub>4</sub>. In addition, with all the chemistry implemented I would have expected the authors to report the simulated global average methane lifetime in the model.

Figure 2 which shows the zonally averaged vs. height plot of simulated CH<sub>4</sub> concentrations raises some concerns. First, clearly a tropospheric concentration of around 1800 ppm cannot be simulated without sources other than wetland emissions. Second, if these are present day (by the way, these simulated concentrations are averaged over what time period.) CH<sub>4</sub> concentrations why is there no interhemispheric gradient present, as also raised by the other reviewer. The 1984-1997 NOAA surface CH<sub>4</sub> concentrations show an interhemispheric gradient of around 140 ppbv so even with the color scale used in Figure 2, I would have expected to see something. This is one of the sections of the paper that hasn’t been written in great detail. A comparison of simulated zonally- averaged surface CH<sub>4</sub> concentrations with observations would also be useful here.

Page 1009. Lines 6-7. “However, no representation of crops or land management are yet included”. I think, although TRIFFID does not include a crop PFT, it represents crops using grasses. If this is the case please make this clear. Earlier in the manuscript it is mentioned that because of anthropogenic land use change a soil carbon climatology is used for CH<sub>4</sub> emissions. So as a reader I would like to know how anthropogenic land use change is implemented.

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Page 1010. Line 3. "In the absence of fires or time varying land-use, ...". How is NEE calculated when agricultural mask increases in area over time.

Page 1011. Line 1. Please consider rewording the phrase "allows the completion of the carbon cycle". This sounds very non-technical.

Section 2.8. Ocean carbon cycle. After reading this section I am unsure if iron is a 3-D tracer or only used to restrict photosynthesis in the ocean.

Page 1012. Line 5. "Combining these couplings leads in turn to biogeochemical feedback loops." Please consider rewording this seemingly non-technical sentence.

Page 1012. Lines 26-27. "Other emissions from the terrestrial biosphere such as from vegetation, soils and fires are not considered." This sentence is too vague to follow. Please be more explicit here. Emissions of which trace gases and/or aerosols?

Page 1013. Lines 7. Please expand CLAW when using it the first time.

Page 1013. Line 27. "The model had a soil dry bias in the summertime ...". Please replace this by "low precipitation bias" because in the absence of any global soil moisture data set it is difficult to say if any model has a soil moisture bias or not.

Page 1014. Lines 2-3. "This spiral eventually led to ...". Please consider rewording this phrase.

Page 1014. Lines 14-17. It seems inappropriate to lump physical ocean (timescales of ~1000 years), vegetation distribution (~200 years), soil carbon content (~1000 years) and methane concentration (~10-12 years) altogether.

Page 1016. Lines 1-6. The manuscript really does not describe properly how CH<sub>4</sub> is handled. These lines indicate that natural CH<sub>4</sub> emissions are calculated using simulated CH<sub>4</sub> lifetime and observed 1860 concentration minus the CMIP5 anthropogenic emissions. But what about the natural CH<sub>4</sub> emissions from wetlands that were mentioned earlier. And, of course, natural emissions include emissions from other non-

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wetland sources as well which the manuscript does not mention at all. In addition, if you control simulation was run for 280 years why was the simulation with CH<sub>4</sub> run only for 15 years. This seems to imply that CH<sub>4</sub> is not an integral part of the Earth system model and something totally separate. Plus, as a reader I am still confused if the model uses CMIP5 anthropogenic CH<sub>4</sub> emissions why it does not simulate the interhemispheric CH<sub>4</sub> gradient for the present day which I mentioned earlier.

Section 4.1. I do not agree that the simulated inundation fraction is overall reproduced reasonably compared to observation-based estimates. Please reword this sentence and properly highlight model deficiencies.

Page 1017. Lines 1-2. The reason for overestimation of the inundation extent over the Amazonian region is mentioned to be the limitations in topographic data. What does this exactly mean? Please be more explicit.

Figure 7. It is really difficult to see how well the simulated river discharge from the model compares to the observation-based estimates. It would be much useful if these data were plotted on a log-log scatterplot.

Page 1019. Line 1. "Note that we used the same anthropogenic emissions as were used by ...". Anthropogenic emissions of what?

Page 1019. Lines 19-20. Please reword this incomplete sentence.

Page 1020. Section 4.4. I am confused after reading this section about whether or not transient historical simulations were performed and/or analyzed for the purpose of this manuscript. Line 2 reads - "it was much too computationally expensive to perform repeated 140 year historical simulation" and lines 9-12 read "hence we also show here a comparison of the model state from a transient historical climate simulation ...". The phrase "this tension between" reads very awkward, so please reword this. In addition, I am unable to follow the overall logic of this section. Clearly, the HadGEM2 model has been used to perform historical simulations for CMIP5 so I am unable to follow the

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arguments presented and the logic of this section.

Page 1020. Line 21. Please explicitly mention what does "climate correction term" actually means.

Page 1021. Lines 13-14. "Overall HadGEM2 does a good job at simulating the global distribution of trees". It would be really useful if statements like this are accompanied/corroborated with a spatial correlation number.

Page 1023. Line 23. Please replace "Potsdam model mean dataset" with "model mean NPP from the Potsdam inter-comparison".

Page 1025. Lines 1-9. The discussion of Figure 18 where HadGEM2 and HadCM3LC simulated terrestrial ecosystem fluxes are compared at the Harvard forests site reads in a very non-technical manner. The phrases like "flattening of the heterotrophic respiration", "more peaked GPP", "less peaked respiration" appear to suggest that this and a lot of other terrestrial processes related sections were written by non-terrestrial/non-carbon researchers. In addition, neither the section nor the figure caption tells over what time period are the observations and model simulated results averaged. This has implications for the comparison being made here. Clearly, the year-to-year climate observed at the Harvard forests site cannot be reproduced in the climate model. Terrestrial ecosystem fluxes show a large interannual variability. Assuming that the model climate is similar to the climate at the Harvard forests site then the comparison must be made over a sufficiently long time so that the effect of interannual variability is minimized.

Page 1028. Lines 9-10. "However we expect our simulation of future climate to be better using an Earth system model". I do not think anybody can make this claim. Please reword this sentence.

Section 6. Conclusions. Please reword "enhanced our ability to understand" with "provides the ability to understand". I do not think this manuscript has shown any examples

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of enhanced understanding.

Table 2 mentions ISLSCP. Is this meant to represent the Potsdam model-mean? If yes, either say so explicitly in the text or in the table caption.

Figure 1. Average over a longer time period of at least 50 years would be more useful and indicative of the model behavior.

Figure 8. Why is there no shading on observations in the bottom-most panel. Also, what is the reference for observations in this figure.

Figure 11. The color scale used in this figure is really difficult to follow. Why not choose more sensible colors like warm and cold colors for positive and negative numbers.

Figure 17. In the legend use the proper sign for writing +/-.

Figure 18. In the bottommost right-hand panel replace "DATA" with "Observations".

Figure 19. Please use the same y-axis for the right-hand side figures.

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