

Reply to the referee comments on the paper “MIROC-ESM: model description and basic results of CMIP5-20c3m experiments”

Dear Dr. Hughes,

We would like to thank you for providing constructive comments on our paper. In the following our response to the comments are underlined.

pg. 1069, line 4. please explain in more detail the phrase "are launched at 70 hPa", with regards the gravity wave source. To me this implies that the QBO is forced with 2000s conditions (lines 6-9). It seems to me that the QBO is then unlikely to change in response to climate change - so is it appropriate for the RCP simulations? I would like the authors to explain the consequences with a few more sentences (though I note that full analysis of the QBO is reserved for another publication).

We have added a sentence describing the gravity wave sources and a main conclusion of the QBO change to the text:

The non-orographic gravity waves are mainly emitted from convection, jet-frontal systems, and adjustment processes, in the troposphere, propagating upward to the 70 hPa level. In the tropics, an isotropic source of non-orographic gravity waves is launched at 650 hPa in the present version. The strength of the tropical source is arbitrarily tuned so that the QBO with a realistic period of 27-28 months on average can be reproduced under present-day (2000s) conditions. As a consequence, the period of simulated QBO elongates with increasing GHG concentrations due to strengthening of the Brewer-Dobson circulation in the stratosphere (Watanabe and Kawatani, 2011).

pg. 1076, line 4. Please include more detail on what "recursively adapted to each model" means.

We have added some more words to the text.

pg. 1076, line 7. "carbon cycles was integrated" should be "carbon cycles were"

Thanks. We have corrected the sentence.

pg. 1076, line 8. "the resultant carbon cycle data was again" - my preference would be

to use "state" rather than "data".

We have changed the word following your suggestion.

pg. 1076, general comment. Mention how a run is judged to be in equilibrium.

We have added explanations on this to the text.

pg. 1077, line 9. "subsequent updates" - is there any reference available for this?

There is no peer-reviewed publication after Sato (1993).

Data is available at <http://data.giss.nasa.gov/modelforce/strataer/>.

We have added this reference to the text.

pg. 1077, lines 15-20. The 4 socioeconomic studies do the RCP scenarios (future climate change) not the historical period.

We have corrected the description of land-use scenario.

pg. 1078, general comment. It would be good to know if the changes observed are statistically significant.

We agree your comments. However, from the viewpoint of visibility of the figures, we cannot show the statistical significance of the observed changes.

pg. 1078, lines 11-18. this needs more explanation/description since it's not clear to me that it does match the observations particularly well. For 1900-1950 over the ocean, the model gets about 1/2 the warming and warms over Eurasia rather than the observed general cooling.

Please see below.

pg. 1078, line 14. "slightly smaller" - seems to be up to 50% smaller. On average, how much smaller over the ocean, land and globally?

Please see below.

pg. 1078, line 17. "slightly smaller" give some values

We have changed the explanation/description on this paragraph following your comments.

pg. 1078, line 20. The text "climatological values of" can be deleted in my opinion.

We have deleted that phrase.

pg. 1078, line 22. "The climatological" replace with "MIROC-ESM-CHEM"

We have changed the text following your suggestion.

pg. 1078, line 22. "slightly cooler" - values? how about a plot of temperature difference versus latitude?

We have added values to the text.

pg. 1078, general. In figure 4 there is a very large drop in temperature around the model year 1995. This is not commented on yet after this the model is consistently cooler than the observations. Can the authors comment on this? How does this affect later results - such as figure 10.

We have added comments on this to the text. This cooling bias is about 0.2~0.3K on global average, so we think that this does not largely affect Fig.10.

pg. 1079, lines 18-19. I wouldn't use the phrases "fairly well" or "basically alike" myself.

We have changed the phrases.

pg. 1079, line 27. "may have disappeared by the 1990s" is it there in the preindustrial control simulation?

Yes, the preindustrial control simulation has more sea-ice than the historical, but shows similar bias in the slight warming trend.

pg. 1081, lines 15-21. but earlier it was stated that a gravity wave source term was tuned to get the right QBO period. Without understanding the mechanisms involved I would suggest that this is why the period agrees and also why the simulated QBO is more regular (since the gravity wave source term is apparently constant). Please comment on this.

We have added a speculation about reasons for the regular behavior of simulated QBO.

pg. 1082, line 14. Why do you get a positive anomaly over the Sahara?

This is because a slight increase of dust as shown in Fig. 14.

pg. 1083, line 5. "range suggested by the IPCC (2007)", please be more specific -a full reference & quote values.

We added a specific description on this.

“range suggested by the IPCC (2007): +0.25 to +0.65 W m⁻² as the 5 to 95% confidence interval of the model ensemble including 9 previously published studies (Mickley et al., 2001,2004; Shindell et al., 2003,2005; etc.) and the 10 models of the ACCENT experiment (Gauss et al., 2006).”

Gauss, M., et al., 2006: Radiative forcing since preindustrial times due to ozone changes in the troposphere and the lower stratosphere. Atmos. Chem. Phys., 6, 575–599.

Mickley, L.J., D.J. Jacob, and D. Rind, 2001: Uncertainty in preindustrial abundance of tropospheric ozone: implications for radiative forcing calculations. J. Geophys. Res., 106(D4), 3389–3399 doi:10.1029/2000JD900594.

Mickley, L.J., D.J. Jacob, B.D. Field, and D. Rind, 2004: Climate response to the increase in tropospheric ozone since preindustrial times: a comparison between ozone and equivalent CO₂ forcings. J. Geophys. Res., 109, D05106, doi:10.1029/2003JD003653.

Shindell, D.T., G. Faluvegi, and N. Bell, 2003: Preindustrial-to-presentday radiative forcing by tropospheric ozone from improved simulations with the GISS chemistry-climate GCM. Atmos. Chem. Phys., 3, 1675–1702.

Shindell, D.T., G. Faluvegi, N. Bell, and G. Schmidt, 2005: An emissionsbased view of climate forcing by methane and tropospheric ozone. Geophys. Res. Lett., 32, L04803, doi:10.1029/2004GL021900.

pg. 1084, lines 1-3. So does the model predict basically no grasslands? It seems that there is either trees or croplands in most non-desert regions. If so this needs to be commented on.

We added some descriptions of the reasons for the appearance of “no natural grasslands”:
“Therefore, this map indicates ~ land use change. *Natural grasslands reproduced by the model seem to exist only in the north circumpolar region and other limited regions. This could be explained by two reasons: First, MIROC-ESM-CHEM failed to capture dry and non-tree area, and represents these areas as the ecosystem with the coexistence of grass and trees. Second, in the model forcing for land use change, most grassland expanding in dry and temperate region is categorized into pasture because they include the area grazed by nomadic livestock as well as the pasture that is enclosed.*”

pg. 1084, line 20. It appears that simulated forest carbon is much less than observed, for example in South America and Africa.

We have changed the sentences following your suggestion:
“The distribution pattern of forest biomass based on observations and reproduced by MIROC-ESM-CHEM are shown in Fig. 19b, c. *Forest carbon in the tropical regions such as South America and Africa carbon was generally underpredicted, while the South and Southeast Asia were overestimated. The extent of forests and moderate accumulations ~*”

pg. 1085, line 1. "linearly scaling" please give some more details of how this was done. The values suggest that you are just ignoring carbon in the lower 0.5 m of soil - i.e. $((2511/1.5) * 1) = 1717$. I am not convinced that adjusting the soil carbon pool size is justified here. It is quite possible that uncertainties in vegetation productivity, soil temperatures etc etc could easily explain the higher soil carbon content. By ignoring the bottom third of the model soil carbon you are proposing that the difference is due to under-sampling of 'real world' soil carbon and I think that needs more justification.

We added the phrase “~previously reported as the carbon stored in the 0-100 cm depth” and more explanations:

“After linearly scaling to 1m depth (in our model, the soil grid system for soil decomposition assumes 1.5m depth), the amount of soil organic carbon was 1717 PgC, which is within the range of 1395–2070 PgC previously reported as the carbon stored in the 0-100 cm depth (Ajtay et al., 1979; Batjes, 1996; Post et al., 1982; Prentice et al., 2001; IGBP-DIS, 2000). However, there is still large uncertainty for the estimates of the total soil organic carbon as there are not enough samples for the soil organic carbon especially in deeper layer (Tarnocai et al., 2009) and the amounts of soil organic carbon estimated by the model is strongly affected by it’s settings (e.g. the number of soil pool, turnover rate in decomposition, and effects from soil water and temperature). In addition, as the mechanisms for the high accumulation of soil carbon like frozen carbon or peat in the north circumpolar regions are not well known, global terrestrial ecosystem models represent these processes implicitly with the lack of precise mechanism of the interaction between physical environment and soil carbon dynamics.

pg. 1087, lines 28-29. I think this sentence would be easier to read if you did not use brackets to create a double sentence. I think it is much better to have a second short sentence, though I realise this is a matter of personal preference.

We would like to keep the current style to keep the sentence.

Figure 8 caption. Mention data source - as you do for figure 6.

We have added ERBE’s reference.

Figure 9 caption. Include the text rather than refer the reader back to the caption for figure 8.

We have revised the caption following your suggestion.

Figure 19. It would be nice to see bias plots.

We appreciate this valuable comment by the reviewer. After consideration, we decided not to present bias plots because of the limitation on the length and the balance among other sub-categories (i.e., atmosphere and ocean) in this paper, although we understand the usefulness of the bias plots.