Dear Editor

We have addressed the reviewers' comments in the revised manuscript; our detailed responses to their comments are given below (reviewer's comments in italics).

Reviewer 2

General Comments:

This paper describes a new version of the Reading Intermediate Global Circulation Model, version 4 (IGCM4). It goes over new additions, setups, and parameterizations to the model, and examines how well the model simulates basic tropospheric and stratospheric variables, including tropospheric and stratospheric temperatures, precipitation, Outgoing Longwave Radiation (OLR), and zonally-averaged stratospheric winds. The climate sensitivity of the model coupled to a slab ocean was also examined. In general, the model does a decent job in simulating temperature, precipitation, and OLR compared to the NCEP-DOE reanalysis and CMIP5 models, with most of the errors being attributed to a lack of aerosol forcing and cloud parameterization errors. The model also compares well to ERA-40 reanalysis in the stratosphere, with errors in zonal average wind speed attributed to the model's gravity wave drag scheme.

I have personally never used this model. However, I think this description is easy enough to understand, and thus good enough to be accepted with revisions. The revisions I have for the paper are listed in the next two sections.

We thank the reviewer for this positive review.

Specific Comments:

There were several scientific clarifications I think would be helpful for this paper:

1) In the introduction, it would be useful to describe the scientific benefits of having an intermediate complexity climate model. For example, it would help if you described in more detail how a "hierarchy of models" can help deduce underlying physical processes. You should also emphasize that given this model's relative computational cheapness, it would be a great candidate for running a large ensemble, or for doing very long simulations. Those long simulations themselves could help estimate equilibrium climate sensitivity given long time-scale changes and feedbacks, and could also help paleoclimate simulations. Describing these sorts of scientific benefits in the introduction would certainly strengthen this paper.

This is a good point: we now include a section in the revised manuscript (lines 48-70) in which we include just such a rationale for the use of intermediate complexity models such as the IGCM4:

'The rationale for such a model in the hierarchy of potential model codes is now addressed. Understanding key scientific questions related to climate and climate changes relies on understanding processes within the atmosphere, whose complex and nonlinear nature entails the use of global circulation models. However, understanding such complex processes in models is extremely challenging since unpicking processes within state-of-the-art climate circulation models can be extremely difficult given their complexity- especially when their computational demands are taken into account, leading to limits in both integration times and data storage.'

'Having said that, it is necessary for models to be complex enough to simulate the processes that are relevant to understanding a given question of interest. This is the niche which intermediate circulation models such as the IGCM occupies. This niche consists of models that are complex enough in terms of dynamical processes to represent a wide variety of processes from monsoonal circulations to extratropical storm tracks. However, their relative simplicity compared to state-of-the-art climate models that are employed by the Intergovernmental Panel on Climate Change (henceforth IPCC), enable process-level understanding to become more tractable because of (a) computational speed enabling long integrations or large ensemble members, and (b) flexibility and ease of use enabling the examination of idealised scenarios. Examples where the IGCM4 might be used are e.g.; conducting integrations of idealised perturbations to boundary conditions such as

sea-surface temperature, topography, or continental distributions; conducting ensembles of multi-century integrations to collect robust statistics of small-amplitude responses to particular forcings".'

2) In section 2.3, there is the statement "the height at which total albedo reaches (A+S)/2". I am not sure what "height" that statement is referring to. Is that the model vertical level (e.g., one of the sigma levels), the snow depth, or something else? I think stating explicitly what that height is will help the reader.

The value is height in metres. We now include a table which shows these surface parameters as a function of surface type (Table 1 in revised text).

3) I am not familiar with the radiation schemes used in IGCM, and thus it is not clear what the benefit is to move from NIKOSRAD to Morcrette. Does Morcrette have ozone absorption while NIKOSRAD doesn't? Is Morcrette more physically realistic, or does it produce a more accurate climate simulation? Is it computationally cheaper, or more easily parallelized? I think spending more time describing how you chose your radiation scheme would help this document immensely.

The NIKOSRAD scheme was found to produce 2Δz oscillations under certain conditions in the stratosphere, which is why it was replaced. The Morcrette scheme is originally written for the ECMWF model so is fast. We now state the reason for replacing NIKOSRAD with MORCRETTE in the text.

4) I was unable to find any description of the convective schemes used in the model, even though there are statements about tuning and rainout timescales. There needs to be a description of the convective scheme somewhere in this paper. Without one, it is impossible to have a strong opinion on the scientific validity of the model.

We now describe the convection scheme in the IGCM4 in section 2.3 (lines 192-195): it is similar to that described in Forster et al (Clim. Dynamics, 16, 833-849, 2000).

5) The same goes for clouds produced by the large-scale dynamics. Is there any sort of physical paramterization to deal with the radiative and microphysical effects of those? You do describe a marine stratocumulus scheme, but what about clouds over land, or those generated by extratropical cyclones that aren't convective? Again, a more complete description of the moist physics in this model is needed.

We now describe the stratiform cloud and precipitation scheme in section 2.3 (lines 204-207); again it is identical to that described in Forster et al (Clim. Dynamics, 16, 833-849, 2000). Cyclones over both land and ocean can form stratiform cloud and precipitation.

6) What is the dataset you are using for OLR to compare against the model? I am assuming it is NCEP-DOE reanalysis, but this isn't explicitly stated anywhere. It would be good to state in the document where you acquired your OLR data.

We have now inserted a reference for the OLR data in the figure where it is used and in the text.

Technical comments:

In the last sentence of the second paragraph in section 2.3, you should drop "e.g.", so that it just says "such as HadGEM2".

We have made this change.

In the first sentence of section 2.4, you need to add the word "was", so that the phrase is either "which was originally written" or "which originally was written".

We have added the word "was" to the text.

In the first sentence of the last paragraph of section 2.4, the final wording should be *changed to something like "A version of the Kawai and Inoue (2006) parameterisation for marine stratocumulus cloud has also been implemented in IGCM4."*

We have changed the sentence to the above recommendation.

In section 2.5, I would either drop the parenthetical phrase "a very good approximation for the stratosphere", or add a citation to support it.

We have dropped the phrase.

In section 3.1, the first sentence needs the word "as" included, like so: "…is prescribed ***as*** *a monthly-varying climatology".*

We have made this change.

I would probably not use the phrase "basket of models". Maybe instead use the phrase "collection of models", or "(sub)set of models".

We have changed the word "basket" to "subset".

In the third paragraph of section 3.1, the third sentence is somewhat difficult to read. I would reword it like so:

As a guide to the IGCM's performance in the context of other models, the mean±one standard deviation precipitation bias amongst a ***subset of models present in the CMIP5 archive being used*** *for the UN Intergovernmental Panel on Climate Change's 5th assessment report (IPCC AR5) is also shown: the comparison is for the CMIP5 model configuration using prescribed "AMIP" SSTs, since coupled ocean-atmosphere biases tend to worsen model performance.*

We have made this change.

*I also found the third sentence of the fourth paragraph of section 3.1 difficult to understand at first. I would probably rephrase the beginning like so: "****Thus****, for the JJA season as* ***well as*** *the DJF season…".*

We have made this change.

In the last sentence of paragraph 5 of section 3.1, I think you meant to state that the imbalance, not the balance, of the energy fluxes is 1-2 w/m2.

We have made this change.

In section 4, is the climate sensitivity the equilibrium or transient sensitivity? Just specifying which type will help.

We now specify that we mean equilibrium climate sensitivity.

When discussing Figure 11, it might be better to call it the energy imbalance, or just the net downward energy flux.

We have changed the wording to "imbalance".

In Figure 2, it may be better if there was a labeled color bar instead of labeled contours, as the actual contour values can be hard to read. However, this is more of personal opinion than a strong suggestion.

We have added bolder and larger labels on the curves, and changed colours, to help readability.

In Figures 4 and 5, the observed precipitation panel plot should have a different, explicitly labeled color bar. That way it doesn't make the reader think there is negative precipitation, which is unphysical. It also should state that the bottom three (CMIP5) plots are for average precipitation bias, not average precipitation.

We have inserted a new caption to clarify these issues. It specifically mentions that there are two colour bars (a,b and c,d,e,f) and also states the CMIP5 subfigures are differences with CMAP (which is also indicated in the subfigure heading).

Figures 7, 8 and 9 would probably be improved by adding a difference plot between the reanalysis and IGCM4. However, it isn't a must-have.

As well as showing reanalysis, Figures 7 and 8 now overlay contours of IGCM zonal wind and temperature on colours of IGCM4-reanalysis difference to help to show where the differences are (e.g. in the southern hemisphere tropospheric jetstream). We have not done so for Figure 9, since the additional land-ocean boundary would make such a contour plot more difficult to read- rather we now show eddy fields of both IGCM4 configurations and reanalysis at both 500 hPa and 200 hPa.

In Figure 11, I would again call it an energy imbalance, or just the net downward energy flux.

We have changed the caption to say “Annually averaged net downward zonal surface energy imbalance”