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.

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.

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.

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.

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.

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.

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.

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".

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".

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."

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.

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

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

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.

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…".

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/m^2.

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

In section 4, when discussing Figure 11, it might be better to call it the energy **im**balance, or just the net downward energy flux.

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.

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.

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.

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