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Geoscientific Model Development Discussions

Interactive comment on "The Met Office Global Coupled model 2.0 (GC2) configuration" *by* K. D. Williams et al.

Anonymous Referee #1

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Review of 'The Met Office Global Coupled model 2.0 (GC2) configuration' by K. D. Williams et al. for GMD.

The paper presents the main features of the new UK Met Office climate model, and the main characteristics of the simulated climate. The paper is clear, correctly structured and written. Methods and results are adequately described. The scope and the results are clearly relevant for a publication in GMD as a Technical/Development/Evaluation Paper.

Regarding the model description, the authors often made the choice to present the differences with respect to the previous model version, and not to fully document the model. The reader is send to the relevant literature for pre-existing model features. This makes the model presentation short, clear and synthetic, which is good. But it

may request some times for the reader to get all the features. However, a climate model is a big thing, and a full description would not fit in GMD.

The part about the model results is clear and fair, except a concern about ENSO (see below). Regarding the planned scientific use of the model and the limited space in a GMD paper, the choice of model features and the level of details presented here seems relevant.

Page 537, about ENSO. The paper states that ENSO metrics are good, but without giving any figure but one. This is not relevant for a scientific paper. Please give the relevant numbers, or remove this part. However, the CLIM simulation is rather short for a proper analysis of ENSO.

Minor concerns.

The paper describes three set of experiments (CLIM, SEAS and NWP). Could you specify in all figure caption which is the set used in the analysis ? And sometimes be clearer in the text.

Page 526, line 5. GC 2.0 apparently uses the 'conservative' option of the NEMO namelist. I don't feel that the text here is very clear for the reader. Could you be more specific about the goal of this option (conservation when ice fraction changes during the coupling time step), and how this results is achieved ?

Page 528, line 19. There is no quantification of the model drift in the paper. Surface and subsurface drift should be quantified. Even with this short run, I would appreciate to also have a quantification of the deep drift. (By the way, can you really say that CLIM is a 'long' run ? With respect to CMIP standard, probably not.)

Fig 15 : the longitude axis of the top panel doesn't fit with the other, which is confusing.

I suggest a publication in GMD, with the improvement proposed above.

Best regards