

***Interactive comment on* “Climate model configurations of the ECMWF Integrated Forecast System (ECMWF-IFS cycle 43r1) for HighResMIP” by Christopher D. Roberts et al.**

Anonymous Referee #1

Received and published: 28 May 2018

The authors provide a detailed description of the ECMWF-IFS model for climate simulations in HighResMIP. Following the presentation of the different model components and the coupling between them, a comprehensive and honest analysis of the model performance is given, taking into account a wide range of aspects from regional to global scale.

The scientific quality of the model description and analysis are excellent, and require no changes except for a few minor technicalities. Given the nature of this contribution, the scientific innovation is limited. An important aspect highlighted in this work, often ignored in the scientific community, is that model calibrations based on "short" periods

[Printer-friendly version](#)

[Discussion paper](#)



of one year +/- are insufficient for multi-decadal simulations.

I recommend publication after considering the following minor modifications:

(1) Page 3, lines 29-30: the time-steps for the two model configurations are large (36s/km for Tco199, 48s/km for Tco399) compared to other modeling systems. It may be worth mentioning that this is due to the different nature of the IFS model (semi-implicit, spectral) than models that the scientific community is likely more familiar with (e.g. WRF, split-explicit, typically 6s/km time-step). Also, if experiments were made with smaller or larger time-steps, it would be nice to mention any differences here. Related to this: page 5, section coupling: if experiments were made with different coupling frequencies, it would be nice to briefly mention this here.

(2) Page 4, lines 11-15: I would recommend adding one sentence on how the 1km landuse classification is mapped onto the model grid and how dominant vegetation categories are derived.

(3) Page 7, lines 13-14: One important aspect of contributions to (not only, but in particular) this journal is the possibility for readers/reviewers to reproduce the results. I believe that readers trying to do so will need more information on the seed differences (see also comment (8) below).

(4) Page 7, sections 2.8.1 and 2.8.2: Although these are partly mentioned later in the text, I would recommend adding a statement with typical spin-up times (vs the spin-up times used here) to each of the sections (2.8.1: atmosphere and land/soil properties; 2.8.2 ocean and ice).

(5) Page 11, lines 27-28: Is it possible to describe briefly what the "dynamic origin" for the temperature errors could be? It seems to me from looking at the figures that the most significant temperature biases referred to in this last section are correlated with coastlines close to significant orography (e.g. the Andes).

(6) Page 12, lines 30-32: Are the deficiency in the TKE scheme and the statement that

an increase in vertical resolution leads to better results (lines 4-5, same page) related? If so, it would be nice to make this connection here.

(7) Page 15, line 21: I believe that there are two commas missing: "shorter, more intense" and "longer, less intense".

(8) Page 19, section "Code and data availability": As mentioned before, I believe that readers should have the possibility to reproduce the results (assuming they have access to the software). In addition to the present description of where to obtain the model code, I would recommend making the changes to the model configuration available (either as instructions in the abstract or to download, or as a patch set to download).

General comments:

(1) Related to the last comment on section "Code and data availability", I am missing a short statement on the computational requirements of running ECMWF-IFS. Where were the current simulations made, what were the required resources? What would be minimal and typical resources needed to run the model (#nodes or cores, main memory, temporary/permanent disk space)? I am not asking for a detailed model performance analysis, just a few numbers that help potential users to estimate the resources that they may need for their experiments. Given the large time-step of the model, I believe that the computational resources are lower than for other coupled modeling systems, which makes the ECMWF-IFS an attractive alternative.

(2) The number of figures is close to overwhelming. I believe that some/few (e.g. figure 13) could be left out and I invite the authors to consider this for their final version.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-90>, 2018.

Printer-friendly version

Discussion paper

