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Geoscientific Model Development Discussions

Interactive comment on "Development and evaluation of CNRM Earth-System model – CNRM-ESM1" *by* R. Séférian et al.

Anonymous Referee #2

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General comments

The article by Séférian et al. presents the first Earth-System Model developed at CNRM. It is based on the CNRM-CM5 coupled climate model (Voldoire et al. 2013) and includes the ISBA module for a representation of the land surfaces in the carbon cycle. The ocean biogeochemistry (PISCES) was already implemented in CNRM-CM5. The subject of the paper particularly fits within the scope of GMD. The overall structure of the paper and the quality of the science are very good, and the paper is well written. The main features of the physical behavior of the model are presented and evaluated. The protocol of the preparation of the model and the realization of the simulations (spin-up, pre-industrial control and historical simulation) are rigorously done. The amount of work and knowledge is impressive. Without a doubt, the authors are

true experts in their fields.

However, the interpretation of the evaluation results deserves revisions (specific comments below). The authors use words like "realistically", "moderate" or "reasonably well-simulated" to describe the behavior of the model compared with observations. This is totally subjective and does not have its place in such a paper. I understand what the authors mean by this, and it would be acceptable in an oral presentation, but not in a scientific paper. These statements should be supported by more objective quantifications or comparisons with other models. For instance, how can we say that a model is "realistic", or an agreement is "moderate"? Two different paths can be considered: either using statistics that objectively quantify the agreement (for instance, "my model explains XX% of the variance of the observations, and we assume that above [a meaningful threshold], the model meaningfully reproduces the observations on this diagnostic"), or by comparing the results with other models that will be used as benchmark. Both approaches are used in the paper (notably the comparison with CNRM-CM5) but not systematically. For instance, Figures 6, 7 and 8 of the supplementary materials are among the most interesting figures for they put the model in the multi-model intercomparison context. I strongly recommend taking advantage of the availability of the CMIP5 ESMs outputs to strengthen the evaluation of CNRM-ESM1 and see how it compares with other models. One way to do it is a portrait diagram showing synthetic metrics of model-data agreement (see Anav et al. 2013, Fig18) with the results of other ESMs for the carbon cycle variables. Nevertheless, this is only a possibility and the most important point to me is to remove, as much as possible, the subjectivity from the presentation of your results. In the specific comments I point out the issues that need more robust support.

One truly annoying point is the use of the supplementary materials. The authors do not specify which figure is pointed out by the reference to the supplementary materials, and the reader has to guess which figure he should look for. I still don't know if you use Figure 1 or not.

Following this, I recommend the publication of the paper in GMD after major revisions.

Specific comments

Abstract: the end of the abstract is about the too strong flow of North Atlantic Deep Water and the accumulation of anthropogenic carbon in the deep ocean. Meanwhile, those points are not reported in the summary and conclusion section (section 5). I suggest either presenting these points more comprehensively in section 5 or removing them from the abstract (maybe focus on something else) so that the abstract and last section are aligned in terms of contents and highlights. As well, it is a shame to end the abstract the way it is right now, with a quite negative statement. A focus on the good performances of the model would be more appropriate.

Page 5688, line 20: the expression 'non-significant' suggests that there is a statistical support behind this statement. Change it for something like 'very small' and add an estimate of this change in percentage (of the average field for example).

Page 5688, line 25: please specify which figure in the supplementary materials is targeted by this mention. I assume you mean Figure 2, but add it for the convenience of the reader. Additionally, I'm not convinced by this figure. Where am I supposed to see an improvement between CM5 and ESM1? Either I don't understand the meaning of the figure therefore I assume it is not correctly explained or there is no comparison with observations on this figure and I don't see how it should be able to show any improvement. Please clarify.

Page 5693, line 16; page 5694, line 12, Line 662, Lines 687-688: specify which figure in the supplementary materials is targeted by this mention for the reader's convenience.

Page 5689, lines 16-18: what I see on Figure 6ab is a bias map, i.e. a quantification of model errors. I have no particular mean to tell if those biases are sufficiently small to tell that the model "realistically simulates" the observed climate (same for "moderate" on line 18). I suggest either adding the results of CNRM-CM5 (either on this figure

or another one) to put this statement in perspective of a model that can be used as a benchmark, or add an objective quantification of the model-data agreement that could make more sense than qualifications like 'realistic'.

Page 5690, line 1-2: I don't understand how you can say that the vertical structure of S matches better with WOA2013 than T. They don't have the same units, and we have no idea about the uncertainty linked with the diagnostic to judge if the difference is smaller for S than T. Either provide objective elements to support your statement, or remove this sentence.

Page 5691, lines 3-4: What are your criteria to say that the model 'reproduces well' the pattern of the observations? Replace 'CNRM-ESM1 reproduces well the regional pattern of MLDmax' with 'CNRM-ESM1 simulates the main regional features of MLDmax'.

Page 5691, lines 19-20: This sentence is too vague. Either remove it, or put this result in perspective with another model (if so, we need to see the figure).

Section 4-2-3: I have an issue with this section. You talk about skills that are 'different', or 'similar'. But objectively, on your plot, you don't have the means to say that two correlations are truly different (statistically speaking). It is even more the case when you compare the behavior of the correlations for two different variables, or two different seasons, since they are not computed on the same number of degrees of freedom. Your statement "the difference in simulated SSS between the two models can be attributed to [. . .]" is too strong compared with the true ability of those metrics to demonstrate what you say. In my opinion, metrics are mainly good tools to highlight outliers, but not so much to demonstrate any physical link (even if, sometimes, we can 'understand' their behavior). I would be much more convinced by a set of well-chosen maps of the biases of SSS, PR and sea ice, with both model versions (CM5 and ESM) side to side. They would show a much more reliable proof of the difference between the models and where does it come from. By the way, Figure 3c of the supplementary materials looks weird to me. On panel (a) mainly, there is sea ice in the Labrador Sea (between

0-10%). There is no sea ice in this region in panel (b), thus the difference should not be zero in panel (c) in the Labrador Sea. You might have an issue with the display of your results.

Page 5696, lines 10-21: What about removing the El-Nino years before the computation of your climatology of CNRM-ESM1 to try to match the methodology of Takahashi et al. (2010)? Either provide a justification for not doing it, or provide an additional figure with the El-Niño years removed.

Minor points

Page 5673, line 6-7: suggest adding a reference to Flato (2011)

Page 5693, line 8: looking at the Taylor Diagram, the standard-deviation ratio for MLD (on average) is around 0,5. Therefore, it shows that the model underestimates (rather than overestimates) the spatial variations of this field compared with the observation-derived MLD product of Sallée et al. 2010.

Page 5690, line 5: suggest changing “Thanks to” with “Because of”

Page 5690, line 27: define in this sentence what MLDmax and MLDmin are (for convenience), or explicitly refer to the caption of the figure.

Figure 8: the color bar seems to be incoherent with the contour line for the model. If I’m correct, the dashed contour line highlights the isoline 15 but it remains within the 0-10 range of the color field. This is really a minor point but you might have a look at it to correct it.

Page 5693, lines 5-6: add ‘apart from a tendency to show higher variability (standard-deviation ratio).’

Page 5694, line 11: replace “lesser extend” with “lower extent”

Figures 10, 11, 12: the color palette with white in the middle should be used for differences (as on Figure 5), not for a full field. Change it for a meaningful color palette

(i.e. in the same way as the color palette you use for sea ice cover). I would also suggest adding the difference maps for all those fields. These to avoid leaving the reader playing a game of ‘guess where it’s greener’.

Page 5694, line 18: it looks like the authors forgot to change (REF) with the right reference of the Princeton University Forcing

Page 5695, line 10: replace “compared to” with “than”

Page 5696, line 8: I’m not too sure about the expression “carbon cycling”... I suggest double-checking that it is truly correct.

Page 5696, line 8: replace ‘In term of’ with ‘In terms of’

Page 5698, lines 24-26: need for a reference to support what you say about the origin of the biases in the equatorial upwelling systems.

Page 5699, line 19: replace “following” with “after” to avoid repetition

Page 5701, line 24: replace “discrepancies similar to” with “similar performances as” (more positive)

References

A. Anav, P. Friedlingstein, M. Kidston, L. Bopp, P. Ciais, P. Cox, C. Jones, M. Jung, R. Myneni, and Z. Zhu, 2013: Evaluating the Land and Ocean Components of the Global Carbon Cycle in the CMIP5 Earth System Models. J. Climate, 26, 6801–6843. doi: http://dx.doi.org/10.1175/JCLI-D-12-00417.1

G. Flato, 2011 : Earth system models : an overview. WIREs Clim Change 2011, 2:783–800. doi: 10.1002/wcc.148

Interactive comment on Geosci. Model Dev. Discuss., 8, 5671, 2015.