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Interactive Comment

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

Anonymous Referee #1

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The authors present the development and a first evaluation of the CNRM earth system model, which evolved from the climate model CNRM-CM5 by coupling modules for the land and ocean carbon cycle to the model. They evaluate the main physical drivers of the land and ocean carbon cycle and compare these results to the previous model version. A number of ecosystem parameters, biogeochemical tracers, and carbon fluxes are evaluated against observation based estimates. The paper is of high scientific relevance for the earth system modelling community and definitely within the scope of the journal. The model-model and model-data comparisons are sound and the paper is overall well written. There are, however, a number of issues listed below that should be addressed by the authors. I recommend to accept the manuscript for publication in GMD after these minor revisions have been addressed by the authors.

General comments:

Comment 1: The authors find a quite large sensitivity of sea ice area to the coupling time step of the model (24h vs. 6h), but they somewhat hide this result in the supplement. Since this issue is mentioned several times in the manuscript, I suggest moving Figure 4 of the Supplement into Section 4.2.2 or 4.2.3 and discussing it there. Also, a discussion as to why the coupling time step causes these differences would certainly be of interest for the audience (although I see that this could be beyond the scope of this model description).

Comment 2: In 4.2.2 there is a discussion about deep convection in the model and the fact that CNRM-ESM1 simulates open ocean polynyas, but not in the region where the so-called wedell polynya was observed in 1974-1976. CNRM-CM5 in contrast simulated plynyas in the wedell sea, and the authors state that "The use of GELATO6 in CNRM-ESM1 compared to GELATO5 in CNRM-CM5 in addition to the change in coupling frequency might be at the origin of this model-data disagreement."

While it is interesting that open ocean polynyas have been observed and are not only seen in model simulations, the number of occurances is a bit low to call this a "model-data disagreement" in my opinion. Is there any evidence that the difference in location is due to the different version of the ice model and/or the timestep? Isn't there a couple of other possible reasons in the model set-up? Altogether I find this paragraph on polynyas a bit too vague for a model evaluation paper, and I would suggest either deleting this discussion from the manuscript or go much more into the details as to why the location of polynyas is where it is in the different model versions (but this might as well be beyond the scope of this model description).

Comment 3: p 5696, l 14-15: "...the model-data mismatch is likely related to the decision of Takahashi et al. (2010) to exclude observations from El Niño years from their analysis [...] This hypothesis is validated when comparing model results against recent data products derived from statistical Monte-Carlo Markov Chain or Neural Network gapfilling methods (Landschützer et al., 2014; Majkut et al., 2014)."

If the comparison with the latter data products gives better or different results, these should be presented here. Why compare to a data product with a known bias and then discuss that if we would compare to another available data product results would look better?

Comment 4: Summary, p 5702, l 5-8: "This change is attributed to [...] a higher coupling frequency that induces a stronger northward flow of deep water masses from the Southern Ocean." In the manuscript, the authors do not provide any evidence or model sensitivity tests to support this hypothesis. Unless they do so in a revised version of the manuscript, I suggest deleting the statement on the coupling frequency here.

Specific comments:

In the abstract, please state to which time period the uptake values (2.2PgCyr-1 land sink, 1.7PgCyr-1 ocean sink) refer to.

p 5673,l 6-10: "The models of this class ... primarily through their contribution to the concentration- and emission-driven experiments that compose CMIP5." I don't really understand what the authors want to say here. The really new point with ESMs is that we can run emission driven (something that traditional climate models cannot)

p 5673,l 18: Please consider replacing "ensemble" by something else. "Ensemble" should be reserved for different realisations of model runs of the same model, which the authors do not mean here, as far as I understand.

p 5680, l 25-29: The nitrogen limitation of the Land BGC model is mentioned and a reference is given (Calvet et al.2008). Nitrogen limitation is a critical point in ESM simulations, and CMIP5 has shown that including it can potentially alter the reaction of land carbon uptake to enhanced CO_2 and climate change dramatically. Therefore, I think it would be good to provide a brief summary of how the nitrogen limitation is implemented in the CNRM-ESM.

p 5682, l 21-22: "Only the internal concentrations of iron, silicon and calcite inside the sinking particles are prognostically simulated." I think this can be misleading: The amount of carbon transported down by sinking is also prognostically simulated, right? Only the ration C:P and C:N is fixed. Please clarify.

p 5683, l 2-5: Nitrogen fixation is mentioned, ("... nitrogen fixation should balance...") but it remains unclear how it is implemented. Could the authors please clarify.

Section 3.1: In my opinion "Equilibrium strategy" is not a very good title for this section. The authors describe the spin-up of the model her, so "Spin-up strategy" or just "Model Spin-up" would be more suitable perhaps. Also, in line 7 a 320 year online spin-up is mentioned. Then towards the end of the section there is "an online adjustment ... for 400 years". Is this something different? Or is this the same as 320 years, but one of both is a typo? Please clarify, and (if the same) consider mentioning the online spin-up only once in this section.

p 5685 l 16: "...performed with the NCAR model (Ammann et al.,2007)." The "NCAR model" should be CCSM.

p 5685 l 24: "...20th century reconstruction of the NCAR model (Ammann et al.,2003)." The "NCAR model" would be the CCSM, but in Ammann et al.(2003) they don't use CCSM or another model developed by NCAR. Please make sure that the model name and reference are correct.

p 5687, l 4-6: I do not understand how the land C flux could be "explained by missing processes in ISBA such as the [...] riverine-induced carbon transport from land to oceans". A riverine transport could close the carbon cycle in the sense that it would transport the excess C taken up by land into the ocean eventually. This would still result in a positive land C-flux (but the land pool size would be constant in equilibrium which cannot be the case in the current model setup).

p 5688 l 3-4: "...appears to amplify the global average cold bias of 0.8C (with biases

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of−0.7 and -1C in boreal winter and summer, respectively)" I do not understand these numbers: Are these the differences between CNRM-CM5 and CNRM-ESM? The ESM biases have already been state above? Please clarify.

p 5688, l 20: "Summer PR is similar between the two models with non-significant changes in simulated values (< 10−5 mm day−1)..." I find it difficult to believe that PR changes are that small between the two different models, particularly since during boreal winter bias is reduced by 0.014 mm day-1. Pleas check and/or clarify.

Section 4.2.2, evaluation of MLD: The terms MLD_max and MLD_min are a bit ambiguous. I guess it is not the real min and max over all model timesteps but rather the mim and max of the mean over some period (probably monthly)? Is this time window comparable to the time resolution of the Sallee data? Also please consider to call the Sallee et all data "observation based estimate" or similar, not "observations" in the figure caption of Figure 7.

p 5693, l 8: "...but strongly overestimates the spatial variations of this field." From Figure 9 i read a standard deviation < 1 for MLD in both cases, which means that variability is underestimated compared to observations. Regarding Figure 9: Since the small symbols for seasonal values are there, they should be discussed (why are there two seasons where MLD is very different in the two models)?

p 5693, l 14-15: "...difference in simulated SSS between the two models can be attributed to the revised water conservation interface and erroneous distribution of sea-ice cover. Besides, changes in coupling frequency (i.e. 24 to 6 h) might be at the origin of differences in skills between the two models." I do not understand what the "revised water conservation interface" is. If this leads to important differences it must be at least briefly described Section 2. Is there any evidence that the coupling frequency changes the model skill in reproducing SSS?

p 5699, l 5: replace "climate change" with "climate forcing". The climate change is seen in the indices looked at, which react to a forcing.

Figure 2: The text says, land carbon flux is 0.75 PgCyr-1. From the Figure I would guess that the number must be more around 1.25. So I assume that either the Figure axis or the number in the text is wrong. Please check.

Technical corrections:

When refering to the supplement, could the authors please give the figure number, rather than just "(Supplement)".

p 5675, l 3: acronym AOGCM not explained at first use

p 5675, l 6: "...(ARPEGE-Climat, SURFEX, NEMO, GELATO, respectively)" this addition makes no sense without further explanation. Since all components are discussed below, I would just delete this.

p 5675, l 8: "...was based on version 5.2. This version of the atmospheric code derives from cycle 37 of..." I think the authors mean their version 6.1, but as the sentence is constructed "This" refers to version 5.2. Please consider rephrasing to avoid confusion.

p 5675, l 23-24: "The main difference from the CNRM-CM5.1 atmospheric model is the improved treatment of volcanic aerosols." But this is not a difference in ARPEGE, right? If so, please consider rephrasing something like "The interactive chemistry module already used in CNRM-CM5.1 has been updated with an improved..."

p 5678, l 1: "The coupling between the atmosphere and the surface models is implicit..." I do not understand what "implicit" should mean here. I guess, the land surface is a sub-model of the atmosphere (organised as a subroutine call)? Please reword this sentence.

p 5678, l21: "... the recommendations of the JPL-2003-25 report (Sander et al., 2006)." The "JPL-2003-25" is not helpful for the reader, consider rephrasing: "...the recommendations of Sander et al. (2006)." Please also correct the reference Sander et al. (2006) in the References section.

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p 5678, l 21-23: "Photochemical production and loss rates of ozone rely on the main gas-phase reactions driving the NOx , HOx , ClOx, BrOx catalytic cycles." I think it would be easier to understand, if this sentence would come before the previous one.

p 5679 l 8: "In the present concentration-driven experiments..." -> "In the concentration-driven experiments presented here..."

p 5679, l9-10: "...provided by CMIP5" -> according to the CMIP5 protocol

p 5679, l 18: "3 non-vegetated surfaces" -> 3 non-vegetated surface types

p 5680, l 22: "This is a key advantage of this approach as most the..." -> A key advantage of this approach is that most of the ... without any additional parameters needed.

p 5681, l 26: "The biogeochemical model of CNRM-ESM1..." -> The ocean biogeochemical model... or similar

p 5682, l 4: "Dependence of growth to..." -> Dependence of growth on

p 5682, l 12: into -> in

p 5682, l 18-19: either "following the formulation of Geider et al..." or remove "formulation".

p 5683, l 12: What is "Princeton atmospheric forcing"? Please provide a reference here.

p 5687, l 26: detailed in (Voldoire et al., 2013) -> detailed in Voldoire et al., 2013

p 5688, l 8: acronym GPCP should be explained

Section 4.2.3: CNRM-CM5-2 is used while CNRM-CM5.2 is used in figure caption. Please use consistent names.

p 5694, l 6: "Figure 10 shows that the amplitude of annual mean GPP as..." Fig 10 shows the annual mean, I suggest deleting "the amplitude of".

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p 5694, l 9: "...patterns of high GPP (values)." Please add values or remove the "(values)"

p 5694, l 18: "...Princeton university forcings (REF)..." Yes, please add a reference.

p 5700, l 5: "... uptake of CO2 of about 2.1 and 1.7 Pg C $y{-}1$ for land and ocean..." Which year do these numbers correspond to? Is it 2005, or a mean over the last decade?

Figure 6d: The scale could be narrower (perhaps -2 to 2 psu?), since larger biases seem to occur only at the surface and the surface bias is already depicted in 6b. As it is now it is difficult to see the structures of the salinity biases at depth.

Figure 9a: In the figure legend it says PAR is given in the figure caption it says RSDS. Please check and correct.

Figure 13: This figure is really difficult to read, since the different sizes of the symbols are difficult to distinguish. I think it would be much easier for the reader to have one panel per depth with all 4 tracers.

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