Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-212-RC1, 2017

Geoscientific Model Development Discussions

Interactive comment on "The sea ice model component of HadGEM3-GC3.1" *by* Jeff K. Ridley et al.

Anonymous Referee #1

Received and published: 17 October 2017

The manuscript describes the sea-ice component GSI8.1 of the Met Office coupled configuration HadGEM3-GC3 and its evaluation against PIOMASS reanalysis and Cryosat satellite measurements. This will certainly be a reference paper for CMIP6 simulations and therefore I recommend its publication but with some substantial modifications.

General comments:

1) It is very unclear what are the novelties in this version of the sea-ice component (GSI8) compared to the one used for CMIP5 (GSI6). I recommend to explain it in the introduction or at the beginning of section 2.

2) It would be valuable to know why the most recent developments in CICE are not

included in GSI8, especially those which have been available for a couple of years. I am thinking of Delta-Eddington scheme, more elaborate melt ponds (which maybe necessitates the use of D-E), EAP rheology, mushy layers etc. Is it a question of timing, performance, robustness (testing)?

3) Besides novelties that need to be explained in details, I think the authors should not forget to give information about the rest of the ice code. Is there an interactive salinity, a fixed profile or a constant salinity? What scheme is used for advection (remapping)? And ridging/rafting? Do they consider biogeochemistry in the ice?

4) Description of JULES coupling lacks clarity but I come back to it in the "specific comments" section

5) Did the authors evaluate conservation of mass, salt and heat in the system? What are the leaks (if any) and where do they come from?

6) What is the performance of GC3 compared to GC2 in terms of CPU since new physics have been added? How much slower it is?

Specific comments:

— 2.1 albedo —

l.6: I do not understand why penetration of radiation is not included. Is it related to JULES? If yes, the authors should say it.

l. 12-15: I understand that the impact of melt ponds on albedo is linear with pond depth in the range [4 mm - 20 cm]. I would have thought more about an exponential dependency as in Lecomte et al. (2011, 2015). Is there a reference for the linear dependency?

l. 23: I suppose the value of Tc is -1degC as in Hunke et al. (2015), then refers to dt_snow_cice in the namelist? Maybe it should be stated more clearly. More generally it is sometimes difficult to relate the namelist variables with the text.

l. 30: What does Hsnowpatch represent? Snow blowing by winds? But then, where does this value (2 cm?) come from?

— 2.2 Thermodynamics —

I find that the JULES interface is poorly described here.

For example, it is said in the text that the ice model passes to JULES the top layer temperature, thickness and conductivity. Well it would be clearer to state all the variables that are exchanged: ice concentration, ice and snow thicknesses, temperature and conductivity of the first layer, concentration and thickness of ponds (I think that's all?)

Also, the authors should say that with JULES there is no transmitted radiation in and through the ice (from what I understood). What consequences do the authors expect on ice temperature or ocean heat budget?

Moreover, the reader must understand why JULES has been chosen (in just a couple sentences).

l. 6: precise that heat capacity depends on temperature and salinity.

l. 8: Fig 1 is not properly described (neither in the text nor in the caption). "Old" means the formulation used in GSI6?

— 2.2.1 semi-implicit coupling —

p.4, l. 10-18: this part is kind of unclear. I would suggest to rewrite it in a simpler way like: Problem to solve = a) non convergence of the temperature solver, b) coupling not physical. Solution = sea-ice fraction passed to the atm. which allows a semi-implicit calculation of the splitting of Fcond onto the ocean grid cells. This flux is then multiplied by ice fraction in each cell. . .

p.5, l. 11: "1000Hi". What does this threshold mean?

p.5, l. 12: Is there any quantification of this "surplus conductive flux"?

I guess there is still a small heat leak due to the non-perfect convergence of the iterative procedure. I think it would be valuable to diagnose it. In Hunke et al. (2015), it is said that this flux is about 0.01 W/m2. Does this value still hold? What does it mean in terms of global heat leak?

— 3. model evaluation —

p.6 l. 27: "this being thin ice that microwave. . .able detect". I do not understand this sentence.

p.7 l. 3-5: Do the authors mean that changes in Arctic sea-ice can be solely attributed to changes in the sea-ice component (contrary to the Antarctic)? I am not convinced that identical seasonal cycles indicate that forcings are unchanged. The mean forcing could also change while keeping the same annual cycle. Am I wrong?

p.7 l. 9: Concerning the great improvement in Antarctic sea ice (at least in summer), I understand it is solely due to the reduction of the warm bias in the ocean but does the atmosphere play any role here?

— References —

Lecomte, O., Fichefet, T., Vancoppenolle, M., & Nicolaus, M. (2011). A new snow thermodynamic scheme for large-scale sea-ice models. Annals of Glaciology, 52(57), 337-346.

Lecomte, O., Fichefet, T., Flocco, D., Schroeder, D., & Vancoppenolle, M. (2015). Interactions between wind-blown snow redistribution and melt ponds in a coupled ocean–sea ice model. Ocean Modelling, 87, 67-80.

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