Geosci. Model Dev. Discuss., 8, C233–C237, 2015
www.geosci-model-dev-discuss.net/8/C233/2015/

Geoscientific Model Development Discussions
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Interactive comment on "Development of Global Sea Ice 6.0 CICE configuration for the Met Office Global Coupled Model" *by* J. G. L. Rae et al.

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Received and published: 25 March 2015

General comments

The paper describes the new sea ice configuration GSI6.0 of the Met Office Unified Model. Improvements compared to the previous configuration are discussed and still existing weaknesses are addressed. While the simulation of Arctic sea ice has clearly improved as compared to the previous configuration, Antarctic sea ice has worsened in the new configuration. This disadvantage is somewhat disappointing, in particular since it is attributed to the higher resolution of the ice-ocean model. Thus, the additional computational costs appear to be unjustified (at least for the Southern Hemisphere), and I am wondering whether this configuration is actually intended for

operational use (including CMIP6 simulations) or only an interim solution towards a further improved configuration.

On the other hand, the improvements in the simulation of Arctic sea ice are so substantial that the new configuration can be regarded as a major step forward. The changes in the model, which led to the improvements, are readily comprehensible: A set of model parameters were changed to counteract too strong ice loss in the previous configuration. However, these changes were not made on the basis of new measurements or advanced theoretical approaches; they rather represent some kind of model tuning. More substantial changes of the model are postponed to future configurations; but that is acceptable because such enhancements are complex and require a lot of time.

The description of the new configuration is concise and precise and represents a well-balanced mixture of scientific paper and technical manual. From this point of view, the paper is more than acceptable and might be a useful reference for potential users of the model as well as for other model developers.

Specific comments

(The following comments refer to the page and line numbers in the PDF version of the discussion paper, available via doi:10.5194/gmdd-8-2529-2015.)

(1) Page 2531, lines 11–14: Although the definition of g as a dimensionless function is a correct citation of the paper of Thorndike et al. (1975), I would argue that the statement is incorrect. If g were a dimensionless function, the product $g(h)\mathrm{d}h$ would get the unit of a length. Assuming that $g(h)\mathrm{d}h$ should actually be a fraction of ice, and given that a fraction is dimensionless by definition, $g(h)$ must have the unit of a reciprocal length. In that case, $g(h)$ can be interpreted as the probability density function that describes the relative probability for the existence of ice with thickness h. The probability itself (that is the fraction) is then given by the integral of $g(h)$ over $\mathrm{d}h$.

This would be consistent and would make sense.

Further, I would not say that g is described by Eq. (1), but that Eq. (1) is the governing equation which describes the evolution of g.

(2) Page 2532, lines 15–18: Does it also mean that the atmospheric surface heat fluxes calculated by JULES are the same for each of the five ice thickness categories? Wouldn't it be another simplification worth being mentioned at this point?

(3) Page 2533, line 2: It would be interesting to know how the fraction of the gridbox area that is covered by snow is determined in the model. The distinction between snow and ice might be just as important as the values chosen for the respective albedos and threshold temperatures.

(4) Page 2534, lines 5–9: To my mind, it is not necessary to discuss the enthalpy in the context of the new sea ice configuration. It is rather confusing than helpful. I would cut out these three sentences.

(5) Page 2534, lines 22–24: Even without any ridging, the ice area should never be able to exceed the grid-cell area, especially not in case of convergence. Maybe it is meant that the ice does not cover the entire grid cell. This should be clarified.

(6) Page 2535, lines 25–26: The physical argument for increasing the roughness lengths remains unclear. The chosen values seem to me higher than corresponding values derived from measurements and boundary layer theory. Is there any specific reason for this extreme increase, other than the sensitivity study of Rae et al. (2014)?

Note: More sophisticated parameterizations for the turbulent exchange over sea ice have recently been developed or are still in development (e.g. Lüpkes and Gryanik, 2015, doi:10.1002/2014JD022418). This could be a consideration for future configurations as well.

(7) Page 2537, line 14: It is unclear to me why increased conductivities lead to reduced basal melt in July and August. The conductive heat flux is negligible during the melting

period due to the small temperature difference between top and bottom of the ice. I think this particular conclusion should be explained.

Technical corrections

(8) Since the sea ice configuration described in the paper is definite, I would suggest adding the definite article 'the' in the title: Development of **the** Global Sea Ice 6.0 CICE configuration for the Met Office Global Coupled Model.

(9) Page 2530, line 23: Williams et al. was published in '2015' instead of '2014'. The same on page 2538, line 11.

(10) Page 2531, line 1: 'GloSea5' instead of 'GloSea4'.

(11) Page 2532, line 1: The title of the section is 'Horizontal, temporal and vertical discretisation', but nothing is said about the temporal discretisation. The word 'temporal' could be removed.

(12) Page 2532, line 24: 'parametrisation' versus 'parameterisation' in the next line and in other places.

(13) Page 2532, line 27: '(The HadGEM2 Development Team, 2011)' instead of '(HadGEM2 Development Team et al., 2011)'. This team already comprises all authors ('et al.' is redundant).

(14) Page 2533, line 14: 'Semtner (1976)' instead of 'Semtner (1987)'.

(15) Page 2533, line 17: The symbol f, which is introduced here as the fraction of incident radiation which penetrates the ice pack, has already been used for the rate of change of ice thickness due to thermodynamic growth and melt (page 2531). One of these fs should be replaced by a different symbol.

(16) Page 2535, line 25: ‘GSI6.0’ instead of ‘GSI6’.

(17) Page 2535, line 27: `calc_Tsfc` does not appear in any of the CICE namelists in Appendix A. Or is `calc_Tsfc=.false.` the default in CICE?

(18) Page 2536, line 5: ‘($-1.8\,^{\circ}$C)’ instead of ‘($1.8\,^{\circ}$C)’. A positive freezing temperature of sea water makes no sense.

(19) Page 2536, line 10: ‘preprocessor keys’ instead of ‘cpp keys’. It should make no difference whether using cpp or any other preprocessor.

(20) Page 2536, lines 11–12: A reference to Appendix A of similar type has already been given on page 2535. One of them could be dropped.

(21) Page 2538, line 3: ‘Labrador Sea’ instead of ‘Labrador sea’.

(22) Page 2538, line 12: ‘austral’ instead of ‘Austral’.

(23) Page 2546, lines 11–12: Megann et al. was published in GMD in 2014. The reference to the GMDD version of 2013 is valid but outdated.

(24) There is quite a number of papers in the **References** which are never cited in the discussion paper. These redundant references should be removed.

(25) Page 2550: In the caption of Table 2: ‘GC2.0-GSI6.0’ instead of ‘GC1.0-GSI6.0’.

(26) In the captions of Table 3 and Figures 2 and 3: Information on the time period of the HadISST and PIOMAS data is missing. They are certainly not 50-year means.

(27) The font size in Figure 2 is really close to the lower limit. Maybe the figure can be replotted with a larger font.

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