

Responses to comments by reviewer Wolfgang Dorn

We thank Dr. Dorn for his comments, and give our responses in detail below.

General comments:

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.

This configuration will not be used for CMIP6 simulations. The Met Office's CMIP6 simulations will be performed with coupled model configurations using the next GSI configuration, which will include the enhancements described in the last paragraph of Section 5 of the present paper.

Specific comments:

(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 $g(h)dh$ would get the unit of a length. Assuming that $g(h)dh$ 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 dh . This would be consistent and would make sense.

We agree with this, and have changed “a dimensionless function” to “a probability density function”.

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 .

We have changed “ g is described by ...” to “The evolution of $g(h)$ with time is described by the governing equation...”.

(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?

These sentences refer to the ice *layers* in the CICE model, and *not* to the *thickness categories*.

However, as mentioned in the conclusions (Section 5), the turbulent fluxes in JULES are indeed calculated as gridbox means (a simplification that will be removed in the next configuration, GSI7.0). We did not state explicitly in the conclusions that the conductive fluxes are calculated separately on each category. We agree with the reviewer that this should be mentioned earlier in the paper than the conclusions section, but think that this would be more appropriate in Section 2.2 (Thermodynamics) than in Section 2.1 (Horizontal and vertical resolution). We have therefore added a paragraph explaining this at the beginning of Section 2.2.

(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.

Our original statement (that the albedos are weighted by the snow-covered fraction of the gridbox) was incorrect. In fact, the total albedo is a weighted combination of the ice and snow albedos, calculated via the “UKMO GCM” parametrisation of Essery et al. (1999) – see the second equation from the top of the right-hand column on p584 of that paper (the equations are not numbered). We have now corrected this error in our paper.

(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.

We have now cut these sentences, as suggested by the reviewer.

(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.

We agree that this sentence is confusing. What we meant was that if, for example, the area of the grid-cell is A_{GC} , and the area of ice in the grid-cell is A_{ice} , and ice covering area A_{adv} is advected into the grid-cell, where $A_{ice} + A_{adv} > A_{GC}$, then ridging will prevent the (physically impossible) situation arising where the area of ice in the grid-cell is greater than the area of the grid-cell itself. However, as this sentence detracts from the explanation rather than adding to it, we have deleted it.

(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)?

The roughness lengths quoted here account for form-drag in the MIZ, estimates for which lead to effective roughness lengths of 2 to 4cm. Our value of 10cm is probably above the 3 sigma on the above, but was adopted from a 1970's field observation (original citation lost). However, its adoption in the NWP configuration of the Met Office model leads to an improvement in the simulation of MSLP over that associated with lower (more realistic) values.

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. Indeed, the belated attention to this topic, since the seminal work by Steiner et al. (1998), has led to a number of such formulations. We shall be adapting that of Tsamados et al. (2014) in our next GSI configuration.

(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.

This was explained in Rae et al. (2014), to which we refer in the section concerned. However, for completeness, we have now added the following explanation in the current paper: “The net melting or growth of Arctic sea ice is the residual of the energy balance, and is extremely sensitive to small changes in the fluxes at the top and bottom of the ice pack (Keen et al., 2013). Rae et al. (2014) found that increased ice and snow conductivities cause an increased upward conductive heat flux through the ice pack in late summer and early autumn, leading to subtle shifts in the energy budget within the ice pack. This results in reduced basal melt in July and August, and increased basal growth in winter, leading to increased thickness, extent and volume.”

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.

Now changed.

(9) Page 2530, line 23: Williams et al. was published in ‘2015’ instead of ‘2014’.

Now corrected.

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

Now corrected.

(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.

Now done.

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

Now corrected. We are now using 'parametrisation' everywhere.

(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).

Now corrected.

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

Now corrected.

(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 f s should be replaced by a different symbol.

We have now replaced f for rate of change of ice thickness on p2531 with Φ .

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

Now changed.

(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?

`calc_Tsfc=.false.` had been omitted from Appendix A in error. We have now included it. We have also deleted `year_init`, `ocn_data_dir`, and `oceanmixed_file` from Appendix A, as they are not relevant for our setup.

(18) Page 2536, line 5: ‘(−1.8 ° C)’ instead of ‘(1.8 ° C)’. A positive freezing temperature of sea water makes no sense.

Now corrected.

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

Now changed.

(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.

We have removed the second reference to Appendix A, and retained the first one.

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

Now corrected.

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

Now corrected.

(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.

We have now updated this to reference the GMD paper.

(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.

Several references had indeed survived from an earlier draft of the paper. We have now removed the following from the reference list: Andreas et al. (2010), Calonne et al. (2011), Curry et al. (2001), Dorn et al. (2007), Kim et al. (2006), Lewis (1967), Maykut & Untersteiner (1971), Maykut & McPhee (1995), Miller et al. (2006), Miller et al. (2007), Nakawo & Sinha (1981), Notz & Worster (2009), Pirazzini (2008), Pringle et al. (2006), Pringle et al. (2007), Schwarzacher (1959), Sturm et al. (1997), Uotila et al. (2012), Vancoppenolle et al. (2005), Vancoppenolle et al. (2009), and Wettlaufer (1991). We have not removed the reference to Keen et al. (2013), as we do now cite it as a result of one of the reviewer's other comments.

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

Now corrected.

(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.

The HadISST and PIOMAS data are means for the period 1995-2004. We have now included this information in the captions of Table 3 and Figures 2 and 3.

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

We have now increased the font size in this figure.

References:

Essery, R., Martin, E., Douville, H., Fernández, A., and Brun, E.: A comparison of four snow models using observations from an alpine site. *Clim.Dyn.* 15, 583—593, 1999.

Keen, A.B., Hewitt, H.T., Ridley, J.K.: A case study of a modelled episode of low Arctic sea ice. *Clim. Dyn.*, 41, 5—6, 1229—1244, doi:10.1007/s00382-013-1679-y, 2013.

Rae, J.G.L., Hewitt, H.T., Keen, A.B., Ridley, J.K., Edwards, J.M., and Harris, C.M.: A sensitivity study of the sea ice simulation in HadGEM3, *Ocean Modell.*, 74, 60—76, doi:10.1016/j.ocemod.20, 2014.

Steiner, N., Harder, M., and Lemke, P.: Sea-ice roughness and drag coefficients in a dynamic-thermodynamic sea-ice model for the Arctic. *Tellus A*, 51, 5, 964-978, doi:10.1034/j.1600-0870.1999.00029.x, 1998.

Tsamados, M., Feltham, D.L., Schröder, D., Flocco, D., Farrell, S.L., Kurtz, N., Laxon, S.W., and Bacon, S.: Impact of Variable Atmospheric and Oceanic Form Drag on Simulations of Arctic Sea Ice. *J.Phys.Oceanog.* 44, 5, 1329-1353, doi:10.1175/JPO-D-13-0215.1, 2014.