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Geoscientific Model Development Discussions

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

Anonymous Referee #2

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The authors describe the new sea ice model component of the climate model HadGEM3-GC3.1. As this model will be applied in a large number of experiments, this description is an important source of information for the community and will be very helpful for the users of the outputs of HadGEM3-GC3.1. The paper is short but focused on the important points. However, I would personally have been interested in a comparison of the choices performed here with the ones made in other state-of-the-art models, not just in the previous or other versions of the same model. This would have provided a wider perspective of the paper.

For me, the only point that absolutely requires additional discussion is the coupling and how it interacts with the ice thickness distribution scheme. If I understand well (page 3, lines 11-15), the surface temperature of the sea ice or snow and surface melt are computed by the land surface module that transfers to the sea ice model a conduction flux which is used as boundary condition for the sea-ice model. This is described in West et al. (2016) but a few additional explanations would be helpful here in the revised version. The distribution of the conductive flux between the grid cells is described in section 2.2.1. However, I was not able to understand from the information given in the manuscript the method applied to distribute this conductive flux among ice thickness categories. It is also not clear to me if the surface melt is the same for all the categories. The thickness is mentioned page 5, lines 11-13 but it is to solve a specific problem of instability, not related to the ice thickness distribution. It is expected that the melting and conduction fluxes are very different between the different categories as they should be strongly related to the thickness. If in the proposed implementation, the same conductive flux is given for all categories, my interpretation is that nearly all the interest of the ice thickness distribution scheme is lost. If this is the case, this should be clearly mentioned as a strong limitation of the approach. If I have misunderstood something, the authors should explain their arguments justifying their choices and how this take advantage of the ice thickness distribution scheme.

I have also some very minor remarks. 1/ Does the atmospheric component have a specific name (as the ocean or surface ones) ? 2/ Page 11, line 11. This would be useful to list the 'GC3.1 ocean-atmosphere variables'. 3/ Page 6, lines 28-29. Here and for the other points mentioned in the manuscript, this would be interesting to discuss briefly the causes of the differences of the results of GC2 and GC3.1. 4/ Page 7, lines 4-5. I do not understand why similar annual cycles but with an offset indicate similar oceanic and atmospheric forcing. I guess a change in oceanic or atmospheric forcing could also be the origin of the offset. 5/ Page 7, line 5. A comparison with the observed ice extent in the Southern Ocean should also be provided. Figure 3. The months selected should be given in the figure caption.

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