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The Cryosphere Discussions

Interactive comment on "Model resolution influence on simulated sea ice decline" *by* J. O. Sewall

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I agree with the spirit of the conclusions and analysis of this paper but I have a few technical suggestions and thoughts about the conclusions.

I strongly recommend that the analysis of ocean heat transport be redone for the heat transport across a single latitude (hence the ocean heat flux convergence into the Arctic), rather than the average heat flux over the Arctic, because the energy budget depends on the ocean heat flux convergence (OHFC), not the heat flux per se.

I am concerned about how few data points are in Fig 4. I have found CMIP3 archived ocean heat flux for 13 models for the A1B scenario. I see no advantage to investigate the 4XCO2 run if so few models provided ocean heat flux for

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this scenario. Why was this scenario used anyway. I have a relevant figure at http://www.atmos.washington.edu/~bitz/ohfc_versus_ice.png with the change in OHFC at 70N at 2050 relative to 1990 in the A1B scenario for 13 models. I have shown this figure in talks but never published it and I am pleased that the author wishes to publish such a figure here. The bottom panel correlates the OHFC change with the April sea ice extent change in the models. The correlation coefficent is 0.73 .

It never occurred to me that the OHFC change depended on the resolution. I would be more convinced of the result, if more than 6 models were used to show it. At the very least, the statistical significance of the correlation coefficient should be assessed.

I have a paper that the author may wish to consider where I argue that the OHFC change is coupled to the ice retreat in a positive feedback. I show that as the sea ice retreats off the Siberian Shelf, the amount of brine rejection increases in winter. This in turn increases mixing and drives sinking off the shelf, and the sinking increases heat transport into the Arctic. This is published in Bitz et al 2006 in J. Climate. I think my results might explain why the ocean model resolution is important because higher resolution models are more likely to simulate sinking off the shelf.

There are two models that submitted A1B scenarios at two resolutions. It would be useful to know if the sensitivity to resolution holds up in these pairs of experiments.

I think it is impossible to know whether ice model physics influences the model sensitivity. There are far too many degrees of freedom in the sea ice models to know if any one is critical to senstivity when only 14 models are available. A better way to investigate the role of ice physics is to vary ice model physics in a single model and compare the results. This has been done by Holland and Vavrus in separate papers in about 1998. The claim in the abstract line 14 and the conclusions should be changed to indicate that the study can conclude nothing about the relationship between sea ice model physics and ice response. The text in section 3 line 4 should be modified as well.

TCD

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Interactive Comment

I am confused about the information in Table 1. Does the ice thickness category include open water? If the number is 2, does the model has leads and a slab of ice in each grid cell? If so, then this should be explained in the caption or text. I'd be surprised if the CCCma model didn't have leads. And how can a model have zero categories? The models with 5 categories, have 5 plus open water. I think the GFDL thickness distribution model is prescribed, so the relative fractions of the categories are fixed. This is very different in my mind from the prognostic ITD models of CCSM and HadGEM. These are some of the reasons why it is difficult to draw conclusions about model physics and sensitivity.

Re Fig 1. There are not 10^{14} grid points in any of these models. For example, CCSM3's ocean model has 384x320x40=5 million grid points which is 6 orders of magnitude fewer than indicated in Fig 1! I suspect that this plot is really of the number of horizontal grid points. And the exponent is suppose to be a 4 and not a 14.

The author should verify that stand-alone CICE uses the same albedo parameterization at the two resolutions. Normally CCSM3 uses different albedo parameters at the two resolutions, and CICE may have adopted such methods from CCSM.

The stand-alone experiments are a nice component of this study, but the author should be careful not to draw too much from them. They are uncoupled, so they have a limited ability to exhibit ice-albedo feedback and other feedbacks that are necessarily coupled.

It is unclear why only a 20 yr avg is used for 2 of the 14 models. Presumably this is because these models did not cap CO2 at 4X but continued to ramp. This should be stated. CCSM3 does have a branch from b30.026.ES01 that capped at 4XCO2. Using it which would make for a cleaner comparison.

The title of the paper is too broad. It should indicate that only the ocean resolution is at issue.

Interactive comment on The Cryosphere Discuss., 2, 759, 2008.