Earth Syst. Dynam. Discuss., 6, C956–C959, 2015
www.earth-syst-dynam-discuss.net/6/C956/2015/

Earth System Dynamics
Open Access
Discussions

ESDD
6, C956–C959, 2015

Interactive Comment

Interactive comment on "Importance of open-water ice growth and ice concentration evolution: a study based on FESOM-ECHAM6" *by* X. Shi and G. Lohmann

Anonymous Referee #2

Received and published: 17 December 2015

Shi and Lohmann examine the sensitivity of the FESOM-ECHAM6 GCM to changes in the value of a sea ice parameter. Specifically, when the ocean reaches the freezing point in the GCM and ice volume forms, the ice is assigned a thickness so that it is distributed into only a fraction of the grid box area. This formulation comes from Hibler (1979), who adopted a thickness of new ice of h0=50cm. The FESOM-ECHAM6 model instead uses a parameterization where h0 can be anywhere in the range 50cm to 150cm (their eq. 3). The authors examine what happens when they increase h0 in the GCM by a factor of (1/0.8). They also examine analogous simulations in a "Simple Idealized Model" (SIM).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

I found the simulation to be potentially interesting, but the analysis and discussion did not make sufficiently clear what new understanding can be gleaned from it. As discussed below, obvious expected responses were not clearly separated from more novel results, and I found the physical interpretation in some places to be unconvincing.

(1) FESOM-ECHAM6 is a comprehensive GCM, but its treatment of sea ice processes appears to be fairly rudimentary compared to the current state of the art (e.g., CICE 5), including the representation of the evolution of the ice thickness distribution (following Hibler rather than a newer method such as linear remapping) and ice thermodynamic processes (using a simple zero-layer model rather than a model with multiple thermodynamic layers and treatment of changes in the brine pocket volume). It would be helpful for the paper to include a discussion of these potential deficiencies in the ice component of the GCM and whether or how they are expected to influence the results.

(2) It was unclear to me whether the SIM includes an albedo feedback. In eq. 7, it appears that the albedo value of multiyear ice is used even when the ice concentration reaches zero. Note that parts of the year with zero ice concentration occur in all the runs that were examined in this study (Fig. 15). If the SIM does assign a different albedo to open water, then this should be clearly specified. If the SIM does not, then this should be justified, especially in light of the point that the SIM is used for comparison with the GCM which of course does have a surface albedo that depends on the presence of sea ice.

(3) The main features of Figs. 5-6 appear to match standard expectations. Since the growth rate of ice during wintertime is faster if the ice is thinner and fastest in locations of open water, increasing h0 causes there to be more open water and hence thicker ice. This standard expectation does not appear to be discussed directly in the paper, although the discussion on lines 1-5 of p. 2150 seems related to this. It would be helpful to add a discussion of this point, for example based on Bitz and Roe (J. Climate, 17, 3623-3632) or references therein. One purpose of such a discussion would be to help distinguish what is novel in these simulation results from what matches standard

ESDD

6, C956–C959, 2015

expectations within the field.

(4) I found Fig. 9 to be less readily interpretable and the discussion of it on lines 6-14 of p. 2149 to be somewhat unconvincing. Is the plotted quantity (i.e., the "annual mean thermodynamic growth rate") the average value of $\dot{h}_{\mathrm{ice}}$, or is it the average only considering times when $\dot{h}_{\mathrm{ice}} > 0$? If the former, in steady-state this field must be balanced by the annual-mean horizontal ice advection, and hence it is closely related to the ice motion field in addition to thermodynamic processes (and specifically to the equilibrium that forms between these two).

(5) How do the relevant differences between the two simulations compare with the level of internal variability in the model? Specifically, it would be useful to know how the results in Fig. 7 and 11 compare with the amplitude of variability from one 50-year period to the next in a control simulation (or in the spinup simulation, if it is sufficiently spun up for an extended period such that this can be examined). A striking feature of Fig. 7a is the large signal in the tropical Pacific. If this is truly a response to the difference in h0, then it is noteworthy (and relevant to recent discussions of tropics-Arctic teleconnections). If this is more likely related to internal variability in the model, then it should be identified as such.

(6) I found the mechanism proposed in Fig. 13 to be plausible but unconvincing. Is this to be taken as a speculative mechanism inspired by the model results or as a mechanism which has been robustly found to be occurring in the model? If the former, it should be specified as such. If the latter, further analysis would be useful. One key point here would be to show that the AMOC weakening really is substantially larger than the GCM's internal variability, such that the AMOC weakening can be expected to confidently be a response to the change in h0. If within the scope of the paper, showing that this positive feedback occurs during the internal variability of the GCM (which seems like it should be expected to happen if the mechanism is accurate) would be a helpful addition to convince the reader that the proposed mechanism really is occurring in the model.

(7) It would be useful if more details were given regarding the GCM simulations. It says there is a 300-year model spinup before the start of FE-CTR and FE80. Is this spinup using identical parameters as in FE-CTR? 300 years seems rather short for a GCM spinup. What were the initial conditions for the 300-year run? Is the extent to which the model has equilibrated during these 300 years discussed elsewhere? If so, this should be cited. If not, it would be nice to include discussion of this here.

Minor typo: On line 3 of p. 2139, was it meant to say “insulating” rather than “isolating”?

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2137, 2015.

ESDD

6, C956–C959, 2015

Interactive Comment