

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

M. Crucifix (Editor)

michel.crucifix@uclouvain.be

Received and published: 8 January 2016

First of all I would like to thank the authors and reviewers for their contribution to Earth System Dynamics.

The present article describes one sensitivity experiment with an ocean-atmosphere-sea-ice model of high resolution, complemented by five sensitivity experiments with a simple 1-D thermodynamic sea-ice model with an ocean mixed layer. The authors conclude by stating that "the results presented in this study emphasise the importance of open-water ice growth. In detail, the distributing of new ice volume between growth in area and thickness can affect the ice properties, the ocean circulation, and even the

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climate system in our climate model FESOM-ECHAM6"

Earth System Dynamics aims at publishing articles of wide interdisciplinary significance. Consequently, an article focusing on the parameterisation of a specific physical process has to make a case for the novelty and potential implications of its findings. The exchange of correspondence between the reviewers and the authors, as well as the confidential reviewer reports, suggests that the results reported here are broadly consistent with earlier findings, and that the methodological and scientific advances may not be compelling enough to warrant publication.

In particular, both reviewers have requested more evidence that the climatic effects of the parameterisation change are significant. The authors responded by showing plots where the regions of "significant changes" are hatched. The authors term "significant" variations, in the 50-year average, that exceed "one standard deviation" of the corresponding variable. The statement gives some lee-way for interpretation. Let us assume for a moment that the authors speak of the standard deviation of the annual time series. It should in this case be noted that the expected ratio between the standard deviation of a 50-year mean and that of interannual values depends on the auto-correlation of these variables. Hence, considering as "significant" anything beyond one interannual standard deviation may be in some cases reasonable but in other cases not so. If what the authors have termed "standard deviation" is in fact the expected variation from one 50-year period to the next, then, clearly, a difference of one standard deviation is not enough to reject the null hypothesis. Hence, whichever way the 'standard deviation' was computed, more critical analysis is needed.

I am particularly concerned with the reported effects of the parameterisation change on the mean overturning circulation of the Atlantic Ocean (AMOC). On Figure R8 (response to reviewer #2), the authors compare time series obtained with and without the parameterisation change. They claim in their response to reviewer #2 that the anomaly of the AMOC is "significant and robust" but this statement is arguable. The sequence shown with FE80, between years 300 and 350, resembles the sequence without pa-

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parameterisation change, between years 220 and 260. Therefore, we cannot be sure that a state-perturbation of the system, without parameter change, would not have had similar effects as done shown here.

Other concerns expressed by the reviewers pertain the fairly rudimentary treatment of sea ice processes compared to the modern state-of-the-art. Reviewer #1 observed that the findings may depend on the complexity of thickness distribution in any given model. The authors have suggested to modify the text by introducing caveats and they provided a qualitative argument that "a modern sea ice model with multi ice-thickness categories will probably lead to similar results".

Finally, reviewer #2 worried about attribution of previously published work (this concerns the equations defining the simple model).

I would therefore encourage the authors to revise thoroughly their manuscript. I am afraid that final acceptance in Earth System Dynamics is not warranted because the case for interdisciplinary significance will be hard to make. The authors may therefore want to consider an other option, that is, submit the revised version to a journal that will accept work focused specifically on parameterisations such as Geophysical Model Development. In this process, the authors may also want to consider different options for actually shortening the article, for example by referring to previously published material where possible. If they chose this options, I would also encourage the authors to inform the editor in charge of this new submission of the present open review process in order to avoid to repeat unnecessary reviewing work. This rule of good practice is unfortunately too often ignored.

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