

## ***Interactive comment on “A high-resolution ocean and sea-ice modelling system for the Arctic and North Atlantic Oceans” by F. Dupont et al.***

### **Anonymous Referee #1**

Received and published: 12 February 2015

In this paper, the authors evaluate a new high resolution ocean/sea-ice model against observations. Such evaluation allows researchers to judge the quality of the model system in particular for future work which is why I in principle recommend publication of this paper.

However, before this paper is accepted, the following issues should be addressed:

1. Throughout this paper, it seems as if observations are seen as the truth which the model must match in order to be credible. However, all these observations have uncertainties, sometimes significant ones, which are not discussed. This needs to be addressed so that readers can understand if a certain mismatch between model and observation is primarily related to issues with the model or might simply be related to observational uncertainty. This includes a discussion of point measurements vs.

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grid-cell averages for some of the data used.

2. Throughout this paper, for any model-data mismatch there is too little discussion of possible error sources that are not related to the representation of physics in the model itself. Such error sources include internal variability, issues with the forcing, issues with the lateral boundary conditions, issues with spin up, etc. Without such discussion, it is again hard to judge how severe (or not) model-data mismatches are.

3. Throughout this paper, there is no discussion of the tuning of the model. Hence, it is not possible to judge if a certain mismatch (or a certain agreement) between model and data was achieved because a particular data set was used to tune the model or whether the agreement is indeed an achievement of the model. This holds in particular for the discussion of sea ice, where slightly different tuning of, say, surface albedo might change the ranking of the different model versions significantly.

4. Validation of an ocean/sea-ice model system is not possible, evaluation however is. The terminology should be changed throughout this manuscript. Compare Oreskes, Naomi, Kristin Shrader-Frechette, and Kenneth Belitz. "Verification, validation, and confirmation of numerical models in the earth sciences." *Science* 263.5147 (1994): 641-646.

Minor comments:

p.2, l.3: It sounds odd that the government of Canada is developing a model. Usually, one would assume that the government has other issues to deal with than climate-model development :-)

p.2, l.6: Is there a reason for using the judgemental term "ice infested"?

p.6, l.7: Do you mean "tuning" in the sense of parameter adjustments or in the sense of model development? The latter seems to be the case, but common usage of the term "tuning" implies the former.

p.6, l.20: If the surface layer is just 1 m thick, what happens when the ice thickness

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becomes larger than 1 m?

p.6, l.11: This seems to be a repetition of the information p.5, l.23

p.9, l.8: Can you provide a few more details on this approach?

p.9, l.23: What is the volume of observations?

p.13, l.17ff: Why is not the same data set used for both mean and fluctuations?

Section 3.2.3: T and S are of importance not least because they determine the density profile. Would be good to compare density in model and observations.

p.19, l.26: I expect that it is much harder to get the trend roughly right than the actual area (which can easily be adjusted by tuning). I hence disagree with the statement that H05 is better than H02 or ORCA12-T321 on this metric.

p.20, l.1: The trend is negative but not necessarily decreasing

section 3.3.2: Would be interesting to compare the seasonal cycle of obs. vs. model

Fig. 1: Is the coast line in the figure the model coast line or a plotting-program coast line? The former would be better.

Fig2ff: Would be helpful if always the model is shown first and then observations (or other way around), rather than sometimes showing model first and sometimes showing obs first.

Fig2ff: Labels of many figures are too small

Fig.7: It might be helpful to harmonise the total range of the individual subpanels to allow for a visual judgement of absolute mismatches.

Typos etc.

p.3, l.20: communities'

p.4, l.6 : no comma after period

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p.6, l.20: 450 m

p.6, l.28: dependence

p.7, l.16: This is commonly referred to as a 3-layer model (2 ice + 1 snow)

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Interactive comment on Geosci. Model Dev. Discuss., 8, 1, 2015.

**GMDD**

8, C32–C35, 2015

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