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Interactive Comment

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 #2

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Overview

In this article the authors introduce a new North Atlantic-Arctic ocean-sea ice modelling system and detail several different incremental test configurations. For each configuration a hindcast experiment is performed and these are assessed using some useful tools in order to ensure the model is fit for operational running.

I think that the documentation of this system and evaluation of the model is of interest to the scientific community and therefore recommend that this paper is published in GMD subject to the points below being addressed.

General comments

* In general I think that a bit more care is needed when describing the comparisons with observations. In particular it is often unclear exactly what is being compared with what (i.e. are we comparing the mean of the observed values with mean of model values, or are the model values interpolated to observation locations or what?). This is particularly true for Figures 7, 9, 10, 11 and 14.

* More explanation is needed in introducing the model experiments. In particular it is not clear how the multi-category ice fields are initialised in your H05 CICE run?

* The development of this model is clearly motivated by the need 'to provide Canada with short-term ice–ocean predictions and hazard warnings' which will presumably be done using an operational analysis-forecast system. However nothing is said about how this will be run. In particular data assimilation is mentioned and so is coupling to the Environment Canada's regional weather prediction system but will both these things be done together (i.e. are you planning to implement a fully coupled data assimilation system)?
I think that if there were a little more information in the Introduction and Conclusions sections about these plans then it would help the paper to highlight the paper's relevance.

* There are a number of instances of 'PSU' in the text and on the figures in relation to salinity which should be removed. There is no such thing as a Practical Salinity Unit (PSU) because, when measured on the practical salinity scale, salinity is simply a dimensionless ratio. Therefore you should give your salinity as numbers with no units. Strictly speaking you should simply state somewhere that "salinity is measured on the practical salinity scale" but one could argue that this is not really necessary these days

because everybody measures it this way(?).
UNESCO (1985) The international system of units (SI) in oceanography, UNESCO Technical Papers No. 45, IAPSO Pub. Sci. No. 32, Paris, France

* Finally I presume the journal language is English (not US English) in which case there are a few misspellings such as 'programs' and 'modeling' instead or 'programmes' and 'modelling'.

Specific comments

p5.l24-5: NEMO is not really "an ocean and ice model" it is much larger than that (inc. passive tracers, biology, etc.). NEMO contains an ice model called LIM but this isn't technically NEMO. Given this is under consideration for the NEMO Special Issue it might be worth ensuring this is correct? The NEMO book says: "The Nucleus for European Modelling of the Ocean (NEMO) is a framework of ocean related engines, namely OPA for the ocean dynamics and thermodynamics, LIM for the sea-ice dynamics and thermodynamics, TOP for the biogeochemistry (both transport (TRP) and sources minus sinks (LOBSTER, PISCES). It is intended to be a flexible tool for studying the ocean and its interactions with the other components of the earth climate system (atmosphere, sea-ice, biogeochemical tracers, ...) over a wide range of space and time scales."

p7.l4: I think it would clearer to include units for the viscosity (10^{-4}) even if they are the same as for the following diffusivity $(10^{-5} m^2 s^{-1})$

p7.l8-9: you say "hindcast H05 requires a decrease to 180 s after July 2007 to ensure

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stability in Dease Strait." Why is this? Was this expected or just a blow-up? The use of "requires" rather than "required" here implies that this was foreseen rather than reactive.

p8.l1-4: the coupling of NEMO and CICE within the Met Office's coupled model HadGEM3 is described by Hewitt et al. (2011) and within the ocean-ice FOAM system by Blockley et al. (2014) (although the latter mainly links back to the former). Can these not be cited instead (or as well) as the pers. comm. (see references below)?

Section 3.1: How are the multi-category CICE initial conditions produced for H03-5?

Section 3.2: Why is there no specific validation of SST? There is a large number of SST data (both in-situ and satellite) that would be useful to compare against the model. At the very least it would be informative to compare against L4 gridded data products such as OSTIA (also available through MyOcean).

p15.l10: Regarding surface circulation comparisons with drifters you say: "The general agreement is remarkable". I think that "remarkable" is perhaps a little strong here. The agreement is pretty good but it's difficult to make a "remarkable" visual comparison between a $1/12$ degree and a $1/2$ degree field. Perhaps the model output could be regridded to $1/2$ degree for a more direct comparison?

p16.l2: how does the number of data in your modified CORA3.4 data set compare with the ERA-CLIM funded 'EN4' data set of Good et al. (2013) (see references below)?

p17.18: be careful with the use of "significantly" here. Do you mean statistically

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significant? If not then “considerably” might be better. However I am confused as to why this should even be described as considerable given that just before (p17.l10) you describe the temperature biases as “very small (less than 0.5 deg. C)”?

p17.l27: “In order to investigate whether these recent variations are reproduced...”. It is not clear to me what the “recent variations” are. Could this sentence be reworded?

p18.l21: You say: “the temperature and salinity gradients across the strait are broadly similar.” I don’t think this is true. Certainly it looks like the temperature *d*ifference across the strait is the same but the gradient is not as the values are quite different in the middle of the strait where the model is cold-biased. This cold bias is mentioned later (p18.l25-6) but I think it should be mentioned sooner around l21.

p20.l1: “decreasing trend” is not necessarily true. Certainly the trend is downward but “decreasing trend” suggests that the gradient of the trend is negative! Additionally I am not sure that the gradients of these lines are that similar either. There is a general reduction in ice area in H02 and T321 but they don’t really capture the 2007 minima very well? Furthermore (and see comments for Figure 12) it looks like the CICE run H05 may be adversely affected by its initial conditions because it drops off pretty rapidly save for the increase in 2008/9. Do you think this model is still spinning up?

p20.l25-28: “The model ... tends to overestimate the thicker ice categories in the Beaufort Gyre and underestimate them near the North Pole.” The converse is also true (i.e. that the model underestimates thicker categories in Beaufort Gyre and overestimates them near pole). Should this be mentioned? How does this compare with the single-category LIM ice fields in H02? I suspect that it is much better but it should be mentioned (but not necessarily plotted). Are the results in Figure 15 consistent with Figure 14?

p21.l17-18: It might be worth mentioning that this over-estimation of volume with NEMO-LIM2 is fairly well known being consistent with the findings of Blockley et al. (2014) and Massonnet et al. (2011)

Section 3.2.2 (Figure 19):
I am not a fan of the use of "average bias" when talking about directional vector quantities such as ice velocity. The main reason for this is that it is difficult to interpret what a positive or negative bias actually means unless the underlying field is entirely uni-directional. For example a positive bias (say) could mean that your velocities are too strong in a eastward regime or too weak in an westward regime. Furthermore if the observations cover an area with ice moving in both directions then it's even more difficult to understand what a positive bias means and what the effect of (possible) compensating errors might be. Therefore I think this piece of text (the interpretation of Figure 19) needs some more careful explanation. Perhaps it might be better to try to understand the errors by using an RMS error time series in Figure 19 and then show the biases spatially? The ice drift maps in Figure 18 would be useful here if we knew where the in-situ observations actually were?

p21.l15: Re. comparisons with PIOMAS in Figure 17 you say "The seasonal cycle (Fig. 17, top panel) for H05 is very close to the PIOMAS value". Although the magnitudes do look very similar there does appear to be a "lag" in your time series whereby the onset of ice growth AND melt is slightly offset temporally. This is not mentioned in the text at all. Do you have any idea why this might be the case?

p22.l23: You say "due to Ekman transport acting of the ocean" which doesn't quite make sense. Do you mean "Ekman transport acting on the ocean" or something like

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“Ekman transport within the ocean”?

p25.l16-17: You say you are hoping to increase the ocean vertical resolution to 75 levels to put you “on par with DRAKKAR and Mercator-Océan’s latest standards”. Is this true? I thought Mercator’s vertical resolution was 50 levels not 75? It is certainly listed as 50 in Drillet et al. (2014) and Tonani et al. (2015).

Figures

Figure 4: please change “modeled” to “modelled”.

Figure 5: It is nice to see the high resolution data in the bottom plot. However the fact that one is 1/2 degree and the other 1/12 degree does make it hard to draw comparisons. Have you coarsened the 1/2 degree model output to 1/2 degree to compare directly? It might be nice to include another image here showing the regridded currents?

Figure 6: please remove “PSU” from salinity colourbars

Figure 7: It is unclear exactly what is being plotted here. For each of these boxes are you comparing the average of all observations with that of all the model points? Or are the model profiles collocated with the observations (either interpolated to obs locations or nearest grid cell)?
Please remove “PSU” from salinity axes.

Figure 8: What does the white missing data mean here?

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Figure 9: The grey shaded area is really not very visible when this paper is printed out (although ok looking on screen). I would recommend adding dashed/dotted lines at the max/min extents of the grey to emphasise it.
Also the differences between the black Proshutinsky et al. (2009) data set and your coloured lines are not explained. Yours looks very different from their with much more fluctuation. Is it simply a case of using a different temporal discretisation (i.e. monthly vs. yearly)? Either way this should be addressed.

Figure 10/11: Same question as Figure 7. How are the model-obs values calculated? Are you comparing means of point observations model means and if so how are they collocated?
Please remove “PSU” from salinity axes.

Figure 12: Your CICE/H05 experiment starts with a relatively poor representation of September Arctic ice area and drops off rapidly. Is this an artifact of the initial conditions? Do you think this model is still spinning up?
It would be interesting to know how the 10 ice categories were initialised in your H05 run.

Figure 14: It would be useful to explicitly state what “difference” means here (i.e. modelled-observed?)

Figure 15/16: As mentioned above this over-estimation of ice volume in LIM2 is well known (Massonnet et al. / Blockley et al.)
As mentioned above your H05 volume time series appears to have a time lag in it but this is not discussed.

Figure 18: What is the resolution of the NSIDC observational product? This is not mentioned in the text either.
How realistic is the circulation in the Beaufort Gyre in this NSIDC product? It doesn't look very pronounced (but this could be answered by the resolution of the product above).

Figure 19: As discussed for Section 3.2.2 above I think some more work is needed to understand the information in this figure.

Minor typos etc.

p2.l9: "model represent" should be "model represents" or "model represents"

p3.l22: "program" should be "programme" (unless it's a computer program)

p5.l15: "re-increasing" is not very good English and should be replaced

p6.l1: please remove "very" as "substantially" shouldn't need any further quantification

p15.l23: "myOcean (www.myOcean.eu)" should be "MyOcean (www.myocean.eu)"

p15.l26: "program" should be "programme"

p16.l2: "programs" should be "programmes"

p16.l3: "programs" should be "programmes" p16.l17: please remove “PSU”

p16.l20: please remove “PSU”

p17.l5: please remove “PSU”

p17.l6: please remove “PSU”

p18.l28: “maximums” should be “maxima”

p19.l21: “coefficicents” should be “coefficients”

p20.l5: “adjusement” should be “adjustment”

p22.l15: I don’t like winds being described as “large”. This should “high winds” or “strong winds” (or perhaps “large wind stresses”?).

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