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Geoscientific Model Development Discussions

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

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Responses to reviewer's comments

»> We thank the reviewers for their careful examination of the manuscript. We have responded to each of their comments and suggestions after the "»>" symbol. The new line numbering refers to the revised PDF produced with latexdiff where the text modifications are clearly marked.

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. grid-cell averages for some of the data used.

»> The reviewer is correct in noting the absence of discussion around uncertainties in observations for most datasets. A careful examination of the datasets used in the manuscript led the added descriptions of observational uncertainty in the revised text for SSH, T&S, velocity, ice concentration and thickness at lines 299, 349, 378, 429, 448, 472, 481, 510, 521 and 531. Because model evaluation is focused on broad scale statistics, uncertainty of point measurements should not affect the conclusions. However we have included a mention of the undersampling problem at lines 380, 449 and 511. Due to issues with melt pond detection in the sea ice concentration product used in this manuscript, we switch from total ice area comparisons to comparisons of total ice extent (section 3.3.1), which is a more robust metric.

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.

»> Impacts of error in model physics and numerics, or impacts due to improvement in these aspects, are clearly indicated by differences in various hindcast results. Lateral boundary condition and mixing parameterization follow common practice in ocean modelling, so we simply document the approaches being used. Model initial conditions are taken from either high-resolution reanalysis product or a global solution with known bias, so no special spin-up is performed. A discussion on impact of using different atmospheric forcing is added at line 623 when comparing differences between CREG12 and T321 results. We have added a description of the river forcing at line 253.

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.

»> The tuning of the model was kept to a minimum. In most respect, the same parameters used in ORCA12-T321 were used in the CREG12 experiments. In CICE, except for explicit parameters discussed in Section 2.1.5 related to the dynamics, the default parameters and physics were used. Text added at lines 190-200.

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.

»> Thanks for this precision. "Validation package" has been replaced by "verification package" and "validation" in general by "evaluation".

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 :-)

»> corrected at line 2.

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

»> The same expression was used in Lemieux et al. (2015), QJRMS. It is appropriate in terms of navigational safety, which is one of the priorities of the prediction system.

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.

»> Text is revised to use "improvement" instead of "tuning".

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

»> The "levitating ice" hypothesis applies throughout the paper (see also conclusions where reference is made to the opposite "embedded ice" hypothesis). In the "levitating ice" paradigm, the ice does not penetrate into the ocean but "floats" above, which has implications in terms of volume and water and salt exchanges. Text added at lines 152-157.

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

»> True, statement removed.

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

»> This is related to point p6l20 where we have explicited the "levitating ice" paradigm. See modified text at lines 152-157.

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

»> The question refers to the volume observations used in the CGRF forcing. The answer is unfortunately outside the expertise of the present authors and the scope of this contribution. We refer to Smith et al. (2014) and the references therein. For instance, further details can be found in Belair et al. (2006). We can grossly say that the World Meteorological Organization organizes a common operational data feed to all participating centres. The core of it includes thousands of full-depth radiosonding carried out twice a day, thousands of ground stations, some data from aircrafts and an increasing number of satellite derived information, mainly radiance at the top of the atmosphere which however runs typically nowadays in the million points a day but where is their infancy in 2006 and almost non-existent at the beginning date of the CGRFs (2002). The total volume would have run in the 10ˆ5 at the beginning of the CGRFs and close to 10ˆ6 by the end, excluding thinning of the data during their ingestion.

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

»> Satellite altimeter provides sea level anomalies (SLA). This yields the information for fluctuations in sea level. However, the information on the geoid is required in order to estimate the mean (the true neutral surface for the dynamics, the geoid, is not spherical; hence the SSH measured from the altimeter can not be used alone for the investigation of the mean). Hence the mean field is provided as a separate dataset, here the CNES-CLS09 (Rio et al., 2011) MDT (blended with the mean of the altimetry SLA over the study period). These geoid models are constantly refined as well which makes difficult their inclusion in the processing of the altimeter data. For the 3D ocean models such as the one presented in this manuscript, the geoid is considered flat, that is, the model is at rest when SSH is a constant everywhere. The information provided in Section 3.2.1 p.13 seems sufficient although a clear understanding of the present issue is a complex undertaking.

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.

»> Yes and no, density would be certainly of interest to determine the circulation (if considered a Lagrangian surface), but in general the analysis of model-obs of density will be redundant with T&S.

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.

»> While we agree on the general statement on the adjustment by tuning of the total ice area and on the difficulty of getting the trend correct, we disagree on the statement that our metric is not sufficient for a conclusion. Our conclusion is based on the improved seasonality and September ice extent (please note that we have switched from “ice area” to “ice extent” in the manuscript for better robustness). This sentence does not address the overall trend which is agreedly better in H02 (see next sentence p.20.l.1 for this). However, at least in terms of ice extent alone, Fig12b does show that in terms of September value, H05 performs better over the 2005-2009 period (hence excluding the spinup period and the pathological behaviour after 2009 also mentioned in the manuscript). Finally, showing the total ice extent after 2009 for H05 (we extended a bit the run compared to the other hindcasts) may bias the reader against H05, in terms of overall trend. The resulting “appropriate” 2005-2009 period for comparison is then too short to be statistically significant to our minds.

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

»> Thanks, will correct trend to "negative". One problem that constrains a seasonal cycle exercise is that ice in H02 is mostly in equilibrium at the start of the simulation (again a question of similarity of model configurations with the used IC) whereas that of H05 is clearly not, going through a quick adjustment period (2 years). The overall hindcast period is also short, so removing the first 2 years reduces the construction of the climatology to 2005-2009 (5 years), which we feel uncomfortable to describe as climatological seasonal cycle.

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

»> The latter. The plotting-program-provided coastline is actually a little coarser than the actual model coastline because of the high resolution (2-5 km) of the model in the Arctic. However, this difference of detail would be too fine in maps to be distinguishable.

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.

»> Corrected (see Figures 3, 4, 10 and 11).

Fig2ff: Labels of many figures are too small

»> Corrected in Figures 3, 4, 6, 7, 10, 11 and 14.

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

»> We have tried but found that this was not reflecting, for instance in the Arctic, subtle, but important in terms of hydrography, vertical variations in temperature.

Typos etc.

p.3, l.20: communities' »> corrected p.4, l.6 : no comma after period »> corrected p.6, l.20: 450 m »> done p.6, l.28: dependence »> done p.7, l.16: This is commonly referred to as a 3-layer model (2 ice + 1 snow) »> yes, corrected

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 doc-

umentation 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.

»> We have added text to make the comparisons clearer for each of these figures.

* 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?

»> We believe that the statement on page 11 lines 5-7 is sufficient.

* 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.

»> Unfortunately, this contribution is not aimed at presenting details (including analysis) of the ice-ocean prediction system (which is not ready yet), not to mention details of the long-term planned coupled atmosphere-ice-ocean prediction system. However, we have amended the text at line 47 to reflect this. The coupled atmosphere-ocean data assimilation is certainly a hot topic that we cannot really address at this stage.

* 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

»> We followed the reviewer’s suggestion throughout the text and figures.

* 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’.

»> Absolutely right! We use though Canadian English which may depart from UK English in a couple of occasions.

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 (at-

mosphere, sea-ice, biogeochemical tracers, ...) over a wide range of space and time scales.”

»> The reviewer is correct. See modifications at Section 2.1.2. We now describe NEMO as a bio-physical multi-component system, wih OPA as the ocean model component.

p7.l4: I think it would be clearer to include units for the viscosity (1e-4 m2/s) even if they are the same as for the following diffusivity (1e-5 m2/

»> done

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

»> The reviewer’s hunch is correct. Corrected.

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?

»> The reviewer is correct. Text modified accordingly at line 146. CICE initial conditions are described in page 11 line 5-7 as already mentioned.

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).

»> Yes, we did the SST comparisons with the OSTIA product but did not include them in this manuscript. There were some redundancy with the upper ocean comparison and the decision was made to not include them.

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?

»> We have corrected the expression and followed the reviewer suggestion of upscaling the model results to 1/2 degree for a more direct comparison. See modified paragraph at lines 345.

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)?

»> Both products contain similar datasets. The QC procedures probably differ a bit between them, but there shouldn’t be a big difference using either for the evaluation.

p17.l18: be careful with the use of “significantly” here. Do you mean statistically 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)”?

»> There was no statistical tests and we have therefore followed the suggestion of the reviewer. The text was improved. "very small" -> "small". "signficantly" replaced by “considerably” as per reviewer’s suggestion.

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?

»> This statement relates to "the recent increase" of line 26. “these recent variations are” replaced by “this increase is” at line 424.

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 difference 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.

»> The reviewer is correct. The original intention was to signify that the temperature difference between the east and west sides of the strait is similar, and we have amended the wording accordingly. We have also made earlier mention of the cold bias in the middle of the strait.

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?

»> The reviewer is correct for the trend. See response to Rev#1. The ice in H05 is likely spun up (in terms of thermodynamics) after 2 years as stated in the text. Then, the model dynamics accumulate too much thick ice in the Beaufort Gyre which starts to show in the total ice area after 2009.

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?

»> For comparison with LIM2, we prefer to look at the ice thickness retrieved from ICE-sat. We fear a typo in the reviewer's "converse" argument. We believe she/he meant the "thinner" categories. Yes, this is now mentioned at line 526. Grossly speaking: yes, Figures 14 and 15 are consistent.

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)

»> Thanks for the references. Wang et al. (2008) [added reference] found that ice area and volume on LIM2 is linear with the parameter hiccrit. The "over-estimation" is therefore not systematic but tends to be an artifact of people using generally a (too) large value for hiccrit. Text modified accordingly at lines 168-174.

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?

»> The reviewer is globally correct. However, we have some confidence that after studying the mean March ice circulation (Fig.18) that we are mainly looking at differences in the intensity of this circulation, and that therefore it is legitimate and meaningful to investigate the bias in velocity magnitude. To complement this, we have also

looked at the RMS of the velocity vector differences (i.e. summation of || v_mod - v_obs ||ˆ2) which yields similar results and ranking. We however amended the text at line 561 to reflect this.

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?

»> The reviewer is correct. There is about a one month lag, now stated at line 537. PIOMAS uses the NCEP forcing which seems to be uncorrected (see Large and Yeager's analysis, 2004 and Hunke's, 2007). We can only speculate if this is sufficient to explain the lag.

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 "Ekman transport within the ocean"?

»> "of" replaced by "on". The expression is ill-posed but tries to discriminate the portion of Ekman transport which is at play in the ice and in the ocean. One could see the Ekman spiral process as being applied to the combined ice-water system.

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).

»> Not all MERCATOR operational systems incorporate indeed 75 vertical levels but this is the goal. The latest GLORYS analyses (2v1 and 2v3) were produced with 75 levels for instance and 75 levels is the standard in research mode (DRAKKAR). Thus, to satisfy the reviewer, we suggest to add the expression "in research" at line 650.

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

»> done

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?

»> Done.

Figure 6: please remove “PSU” from salinity colourbars

»> Done

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.

»> Text added at line 397-402 and in the figure caption. The reviewer's second guess is correct. The model is collocated in time and space with the observations and then both are averaged horizontally and in time to yield a single profile per box. “PSU” removed as per reviewer's suggestion.

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

»> It is regions where salinity exceeds 34.8 over the whole water column. Included in the caption now.

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.

»> Dashed lines added to Fig.9 as per reviewer's suggestion. Indeed, the modelled lines are monthly values whereas the observations are only valid for the summer period. Text added a line 430. Therefore the model values include a seasonal cycle which is not present on the observations. No collocations was involved in this plot as Proshutinsky et al. (2009) provides an estimate for the entire region with error bars. The region of integration is also clearly defined in the same paper. See added text.

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.

»> Yes, ice in H05 adjusts in a 2-year period to the initial condition. See above response.

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

»> added.

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.

»> see above response for p21.l17-18 and p21.l15.

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).

»> NSDIC product has a resolution of 25km. It is sufficient to resolve the circulation in the Beaufort Gyre. It is however a bit negatively biased as shown in Fig.19, possibly related to the methodology employed. The CERSAT estimate is somewhat faster (but still slower than any modelled ice drift) and a little noisier but does not cover as many regions (not shown).

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

»> Texts added at lines 561 and 566. See also response to Rev#2 Section 3.2.2.

Minor typos etc.

p2.l9: "model represent" should be "model represents" or "model represents" »> Corrected p3.l22: "program" should be "programme" (unless it's a computer program) »> done p5.l15: "re-increasing" is not very good English and should be replaced »> corrected p6.l1: please remove "very" as "substantially" shouldn't need any further quantification »> done p15.l23: "myOcean (www.myOcean.eu)" should be "MyOcean (www.myocean.eu)" »> done p15.l26: "program" should be "programme" »> done p16.l2: "programs" should be "programmes" »> done p16.l3: "programs" should be "programmes" »> done p16.l17: please remove "PSU" »> done p16.l20: please remove "PSU" »> done p17.l5: please remove "PSU" »> done p17.l6: please remove "PSU" »> done p18.l28: "maximums" should be "maxima" »> corrected p19.l21: "coefficicents" should be "coefficients" »> done p20.l5: "adjusement" should be "adjustment" »> done p22.l15: I don't like winds being described as "large". This should "high winds" or "strong winds" (or perhaps "large wind stresses"?). » done

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C313/2015/gmdd-8-C313-2015-

supplement.pdf

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