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The Cryosphere Discussions

Interactive comment on "On the influence of model physics on simulations of Arctic and Antarctic sea ice" *by* F. Massonnet et al.

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Communication to the Editor and the Reviewer

Please find below our answer to the anonymous Reviewers #2's comments. We acknowledge your time and careful reading of our manuscript. We have aimed in our answer at providing clear and concise responses. If however there remains some questions regarding the new version of the manuscript, we stay at your disposal for further information.

Sincerely,

On behalf of the authors,

François Massonnet

Answer to Anonymous Reviewer # 2

The Reviewer's comments are in bold font and answers in regular font. The list of references is provided as a supplement to this document.

1. **I would like to see a bit more information on the model setup. For example, what is the frequency at which parameters are exchanged between the sub-model.**

   We now have included some additional information about the simulations setup in Sect. 2.4 of the original manuscript (p. 1173, line 5), such as the frequency at which the sea ice and ocean models interact and the value of the ocean–ice drag coefficient. In this section, we made the following change:

   > *The ocean model has a time step of* $\Delta t_0 = 3600$ *s* $= 1/24$ *day and the sea ice models are called every 6 ocean time steps*

   has been changed to

   > *The ocean model has a time step of* $\Delta t_0 = 3600$ *s* $= 1/24$ *day. Both sea ice models are called every 6 hours, i.e. every 6 ocean time steps. Finally, the ocean–sea ice drag coefficients are set to* 5.0×10^{-3} *in both models.*

   Please note also that, following the remark 1 of Reviewer #1, we have added a table with important sea ice parameters for the two models ("Table 1" of the new manuscript).

2. **The authors need to include a justification as to why it is sufficient for them use hourly atmospheric forcing and even some monthly climatologies. Daily forcing is expected to considerably affect the modelled quantities.**

   About the forcing frequency: Yes, we agree on that the forcing frequency is critical for simulating the ocean and sea ice cover. However, we perform our analyses in a climatic perspective, i.e. we wish to investigate the importance of sea ice model physics in GCMs (as stated in the Introduction). It is well known that only a very limited number of these GCMs use sub-daily coupling frequencies between their atmosphere and ocean–sea ice components.

   Besides, in an earlier model sensitivity experiment, Bernie et al. (2005) found out that most of the diurnal-to-intraseasonal variability of the sea surface temperature can only be reproduced if (i) the forcing frequency is high (less than 3 hours) and (ii) the uppermost ocean model layer is thin (~ 1 m). To our knowledge, the NCEP/NCAR reanalyses used in this study are not processed at a higher rate than 4 times a day. On the other hand, the first of the 42 levels of the OPA ocean model in our configuration is ~ 5 m thick. This is why we use daily forcings for the 2 m air temperature and the components of wind at 10 m.

   About the use of some climatologies: Regarding the use of climatologies for total precipitation, relative humidity and cloud cover, we know by experience that the NCEP/NCAR data sets introduce large biases in the two ocean–sea ice models NEMO-LIM2 and NEMO-LIM3, because the products are themselves biased (Bromwich et al., 2007; Walsh et al., 2009; Vancoppenolle et al., 2010). In contrast, the climatologies of cloud cover, precipitation and relative humidity mentioned in the manuscript have shown realistic results on the simulated global sea ice cover (see previous studies of Timmerman et al. (2005) and Vancoppenolle et al. (2009)). We have added for clarity the following text in Section 2.3 (p. 1173, line 1 of the original manuscript):

> *The use of some climatological forcings is motivated by the questionable reliability of the corresponding NCEP/NCAR atmospheric data sets in the polar regions (Bromwich et al., 2007; Walsh et al., 2009; Vancoppenolle et al., 2010) as well as the realistic global sea ice cover obtained in similar studies with these climatologies (Timmerman et al., 2005; Vancoppenolle et al., 2009).*

3. **The authors need to explain why the model comparisons are carried out from 1981 onwards but why the runs start in 1948. Surely the model realizations do not require a 30years + spinup.**

   We agree with the Reviewer that the sea ice models do not need such a long spinup run; however, the ocean model does because of its slower response to external forcings. We think that 30+ years is a reasonable duration to drive the surface ocean and sea ice towards climatic states, so that the initial conditions have little impact on the sea ice cover, and thus allow proper comparison between the two simulations.

   On the other hand, we focused our analyses on the 1983-2007 because (1) the data for concentration are only available from 1979 and (2) we excluded the years 1979-1982 given there is a known bias towards high temperatures in the Arctic NCEP/NCAR atmospheric data set (p. 1174, line 10 of the original manuscript).

4. **More emphasis should be given to the albedo effects.**

   Certainly, the snow and sea ice albedos critically determine the rate of melt and therefore the summer sea ice cover. In our study, the parameterizations of surface albedo are identical in both sea ice models and follow that of Shine and Henderson-Sellers (1985). The main differences in grid cell averages of albedo arise from the different representations of the sea ice thickness distribution in the two models. Given the nonlinear parameterization of Shine and Henderson-Sellers (1985), lower surface albedos are prescribed for melting thin ice while a constant value is assigned over a certain threshold thickness. Following the Reviewer's suggestion, we have computed the mean surface albedo of LIM2 and LIM3 in the ice-covered area of the black box depicted in Fig. 6 of the original manuscript. We obtain values of 0.52 and 0.46, respectively. As explained above, the lower mean value for LIM3 is due to the presence of different ice thickness categories in that model: the individual albedos (from the thinnest to the thickest category) read 0.39, 0.47, 0.53, 0.53 and 0.53.

We now have included this information in Section 5.1, p. 1182 line 12 of the original manuscript:

> *For the same mean thickness, the reductions in ice concentration and thickness are thus enhanced in LIM3 compared to LIM2 when melting occurs, and the ice-albedo feedback is accordingly stronger.*

has been changed to

> *For the same mean thickness, the reductions in ice concentration and thickness are thus enhanced in LIM3 compared to LIM2 when melting occurs. To a large extent, this is a result from the sensitivity of the identical parameterization of surface albedo in the two models (Shine and Henderson-Sellers, 1985), to different ice thickness distributions. The mean LIM2 (LIM3) albedo over the ice-covered surface shown in Fig. 6 is 0.52 (0.46), indicating that the summer melt is more intensely represented with a multi-category sea ice model.*

5. **Need to give a justification why Rampals et al.'s (2009) spatio-temporal averaging scales are considered valid in the Antarctic sea-ice zone.** This is a good point, thanks. Rampal et al. (2009) only focused on the Arctic, and we decided to translate their rules of averaging to the Antarctic for two main reasons. First, to our knowledge, there is no equivalent study in the Southern Hemisphere.

Therefore, as a first guess (P. Rampal, pers. comm.), we used as a best estimate of the mean drift the averaging rule of Rampal et al. (2009). Second, a key finding of our study is that the representation of advanced physical processes in sea ice models is critical in the Northern Hemisphere but to a smaller extent in the Southern Hemisphere; to derive such a statement, we need obviously that the procedure of evaluation be identical in both hemispheres.

We have made the following change (p. 1177, line 17 of the original manuscript):

> *We follow these recommandations for both hemispheres.*

has been changed to

> *We follow these recommandations for Arctic sea ice. As a first guess, and because no equivalent study exists in the SH to our knowledge, we transpose this averaging method to the Antarctic sea ice.*

6. **It would have been useful to see the metrics for a single grid cell or small region (each in the Arctic or Antarctic) in addition to the hemispheric metric. Are these available to inclusion?** We agree with the Reviewer that local metrics are also important for model evaluation. Here the main limitation is the spatial distribution of the reference data set (i.e. the observations) used to evaluate the models. For the period of evaluation (1983-2007), only the sea ice concentration and drift have been retrieved on gridded supports. For these variables, our metrics take the local and global evaluation into account: (1) ice concentration (local) and extent (global) are evaluated for each hemisphere, and (2) ice direction (local) and mean drift magnitude (global) are assessed in each hemisphere. Regarding sea ice draft and thickness, the data sets of Rothrock et al. (2008) and Worby et al. (2008) suffer from irregular spatial sampling. We decided therefore to only derive metrics for the global Arctic and Antarctic. Surely, an intermediate-scale evaluation (e.g. partitioning the Arctic and Antarctic in a few sectors) would

also be welcome for ice concentration and drift, but this was out of the scope of this paper. Thanks however for this constructive suggestion!

7. **Any comments on model performances in the marginal seas (Baltic, Okhotsk etc.)??**

   Similarly to our response to the Reviewer's remark 6, we acknowledge the idea of having a sector-like set of metrics, but we think this out of scope of this paper.

8. **Fig. 4: Use SI units (not cm/s).** We agree on that SI units are certainly more consistent from a scientific point of view, but we would like to keep cm/s for easier interpretation. We propose to let the Editor settle the question.

9. **Fig. 6: What is the physical motivation for the outline of the area assessed?**

   As stated in the text (p. 1182, line 2 of the original manuscript), we illustrate the importance of the sea ice thickness distribution in sea ice models with a snapshot of that distribution in a particular sector, defined by the black triangle of Fig. 6 (original manuscript). At that time (start of the melt period), this area comprises the actual ice edge (see Fig. 1 of this document; caption is at the bottom of page C8). This is thus a relevant choice for analyzing the distribution of ice thickness in the two models, since the sea ice will anyway be subject to melt in this sector. We think that the original sentence in the manuscript (p. 1182, line 5) :

   > *We chose this particular box because it contains the actual ice edge and thus encloses much variability.*

   is sufficiently explicit to motivate our choice. However, if the Editor considers it should be explained in more details as above, we can modifiy the original manuscript.

10. **Fig. 7: Caption reads "(LIM3, purple)" but in the figure that appears green.**

    Thanks for the careful reading: " purple " has now been changed to " green ".

11. **Figures: They are generally too small. Pls make use of the white space on the paper.** Similarly to our response to Reviewer #1's comment 3, we agree that the figures in the online version of our manuscript are too small. However, this is only a question of page layout since our manuscript written with the Copernicus package has readable figures (see http://www.astr.ucl.ac.be/users/fmasson/paper_physic.pdf. We suggest therefore that the Editor makes the final decision.

12. **Figures: Fig. 2 and those displaying geographical distributions are just too small to allow the reader to check on necessary details. This is especially true for Fig 4. The vector fields are just too sparse to derive any conclusion of the model data.... plotting 1 out of 49 vectors is not sufficient here.**

    Please refer to the previous answer.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/5/C818/2011/tcd-5-C818-2011-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 5, 1167, 2011.

Fig. 1. (see Figure below) Simulated ice concentration (with LIM3) on the 1st of April, 2007. The black box is the same as the one used for diagnosing the ice thickness distribution in Fig. 6 of the original manuscript.

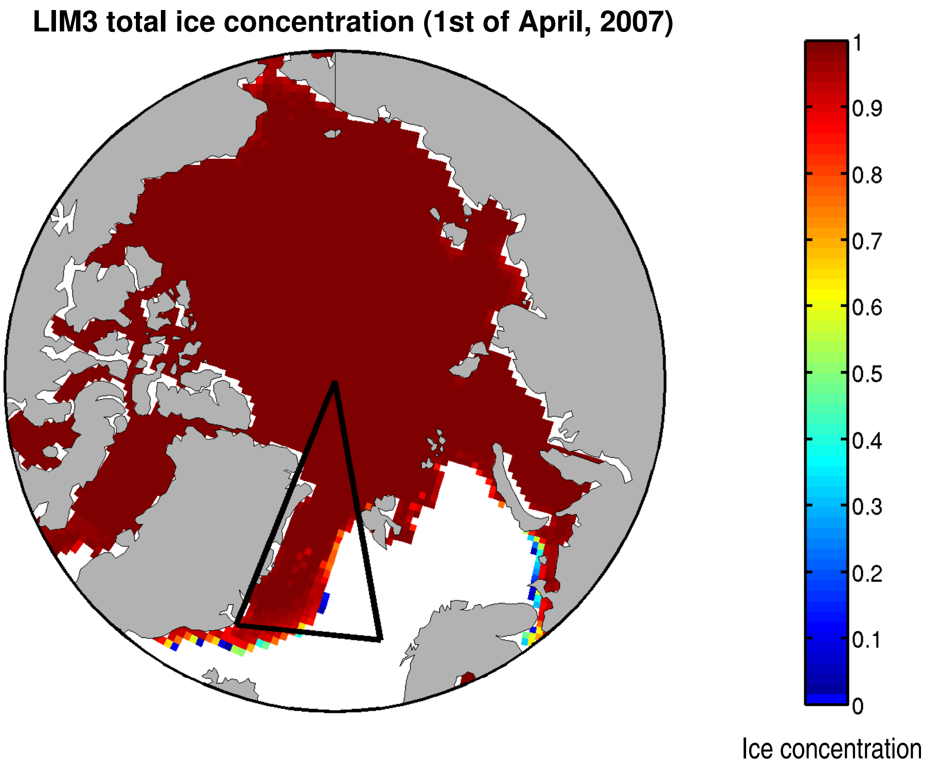
LIM3 total ice concentration (1st of April, 2007)
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
Ice concentration

Fig. 1.