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Interactive comment on "Sensitivity of ice production estimates in Laptev Sea polynyas to the parameterization of subgrid-scale sea-ice inhomogeneities in COSMO-CLM" *by* O. Gutjahr et al.

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R2: My main concern is that the current manuscript fails to appeal to scientists who are not familiar with polynya processes in the Laptev Sea and/or are not using the COSMO-CLM model. Required background information and motivation (e.g. formation of polynyas, importance of polynya ice production) are missing in the study.

A: Although we mentioned some aspects of polynya formation in the Laptev Sea, we agree that too few information is given for readers which are not familiar with polynyas and how they are implemented in regional climate models besides CCLM.

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Changes in the manuscript: We will add more information and details on the polynya processes in the Laptev Sea and how polynyas are represented in RCMs. Further we will state more clearly the objectives of our study.

R2: The discussion of the results is too restricted to differences with one remote sensing product (Willmes et al., 2011) and potential adjustments of the COSMO-CLM model. What can somebody learn from this study who is not using COSMO-CLM? Prescribing the subgrid-scale ice thickness cannot be the best solution to simulate polynya processes. What are consequence from your study for applying a more complex sea ice model which aims to simulate the processes?

A: We chose to compare our results to the estimates of Willmes et al. (2011) because it is based on the same polynya masks and the same satellite date (i.e. on the same original AMSR-E product). We think with the product of Willmes et al. (2011) we chose the most suited product available for our comparison, as mentioned in the manuscript. Otherwise, even more issues arise for comparisons with model results. We think that the results of the sensitivity study are valuable also for other models using the tile approach and prescribed sea ice coverage. Although we use a rather simple approach to represent subgrid-scale ice thickness, some of the issues remain even if more complex approaches are used. Subgrid open water or thin ice fraction is also a problem for complex sea-ice/ocean models.

Changes in the manuscript: We will adapt the discussion section by commenting on the general relevance of our results for other RCMs and consequences if more complex sea ice models are used within an RCM. As far as the remote sensing product is concerned, we already tried to generalize from our results, so we do not see the requirement to adapt the paragraphs dealing with this issue.

R2: Specific comments:

1. Abstract: too long; too many details about setup; mention that COSMO-CLM is atmospheric model; not clear whether numbers are winter averages or extremes from case studies. Better give numbers from preferred reference run. Last sentence too specific for COSMO-CLM setup (see General Comments.)

A: We agree on the issues raised by the reviewer.

Changes in the manuscript: The abstract will be revised and shortened considerably. We will also make clear what the numbers represent.

2. Introduction: mainly just technical introduction; paragraph about polynyas and their importance missing; mention discrepancy of estimates of polynya ice production from previous studies.

A: We put the paragraph on polynyas within section 2 as 2.1. This was not the best option and the introduction (but also the general structure of the manuscript) needs to be overdone.

Changes in the manuscript: We will move and integrate section 2.1 to the introduction and add more background information on polynyas, their importance and what was not represented in previous studies.

3. Configuration of CCLM / initial conditions: If a model grid point has e.g. SIC = 60%, is this grid box regarded as polynya box in which 60% of the area is covered by ice with a thickness of TIT (10 cm for model run C05wt1) and 40% with a thickness of the subgrid-scale TIT (1cm for this model run)? Do the ice thickness, TIT and subgrid-scale TIT change during the 24h simulation period?

A: This is correct, the SIC of AMSRE constitutes the grid-scale ice thickness (10cm) in this example and 1-SIC is the 'open-water' fraction or the area with subgrid-scale ice thickness. However, this differentiation is not restricted to polynyas but is applied

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generally for fractional sea ice. The ice thickness is allowed to change within a time step, but is restored after every time step.

Changes in the manuscript: We will reformulate the sentences concerning these two issues to make the procedure more clearly.

4. Page 8, line 13: "the turbulent exchange coefficient CH is variable in time": Why? Please write "is a function of ..."

Changes in the manuscript: We will reformulate the sentence.

5. Verification with in situ data: The AWS were deployed over the fast ice and Table 3 and Figure 4 document that there are no significant differences between the sensitivity runs as long SIC > 95%. Just show results from one simulation (CO5nt) in Table 3. Figure 4 could be omitted in my opinion.

A: We agree on this comment.

Changes in the manuscript: We will remove Fig.4 from the manuscript and restrict Tab. 3 to three simulations (C05nt, C05wt0, and C05-50/1). We will also change the abbreviations of the simulation runs to: C05, C05-10/1, C05-50/1 for a better readability.

6. Case study on 4 January 2008: The differences in Figure 5 and 6 are quite difficult to spot and at this stage the reader is not aware whether you have a preferred reference run. Might be helpful to change order and to focus just on the preferred run for the case study

A: We agree on this comment.

Changes in the manuscript: We will restrict Fig.5 and Fig.6 to only three simulation

runs (see comment before). Further we will introduce in the beginning of section 2, what configuration is the reference and what is the optimal one in our opinion so that the reader can follow our chain of arguments more easily.

7. Conclusions: Only present numbers from your preferred reference run. Put your results in wider context. See general comment. Add a paragraph about how your results might help to simulate polynya processes using a more complex sea ice model including recent advances of frazil ice modules (e.g. Wilchinsky et al., JPO 2015).

A: We agree on this comment.

Changes in the manuscript: We will extend the conclusion section by mentioning what our results mean for more complex sea ice models and restrict the presentation of numbers to our preferred simulation.

8. Give numbers with adequate decimals in text and tables (e.g. +110% instead of 109.7% in line 11 or 29 km^3 instead of 29.05 km^3 in line 14).

Changes in the manuscript: We will change the numbers accordingly to improve the readability of the manuscript.

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