The Cryosphere Discuss.,
doi:10.5194/tc-2016-83-AC1, 2016

The Cryosphere Discussions

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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Received and published: 29 August 2016

R1: The attention to detail is totally overwhelming, and there is almost nothing learned in terms of physics.

A: We agree that many details of the results were included in the manuscript, which could make it difficult to focus on the main aspects. The primary objective of the paper was to assess the sensitivity of ice production of Laptev Sea polynyas on the chosen assumption for thin-ice thickness of a tile approach for subgrid-scale energy fluxes. This is not a specific problem of the used model (CCLM), but a general problem of

all regional climate models using the tile approach. To our knowledge, it is generally assumed that the subgrid fraction not covered by sea ice is assumed to be open water (e.g. in the recently published ASR data set). We could show that the ice production is very sensitive to the tile-approach and thin-ice thickness, which affects also the atmospheric boundary layer structure. However, we agree that for example the latter issue was not discussed sufficiently.

Changes in the manuscript: In the revised manuscript we change the structure of the paper and focus more on the physical aspects by simultaneously reducing the details of the results, i.e. we present only the results of three simulations and show sensitivities only where useful.

R1: The use of abbreviations also has no end, and is a clear sign that at this level the text is more like a data report intended for those that may apply the same model system in a future study. One look at Table 3 should say it all.

A: Multi-model or sensitivity studies always include a lot of abbreviations. We accept this remark and thus reduce the amount of details and abbreviations to a necessary minimum.

Changes in the manuscript: The abbreviations of the simulation runs will be homogenized and we will change the structure of the manuscript so that it focuses on the scientific aspects not on the technical details. Therefore, we restrict the presented results to three simulations: C05nt (the reference), C05wt0 (subgrid-scale open-water scenario) and C05-50/1 (most realistic assumptions). We will change these abbreviations to: C05, C05-10/0, and C05-50/1. Table 3 will be condensed.

R1: For the main conclusion not much has been learned about the atmospheric boundary layer, where the model actually should resolve processes in a better way than earlier model attempts. That the overall heat loss increases along with the total ice produc-

tion when areas of thinner ice is added as a new lower boundary condition is indeed what is expected and does not contribute to an increased scientific understanding.

A: We agree that we did could include more results on the ABL. However, we have already addressed some important aspects (impact on the warm plume formation, turbulence structure, cloud formation), which contribute to an increased quantitative understanding of the processes and their feedbacks.

Changes in the manuscript: We will rewrite the ABL part to point out the main conclusions.

R1: The text is also written only from a modellers perspective, without even the most basic understanding of processes in a polynya in nature. Moving downwind from the beach there is CONTINOUS change from open water to thicker and thicker sea ice, much like the MODIS observations plotted in Figure 13. In essence resolving the heat fluxes and the ice thickness inside a polynya is a coupled problem. Such coupling has been done albeit in a very simple way starting with Pease (1987). I'm not saying that you should invent a new downwind thickness parametrization for thickness, but rather state that you have made your choices, and then how this is simplified from nature.

A: We see the point that the text is focused too much on the modeller's perspective, however it is not clear to us how we missed "the most basic understanding" of polynya processes. We did not intent to give a too detailed introduction on polynya processes and thus cited relevant papers for more information. But we agree that some more information on e.g. polynya formation and the spatial structure of thin-ice within a polynya are useful additions. We are aware that the ice thickness increases with downwind direction, which is not represented in CCLM yet. Figure 13 shows the spatio-temporal histogram of thin-ice within Laptev Sea polynyas retrieved from MODIS data, which is not to confuse with the spatial sequence of thin-ice in a polynya. Our implementations to CCLM are just the first step to represent fractional sea ice, which was not present

in all CCLM simulations before. In this context, we would like to note that even Polar-WRF does not use spatial distributions of thin-ice within polynyas, in fact in WRF there is always subgrid-scale open-water assumed, which is much more unrealistic then our assumptions. Comparing e.g. Fig.11a and Fig.11c there is still a downwind structure of the ice production visible for the WNS polynya (opened on 30 April 2008), which is not present in the reference simulation (Fig.11a) (and weaker for the other simulation runs).

Changes in the manuscript: We will comment on our chosen assumptions on the thin-ice distribution and that it is a simplification to the thin-ice structure observed in nature. This is an important point we missed to mention in the manuscript.

R1: A statement like 'open water areas particularly produce new ice and are hence rarely free of ice' (Page 6, line 10) is not correct at all. Polynyas stay open for many hours during strong wind events that effectively transport sea ice (frazil, grease, pancake, solid ice) downwind (Morales-Maqueda et al 2004, Fig. 17). An open polynya length along the wind direction between 10 – 30 km is not uncommon.

A: We guess our formulation might be too imprecise as we actually meant that the heat loss is highest over open-water areas. These open-water areas quickly produce frazil and grease ice, which is then advected downstream and consolidates to thicker ice, hence the continuous increase in thickness mentioned in the previous comment. However, based on field experience of the authors we argue that the fraction of the Laptev polynya area that is completely free of ice is relatively small during winter (as illustrated in the appendix and Fig.13 of our paper).

Changes in the manuscript: We will reformulate this sentence to make clear what we wanted to express and we further add the information that in our simulations it is assumed that new ice is instantly advected downstream so that the initial thin-ice thickness is restored after every time step. We will also add the word 'wintertime' to

the polynyas.

R1: The most interesting part of this study is the response of the atmospheric boundary layer, as shown in Figure 7. But here two plots should be shown, the “best” case and the similar without the tiles (C05nt – perhaps, it is just a total misuse of abbreviations here). This should be the case also for Figure 5, Figure 6, and Figure 11. All the tables should only compare values between your “best” model simulation and the one without the tiles. The details are not interesting, unless you have some way of evaluating the model performance.

A: We think that the most interesting part is the ice production, since this has impacts also for the ocean circulation. We will pick up the suggestion of taking the “best” model simulation as reference.

Changes in the manuscript: As mentioned above we will restrict the presentation of results to three simulation runs. That is we reduce the amount of subplots of the mentioned figures and also reduce the tables to a necessary minimum.

R1: The paper needs to be totally rewritten if it is to be published as a scientific article. First – make your choice on the “best” model simulations, and present all relevant results to this one case first. Then compare to existing simulations without the tiles. At the end you can include some sensitivities to some of the different choices made, like the different thin ice thickness’ inside the polynya. This reviewer has not been convinced that new scientific understanding has been achieved here, but I’m willing to review a new version of a totally rewritten paper if that is submitted. Sorry to be so negative, but this version can be saved as a technical report for researchers that will work on the same model in the future. No one else would have the interest to read about all these details, and you have not done the important scientific job it is to extract the new understanding based on your model simulations.

A: We have a different opinion concerning the reviewer’s statements about scientific understanding and technical report, but we will restructure and rewrite the manuscript also considering the remarks of reviewer #2 (who states that we show the significant results).

Changes in the manuscript: As mentioned above we will present and compare the results of three simulation runs: a reference without the tile-approach, one run with subgrid-scale open-water as a possible upper limit, and one run which we think is the most realistic configuration.

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