Geoscientific

Model Development

Discussions

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Interactive comment on "An EC-Earth coupled atmosphere-ocean single-column model (AOSCM) for studying coupled marine and polar processes" *by* Kerstin Hartung et al.

Anonymous Referee #2

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The manuscript provides a detailed description of a coupled atmosphere – sea ice – ocean column model and examples of its applications. The model components and the coupling methods are basically well presented. Particularly good in the manuscript is the discussion on how to design experiments applying the coupled column model. Also the Introduction is very good and motivates a reader. The manuscript has potential to become a good paper, and particularly useful for those who carry out column model experiments or plan to start such activities. The manuscript has, however, also weaknesses, and substantial revisions are needed before I can recommend it for publication.

Major comments

1. In several parts of the manuscript, the text is too technical, including acronyms never explained, e.g. on page 6, lines 8, 10, and 17; page 7, lines 12, 14, and 15, and many other places. This holds also for Figures 1 to 3, which are more like computer codes than illustrations that one would expect to see in a peer-reviewed journal.

2. The description of sea ice component is very brief, and leaves many open questions. How many model levels there are in sea ice and snow? How the grid is affected when ice and snow thicknesses change, i.e., is the number of levels changed or is the vertical resolution changed? Is penetration of solar radiation into snow and ice taken into account (as in case of the open ocean)? Is snow-to-ice transformation (via refreezing of melt water, rain, or sea water flooded on top of ice) taken into account? Are melt ponds taken into account? These are important questions in atmosphere – ocean coupling in Polar regions. Figure 3 does not provide any help on these issues.

3. I am concerned about conclusions made on the basis of comparisons of the model results and ERA-Interim reanalyses in the open-ocean experiments. Are the vertical profiles in ERA-Interim over remote ocean areas accurate enough to make robust conclusions on the performance of the column model? Some references or other evidence on the accuracy of the ERA-Interim profiles should be provided. Further, on page 11, line 3, it is stated that observational data are complemented by ERA-Interim for the atmosphere. It should be more clearly explained what atmospheric variables were observed.

4. The presentation of the results of the three cases studies should be clarified. It is presently hard to understand the basis of the conclusions made.

5. On page 11, line 24, it is told that 0.06 m was used for sea ice roughness. This value is much larger than observations indicate (see papers by Andreas, Lupkes and others). If the key parameter for dynamical coupling between the atmosphere and sea ice is so strongly tuned, what is the relevance of the column model experiments?

6. Advective tendencies were not applied in the ocean, but could be obtained from ocean reanalyses, analogously to obtaining them from ERA-Interim.

7. In Summary and Outlook the authors should better evaluate which processes can be simulated applying AOSCM and which cannot. Considering processes acting in the vertical dimension in the Arctic Ocean, there are major challenges in understanding and modelling at least the following processes: gravity drainage of salt in sea ice, brine formation, turbulent exchange of momentum, heat and salt at the ice base during ice growth, double-diffusive convection, and those mentioned in comment 2 above. After reading the manuscript, it remains unclear if AOSCM takes these processes into account.

Minor comments

Page 3, line 7: Do you mean 44% of meteorological modelling centres or modelling centres in general?

Page 5, line 18: Clarify what is meant by a reference state.

Equations (5) and (8): If part of the solar radiation penetrates below the surface, as in Eq. (8), it should not contribute to surface energy balance, but this is not reflected in Eq. (5).

Page 5, line 31: should there be a comma after "(2016)"?

Page 7, line 21: does "fraction" mean areal coverage of sea ice?

Page 8, line 23: ". . . only, or not, over. . ." is puzzling.

Page 9, line 12: Briefly explain what is asynchronous coupling.

Page 10, line 24: Does "near-surface temperature" mean 2-m air temperature?

Page 10, line 28: Was the tide only due to M2 component? Is the diurnal tide had large contribution, 12 h running means do not help. Note that tidal currents may also affect

vertical mixing in the ocean (although not necessarily much in the case presented, as it is in the deep ocean) and, if the model does not include tides, temporal averaging does not help.

Page 12, lines 29-31: Why did the results improve?

Page 14, line 31: Why not simply write RMSE instead of confusing "integrated bias".

Page 17, line 2: Why do you call EC-Earth as "the next version climate model"? It sounds very strange, particularly when referring to a paper eight years old.

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