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Interactive Comment

Interactive comment on "Importance of open-water ice growth and ice concentration evolution: a study based on FESOM-ECHAM6" *by* X. Shi and G. Lohmann

Anonymous Referee #1

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The authors examine how a change in the distribution of lateral vs. vertical sea-ice growth and melt affects simulation results of a global coupled climate model. In addition to the obvious, known findings that there is more sea ice if open water is allowed to be maintained for longer, they also establish a feedback of the ice-volume changes on the oceanic and atmospheric circulation and temperature fields.

I find that currently it remains unclear which of the results found here are indeed new, and which of the new results are indeed significant. I can therefore only recommend a major revision of this work, which in particular must address the following overarching issues:

1. Fichefet and Maqueda (1997) carried out a similar study in an uncoupled setup. Which results in the current study go beyond these results?

2. Mauritsen et al. (2012, doi: 10.1029/2012MS000154) and Notz et al. (2013, doi: 10.1002/jame.20016) describe the impact of tuning the lead-closing parameter in MPI-ESM in some detail. Which results in the current study go beyond these results?

3. A number of studies have examined the impact of changes in sea-ice volume on atmospheric circulation, most recently regarding the possible impact on mid-latitude weather systems. Which results in the current study go beyond these results?

4. What is the experimental setup of the experiments conducted here? How long was the model run? Is it in equilibrium? How significant are any of the results found? Could the differences between the two simulations simply be caused by internal variability?

5. The description of the ice-concentration evolution in section 2 follows closely Hibler (1979). I think this should be made explicit, and only the modifications to the original scheme should be discussed in more detail.

6. The description of the simple idealized model in section 3 follows closely the PhD thesis Notz (2005). In particular, all approximations for atmospheric fluxes were apparently directly copied from that thesis without any reference. This should be changed.

7. The relevance of any of these findings depends on the complexity of the ice-thickness distribution in any given model. It should be discussed if these results have any relevance to modern sea-ice models that usually have more complex distribution schemes of sea-ice thickness than the one given by Hibler, 1979.

8. Units in Figure 10 are confusing: a mass transport usually does not have units m^2/s.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2137, 2015.