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Interactive comment on “The location of the thermodynamic atmosphere–ice interface in fully-coupled models” *by* A. E. West et al.

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Dear Dirk,

Thanks for your comments and suggestions, which are very useful. I would like to respond in particular to three of them:

“p.9710, l.9: The coupling across the interface could be implicit, too, if the entire sea-ice temperature field is updated by the atmosphere solver...”

Yes, this is a good point, and as you say is how GFDL manage the problem. But I think that in the framework described in this paper, this would be equivalent to choosing the atmosphere-ice model interface to lie immediately above the base of the sea ice. The coupling across this interface would still be explicit, but this would matter less

if the sea ice base was assumed to be at the ocean freezing temperature. And the thermodynamics above the interface – i.e. throughout the atmosphere and ice – would be implicit.

A sentence will be added to the paragraph in question to note this possibility in the final version.

“p.9716, l.23: I could not identify any solid grey lines.”

This was probably not very clearly worded. The lines in question do not plot any particular quantity – they are vertical, and are placed at 3h intervals to indicate exchange of coupling variables, corresponding to the major tick marks. I think they are clearly visible on the figure, but I notice now that these have been incorrectly placed for Figures 2 and 4, where they should be at 1h intervals – this will be corrected.

The reference in the text could be amended to ‘vertical solid grey lines’.

“p.9721, l.9ff: The CICE documentation suggests that "accuracy may be significantly reduced" by placing the interface below the surface...”

This suggestion from the CICE documentation (Section 2 introduction, final paragraph) appears to relate to the theoretical necessity of reducing the conductivity of the top layer to aid convergence in the case of thin ice (when the ‘JULES’ method might become unstable). It is true that if it became necessary to do this, the results of the study would no longer hold. But in practice, we find that setting a minimum ice thickness of 20cm is sufficient to ensure stability in our coupled model, as mentioned at the end of Section 5, so this situation would not arise.

The stability analysis of the Appendix describes how instability arises not in the limit of thin ice, but rather in the limit of strong turbulent heat flux coupling (e.g. during storms), for an ‘intermediate’ band of ice thicknesses. Very thin ice appears to be unconditionally stable, precisely because disturbances to the top conductive flux are able to propagate downwards very quickly, meaning the temperature gradient is always very close to linear.

It should also be noted that in situations when large, rapid changes in conductive flux cause convergence failures in the thermodynamic solver, reducing the effective conductivity by itself does not help, as on short timescales it has no effect whatsoever on the top conductive flux forcing the ice, which is calculated in the atmosphere model. One problem we came up against while implementing this method in our coupled model, with multilayer CICE, was that in cases of slow convergence CICE was prone to reducing the effective conductivity without limit; this had no effect on the strong top conductive flux, and served only to decouple the top layer from the layers below, rendering it actually more vulnerable to instability.

In the final paragraph of Section 5, when the stability, and minimum ice thickness are mentioned, a reference to the suggestion in the CICE documentation will be added, with an explanation as to why it is not necessary to use it in this case.

Alex West

Interactive comment on Geosci. Model Dev. Discuss., 8, 9707, 2015.