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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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This paper explores two thermal coupling methods between an atmospheric model and a sea ice model. The paper is a welcome contribution to literature for a few reasons: (i) the paper describes a well known scientific/technical issue, (ii) a sensible testing and evaluation procedure is described, and (iii) it is good to have this work in open literature because often it ends up in obscure technical reports only. The topic is also highly suitable as a contribution to the discussion on atmosphere to surface coupling.

Coupling through turbulent diffusion in the atmosphere and thermal diffusion in the surface has a few facets: (1) Numerical stability, (2) Conservation, (3) Code modularity, and (4) Accuracy. Numerical accuracy is obviously the highest priority; without stability,

there is no solution. Conservation (of energy) is my view also a high priority, because it is a basic physical property of the coupled system. Often (also in the current paper, I think), it is sacrificed to code modularity. Code modularity is obviously important; without it, code becomes unmanageable. Finally, numerical accuracy is important of course, but given the uncertainty in processes and diffusion coefficients, it may not be the highest priority, although it is good to separate numerical errors from parametrisation errors.

Although I am quit familiar with atmosphere / surface coupling issues, it took me multiple readings to understand how the two coupling methods work. In fact coupling only becomes an issue due to the combination of long time steps requiring implicit solvers in both atmosphere and sea ice and the technical separation of the atmospheric and sea ice codes. Ideally, one would solve the atmospheric turbulent diffusion and the sea ice diffusion equations simultaneously in a fully implicit and coupled way. Some models follow this route but it is often thought that it requires full integration of the sea ice and atmospheric codes. However, it would be possible to define a proper interface to exchange information between the two models. The information to exchange is a linear relation between temperature and heat flux from both the atmospheric and sea ice models models. Such relations can be obtained from the downward elimination sweep of the tridiagonal solver of the atmospheric diffusion problem and the upward elimination sweep of the sea ice problem. In future, I feel that models should aim for this, not only for stability but also for conservation.

In addition, the issue of snow on top of ice is not discussed, although it has a big impact on the heat transport into the ice layer. It also has a big impact on the diurnal cycle of temperature (when the sun is above the horizon), which can be seen from ice buoy data.

A few suggestions to improve the manuscript:

1. Please consider the points above; some of them may be worth discussing in the

introduction.

2. The main difficulty with the manuscript is the interpretation of the results. It is concluded that the flux coupling below the surface is best, but what is the reason. In the simple configuration that is tested (sensible heat flux from the atmosphere matches the heat flux into the ice), the diffusion problem from atmosphere to ice is just a continuous diffusion problem in which the diffusion coefficients vary. So why does it matter whether to shift the coupling level by one layer? I can see three possible reasons: (i) There is a jump of diffusion coefficients near the surface that makes one method of coupling better than the other? (ii) Deeper coupling is always better because more fast responding layers are included in the atmospheric problem (where the diurnal forcing is)? (iii) The coupling below the surface avoids the derivative of fluxes with respect to surface temperature (which causes non-conservation; cf. eq. 10), i.e. it is the conservation that improves the accuracy? It would be nice to discuss the possible reasons for the advantage of one coupling method over the other.

3. p.9712 l.9 The expression for K_k is not correct; it has a different dimension than in in equation (9). It appears that K_k is scaled with the layer thickness, but not in equation (9).

4. p.9712 eq. (10) It is commented that equation (10) is an approximation because of the non-linearity of the outgoing long wave radiation. However, if Fˆ*_o is updated after each iteration the equation could be exact? Below eq. (10), the iteration procedure is described. Is it correct that the result is fully implicit in the sense that also the diffusion coefficients correspond to the new time level? At the end of the iteration with full convergence, Tˆm+1 should be the same as Tˆ*, so I do not see a reason that conservation is compromised? Is it because for this way of coupling, the atmosphere does not use the same surface temperature as the ice model? Please explain.

5. p. 9713 l.19-22 The solution method for equation (11) is explained here in a single sentence, which is difficult to digest. It is not clear how JULES computes transfer

coefficients. Does it need the Richard number as input (i.e. temperature difference and wind) or does it need fluxes? The sentence suggests that it uses temperature first and then fluxes?

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