The Cryosphere Discuss., 7, C2774–C2777, 2013
www.the-cryosphere-discuss.net/7/C2774/2013/

The Cryosphere
Discussions
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Interactive comment on "Decadal trends in the Antarctic sea ice extent ultimately controlled by ice-ocean feedback" *by* H. Goosse and V. Zunz

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Received and published: 19 December 2013

We would like to thanks the Referee for his/her helpful comments and for the positive evaluation of our manuscript. Please find our answer to the comments below.

-1. I find the description of the ice-ocean feedback mechanism somewhat problematic. First and foremost, I do not understand why the mixed layer depth is assumed to be constant in time unless it overturns, in which case its depth becomes a second constant, namely, h_1+h_2. It is clear from the T and S time series presented in Figures 9 and 10 that the density contrast between the two active layers changes significantly over one season. So should too, therefore, their relative thicknesses. Secondly, while 50 m is probably a sensible figure for the depth of a winter mixed layer underneath ice in the Southern Ocean, the choice of the value h_2=10 m for the thickness of the

second layer is unjustified in the paper and open to criticism as follows. Since layers 1 and 2 only exchange water during overturning episodes, it is clear that the properties of layer 2 are identical to those of the deep ocean whenever the upper ocean is positively stratified. Consequently, layer 2 is in now way separated from the deep ocean and its nominal thickness is entirely arbitrary. You could as well postulate that h_2 = 100 m or 1000 m. I view this as a very significant weakness of the model.

The two-level model was presented to illustrate some processes we consider as important in the simplest way and to explain them as clearly as possible. We had thus included in the model the minimum of degrees of freedom and have removed all the elements that were not essential. We agree that the relative thickness of the two layers should change with time in a more realistic framework but this would require increasing the complexity of the model. When using a constant thickness for each layer, we follow exactly the approach of Martinson et al. (1981) for the same sake of simplicity. We had chosen h2 equal to 10m as it is the order of magnitude of the trend in mixed layer depths over the 30 year periods characterized by a large increase in sea ice extent. So we assume that it is the thickness of the layer that could be directly incorporated in the mixed layer during some years and not during others. Our results are not very sensitive to moderate changes in this value. Choosing h2=20 m and T2=-0.75 leads to nearly the same results. By contrast, assuming a second layer of 100 or 1000 meters would imply that we have variations of the mixed layer depth when the two layers are mixed that are much larger than the ones simulated in LOVECLIM. This case would then be less relevant to the LOVECLIM results as it will more likely refer to a case with deeper mixing and polynya formation for instance.

In the revised version, we will insist that the simple model should by no mean be considered as a realistic case and its goal is only to illustrate some processes that may play a role in LOVECLIM simulation. The choice of the processes represented and of the parameters are thus linked to this goal. We will, however, include a third level in the simple model for the revised version. The intermediate level will then be the one that is

incorporated in the surface layer in some cases and is part of the deep ocean (whose depth is arbitrary if no convection occurs in this layer) in some other cases. Adding additional layers would make the deepening of the mixed layer more smooth but would not modify fundamentally the description of the mechanism.

-2. The “distillation” mechanism that the authors propound, whereby brine rejected by sea ice sinks to the deep and therefore contributes to stratifying, rather than destratifying, the upper ocean, is reminiscent of the arguments put forward by Duffy and Caldeira (1999,Climate Dynamics, 15:81âĂŤ88) in support of their parameterisation of subgrid scale brine convection associated with sea ice formation. Is a parameterisation of this type included in LOVECLIM? It certainly would contribute to an enhancement of winter stratification along the lines described in the paper.

Indeed, the parametrisation of subgrid-scale brine convection of Duffy and Caldeira (1999) would also lead to an enhancement of the stratification but such parametrisation is not included in LOVECLIM. In the model, the downward transport of salt is achieved through seasonal changes in the mixed layer depth and then by classical diffusion and the large scale circulation. A note on this point will be added in the conclusion in the revised version of the manuscript.

-3. Finally, while the results reported in Section 5 do not contradict the existence of the ice- ocean feedback discussed by the authors, they in no way constitute conclusive evidence in its favour, and certainly do not permit, in the form they are presented, a quantification of the mechanism. A cautionary note reflecting this shortcoming should be included in the conclusions section.

We agree that it is an important point and we will add such a cautionary note in the Revised version.

Figures 9 and 10 captions. Temperatures are in degrees Celsius, not Kelvin.

Thanks for noting this mistake. It will be corrected in the submitted version.

Interactive comment on The Cryosphere Discuss., 7, 4585, 2013.