

Review of “Decadal trends in the Antarctic sea ice extent ultimately controlled by ice-ocean feedback”, by H. Goosse and V. Zunz

My apologies for the slight delay of my review. I have had the benefit of reading the reviews of the two other anonymous referees and so, for the convenience of both the authors and myself, I shall not touch on points already raised by them.

The article puts forward a new explanation for the increase in Southern Ocean sea ice extent during the last 30 years. This explanation is based on a sea-ice ocean feedback that has hitherto been overlooked. Altogether, the case is well made and the manuscript well written. The paper certainly merits publication. Points that I recommend the authors to address before the paper is accepted are as follows.

-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.

-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*, 15:81—88) 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.

-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.

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