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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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We would like to thank the Referee for his/her evaluation of our manuscript and for the suggestions that will certainly help us to improve the quality of the revised version. Please find our answer to the comments below.

Main comments:

-> The claim that the net downward salt transport due to the presence of sea ice eventually leads to a stabilization of the water column (also illustrated in Fig.4) seems new and somewhat controversial. I would assume that brine release initially weakens the stability of the water column thus leading to a larger oceanic heat flux. This would reduce sea ice formation. Furthermore, the rate of brine release would need to change

over longer time scales to cause a long-term change in stratification. Zhang (2007) argues that reduced ice formation due to surface warming leads to less brine release, less oceanic heat flux, and thus overall thicker ice. On the other hand, based on 1000-year numerical experiments with a comprehensive coupled climate model, Martin et al. (2013) argue that a gradual involvement of the seasonal freezing/melting cycle will eventually lead to a switch from a mode of strong open-ocean convection to a mode of no open-ocean convection. Perhaps the authors could relate their findings to those of Zhang (2007) and those of Martin et al. (2013).

In order to make the role of the net downward transport of salt due to brine rejection clearer, we will reorganize the discussion of the associated mechanisms in the revised version of the manuscript. We will first reduce the section devoted to this point in section 3, just mentioning it and explaining that more details will be given in the following section. The description of the results of the simple model in the revised section 4 will be more focused on this downward flux of salt, to show in detail how it can cause a long-term change in stratification (after the initial weakening of the stability) and to have a clear illustration of its potential relevance to the simulation performed with LOVECLIM. We will not compare the proposed mechanism with other ones described in the literature at this stage (in particular the ones of Zhang 2007 and Martin et al. 2013) to avoid redundancies with the conclusions in which 3 paragraphs (including new material and a reorganization of the text of the submitted version) will deal specifically with this point as suggested by the Referee. We will also stress at several occasion that we discuss in our manuscript feedbacks related to ice-ocean interactions, not the initial cause of the trend.

-> The paper should be shortened avoiding redundancy. The number of figures could also be reduced. I don't think that Figs.1 and 6 are necessary.

As suggested, some parts of the paper will be shortened in the revised version. In particular, as proposed, Figure 1 will be moved to a supplementary material. We will also select 3 periods among the eleven ones that are shown on Figures 5 and 6 to illustrate the variations between the periods. All the individual periods will then be given as supplementary material. Consequently, we will replace the two 11-panel figures of the first version by one 6-panel figure in the revised one (new figure 4). Nevertheless, to take into account all the Referees' comments, some sections will have to be slightly expanded too.

-> An English language overhaul is necessary. This should also lead to a clearer presentation of the main points the authors want to make.

The text has been modified to improve the quality of the language and the clarity of the discussion, as discussed in more details below and in the answers to the second Referee.

Details:

-> Page 4588, lines 18-21: Even though their experiments were multi-centennial, Stössel and Kim (2001) proposed a mechanism for decadal variability, not "multi-centennial variability". That reference would better be suited for the last sentence of this paragraph.

The reference will be moved in the revised version to the last sentence of the paragraph as proposed by the Referee.

-> Fig.3 is very confusing as the color scales do not consistently indicate red for positive trends and blue for negative trends. Furthermore, the choice of the color ranges seems arbitrary. Why do some variables cover the whole range from deep blue to deep red, while others, e.g. (c), (h), and (k), do not? It is also confusing to not have all subfigures on one page. What does the "vertical oceanic heat flux at the ocean surface" mean? Is it the heat flux into the uppermost model layer? The units for (i), (j), and (k) are hard to compare (cm/year for precipitation, m/year for snow precipitation, and m/day for net sea ice production). It might be better to convert all of them into water or ice equivalent units. Concerning the signs of the trends and color scale ranges, the same applies for

Fig.7 and 12.

Figure 3 will be modified following the suggestions made by the Referee. The choice of the colors and their range will be changed to cover a wider range of colors in the revised version, keeping the important criteria to our point of view that the color scale is symmetric around zero to immediately distinguish positive and negative trends. We will also reorder the figures in the order they are referenced in the text. We agree that this figure contains a lot of information but consistently using red for positive trends and blue for negative trends would be even more confusing. It would mean for instance that blue is associated with a cooling but also with less precipitation while it is standard to keep this color for higher precipitation rates. In a similar way, blue would mean an increase in ice thickness but a decrease in ice concentration. We thus prefer to keep the choices proposed in the submitted version. The subfigures were on two different pages because of the layout of discussion papers. We will ensure that, in the final version, they will be on one page only. The vertical oceanic heat flux at the ocean surface is the flux at the ocean/atmosphere or ocean/ice interface. This will be specified in the revised version. The units for total precipitation, snow precipitation and ice production will be given in water equivalent in the revised version as suggested.

-> Page 4591, line 26: "...is associated with..." sounds like the increase in ice concentration is due to a "cooling of the air". In a coupled model, this may also be the result of a higher ice concentration.

We did not mean any causal effect and we agree that a cooling can be due to higher ice concentration. "... is associated with .. " will thus be replaced by "... corresponds to ..." in the revised version which may appear more neutral.

-> Page 4592, line 12: "0.72"; shouldn't the spatial correlation between trend in ice concentration and that in oceanic heat flux be negative? See Fig.3a vs. 3f. Is "Heat flux at the ocean surface" in Table 1 the same variable as that shown in Fig.3f? Lines 19-21: what do you mean by "shallower at higher latitudes"? Fig.3g shows wide regions south of 65S that have apparently no trend (the same holds for Fig.3c and 3f), and locations north of that latitude with large negative trends. Also, the increase of heat content at depth does not reduce the vertical oceanic heat flux, but is rather a result of its reduction. Line 24: should read "...other surface variables...".

Thanks for noticing the mistake. The value should be -0.72. We will be more precise in the revised version when discussing the changes in mixed layer depth. In particular, the region where the signal is the clearest is the Pacific sector where the limit between positive and negative trends is well at the location of the winter ice edge. This will be specified in the revised version. The heat content at depth and vertical oceanic heat flux are coupled so it is not possible to determine if one is responsible for the other or vice-versa. The corresponding sentence will be modified in the revised version to reflect this point. The word 'other' will be added as suggested by the Referee, and the sentence will be modified to make it clearer in the revised version.

-> Page 4593, lines 8-13: Is the snow precipitation (Fig.3j) a fraction of the "precipitation" (Fig.3i)? In other words, is the latter the total precipitation or just the water portion of it? There are regions where the precipitation trend is -0.02 m/year that simultaneously show some +10 m/year snow precipitation trend. There is either something wrong with the numbers or units, or all precipitation must have been converted from water to snow in those regions. Even converting to water equivalent (about + 3 m/year) seems vastly overestimated. Concerning "bringing additional mass to the snow/sea-ice system" and the resulting "weak positive feedback" do you mean snow-ice formation, or just addition of freshwater from snow melt? Both would lead to thicker ice.

The snow precipitation is indeed a fraction of the total precipitation but the unit (m yr-1) were wrong on the figure (but correct in the caption, cm yr-1). This will be corrected in the revised version (see also the response to a comment above). By mentioning the additional mass to the snow/ice system, we indeed focus on the snow and ice themselves (thus in particular snow ice) rather than on the role of freshwater. To make this clearer, the snow ice contribution will be explicitly mentioned in the revised version

of the manuscript.

-> Page 4594, lines 20-29: This mechanism of downward salt transport explains the seasonal increase in stratification. How is this mechanism going to lead to a long-term upward trend in sea ice extent/thickness (see also main comment)? With less brine release due to gradual warming there would accordingly be less downward salt transport thus leading to a weaker stratification, and thus less ice because of enhanced oceanic heat flux.

The goal of this section is to propose a mechanism that explains why the mixed layer depth is decreasing although the trend in annual mean freshwater at surface is close to zero or even destabilizing the water column. The first impact of the brine release and sea ice melting is indeed a seasonal increase in stratification but it also has a longer term effect. This point will be discussed in more details in the section 4 of the revised version and in the conclusions (see also the answer to the main point). Consequently, we propose to only mention the potential interest of this mechanism at this point in the revised version (and thus reduce this paragraph compared to the submitted version to avoid repetitions).

-> Page 4596, lines 3-11: I cannot follow the logic here. Why is it "surprising" to have a lower ice concentration go along with a warmer upper ocean? Irrespective of the fact that the deeper heat content and the surface salinity are not everywhere spatially correlated with ice concentration and the upper-ocean heat content, there should be no reason for why a "lower heat content at depth" (actually only between 200 and 500m) "would favor an increase in ice extent".

This part will be rewritten in the revised version. The reference to the maybe "surprising" result will be suppressed. The discussion regarding the potential role of the heat content between 100 and 500m will be modified in the revised version:" Besides, a lower initial heat content between 100 and 500m (Fig. 5c) could contribute to an increase in ice extent as it stabilizes the water column and reduces the amount of heat available to melt sea ice or delay its formation. The signal is, however, not significant at high southern latitudes in our experiments, except in the Pacific sector."

-> Page 4597, lines 20-21: the described mixing process is due to static instability not "diffusion". Line 23: change "identical if sea ice is present or not" to "independent of sea ice".

'by diffusion" will be suppressed in the revised version and we will replace "identical if sea ice is present or not" by "independent of sea ice" as suggested in the revised version.

-> Table 2: "level" should read "layer". The latent heat of fusion of water is normally around 3.3x105 J/kg (e.g., Washington and Parkinson, 1986).

Actually, we prefer the term level as layer is often associated with layered models in which each layer has a constant density but variable thickness but thanks for pointing out the inconsistency with the main text where we have modified all the 'layers' by 'levels'. For the latent heat of fusion, we used the value proposed by Martinson et al. (1981), their page 474, for consistency with this study. Martinson et al. (1981) chose this value, which is indeed lower than the one used in many other studies, to take into account the influence of the sea ice salinity. Putting L=3.3 105 J/Kg does not change at all the results of the model (we only have a ice thickness that is smaller) but we will select this value for the revised version.

-> Page 4599, lines 4-5: "the top layer is always stable" doesn't make sense in this context Fig.9a, caption: K should be in °C.

This could be replaced by "the top layer is always less dense than the bottom one, leading to a stable stratification throughout the year", but the whole section will be modified in the revised version. The caption of figure 9 will be changed in the revised version.

-> Page 4601, lines 21-24: referring to the trend in ice extent and its value being much

higher. I guess what is meant here is that the rate is much lower in the assimilated than in the non-assimilated simulations.

The value of the trend in ice extent with data assimilation is -38000 km2 per decade while in the simulation without data assimilation it is -152000 km2 per decade. The trend in the simulation with data assimilation is thus higher. However, this can be confusing as in absolute value the trend is lower or in other words we have a lower decreasing trend with data assimilation. This part will be rephrased to avoid confusion: " The simulated trend in this experiment is, however, much closer to the observed one than the mean of an ensemble of simulations performed with LOVECLIM without data assimilation"

-> Page 4602, lines 19-21: it is hard to compare Fig.12b with Fig.3c since different scales are being used. Lines 21-22: I cannot reconcile from Fig.12d that "the depth of the mixed layer is decreasing at high latitudes". Lines 24-25: I can also not see how the decrease in surface salinity is supposed to be "associated with an increase in the oceanic heat content at depth". The corresponding trends seem spatially uncorrelated. Lines 27-28: What is "those results" referring to? Assuming these are referring to Fig.12, how are these results "compatible with the combination of a general temperature increase and freshening at high latitudes"? Both are not shown. Only the decrease in surface salinity indicates a freshening.

We will rewrite this section in the revised version of the manuscript and use the same scales for figure 12 and 3 for an easier comparison. The sentence "the depth of the mixed layer is decreasing at high latitudes" was indeed not specific enough and we will be more precise in the revised version. We were not suggesting a local link between the freshening and the heat content so we will make this clearer in the revised version too. Finally, the warming was mainly based on the results of the simulation without data assimilation that are nearly not discussed here so we will not refer anymore to this point in the revised version.

-> Page 4603, line12: “except the salinity increase in the Bellingshausen Sea” referring to observations: a few lines before it says that the salinity increase is what is being observed. There is a similar contradiction with the sentence at the bottom of this page and the last sentence of that paragraph.

Indeed the observations suggest a salinity increase in the Bellingshausen Sea but the model simulates a freshening. This is what we meant but, as it is not clear, we will modify it in the revised version to make it more explicit. The organization of the last paragraph of this section will be modified in the revised version to remove the apparent contradiction.

-> Page 4604, line 8: it seems better to write “Each 30-yr period member of a ...”, and line 11 “that differ among members.”. Lines 21-22: the “downward transport of salt” due to brine release would need to change in order for a long-term change in stratification to occur. How do you suggest more brine release to occur with warmer surface conditions (see main comment)?

We prefer avoiding referring to the different periods as members here as we keep this wording for the members of ensemble of simulations (with and without data assimilation). We will, however, modify this sentence in the revised version to make it clearer. The conclusions will be reorganized in the revised version to make the role of the positive feedback more explicit (see answer to main comment). We do not suggest that more brine will be released in warmer conditions but that the brine release during one winter can induce a long-term change in the stratification that allows sea ice anomalies to be sustained at decadal timescales. We will also stress that the downward transport itself is not responsible for the trend but provides a feedback that amplifies the perturbation and thus 'control' the magnitude of the trend.

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