The Cryosphere Discuss., 7, C2767–C2773, 2013
www.the-cryosphere-discuss.net/7/C2767/2013/

The Cryosphere
Discussions

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 first to thank the Referee for his/her positive evaluation and for his constructive comments. We will take them into account in the revised version of our manuscript as detailed below.

1. Page 4591: The authors used the last 1000 thousand years of a 5000 year simulation. Was any part of the first 4000 unsuitable? If yes why? What does "stable" mean and how is it defined? Could you comment some more on the differences in variability of the 11 thirty year periods – for example could feedbacks other than the ones discussed later on in the paper be involved here?

We used the last 1000 thousand years of the simulation for practical reasons. The first

4000 years were performed as a standard control simulation for the model, independently of this study. Unfortunately, not all the variables required in the present analysis were saved at the needed frequency. As a consequence, we simply prolonged the run for 1000 additional years with the adequate outputs. Some parts of the first 4000 year were probably suitable for the analysis but this experimental design seems simpler to us. During those additional 1000 years, the drift in the global mean ocean temperature is smaller than 0.01°C per century. This is of the same order of magnitude as the internal variability of this variable and is assumed here to be small enough to consider that the system is stable. This will be mentioned in the revised version of the text.

Each period is different but we wanted to focus first on some common robust points rather than on the differences. Some differences are for instance illustrated in Figure 5 and 6. In order to reduced the number of figures, and as suggested by Referees 1 and 2, the majority of the material included in those figures will be moved to Supplementary Material in the revised version. We considered than discussing in more detail each of the periods would increase the length of an already long manuscript. Besides, discussing some other feedbacks is a very interesting suggestion. It is out of the scope of the present study to discuss and compare all of them, in particular as they have been the subject of many studies. But, in the revised version, the role of the other feedbacks will be mentioned more explicitly in the conclusions.

2. Page 4592: Figure three poses a problem. The plots do not come in the order that they are referenced in the text. This must be changed. Also, in relation to figure three you have a spatial correlation of 0.72 between the trend in sea ice concentration and the heat flux at the surface. Given that the signs of these trends are different, how did you get a positive correlation? Is that just a typographical error?

In the revised manuscript, the panels will come in the order of the reference in the text. Thanks for noticing the error in the sign of the correlation. The correct value is indeed -0.72. This will be corrected in the revised version

3. Page 4593: lines 5-6, do you mean the simulated freshening? Do you mean to say that the simulated freshening cannot be related to the precipitation? Lines 8 – 10, the increase in precipitation as snow does not always occur close to the ice edge in Figure 3j and the pattern in Figure 3j is more heterogeneous than, for example, the SIC. Can you comment on this? Lines 14-15, Figure 3k has to be enhanced to show this better. The trend, although significant, is quite weak and difficult to see.

Yes, we mean to say that, as precipitation is decreasing, it cannot explain the simulated freshening. The wording will be modified in the revised version to make this clearer. For snow, two opposing effects are playing a role. On the one hand, the lower total precipitation reduces the amount of water available and on the other hand, because of the cooling, a higher fraction of precipitation comes in the form of snow. As a consequence, snow precipitation increases in some regions, mainly at lower latitudes, while it decreases in many areas closer to the continent. This will be specified in the revised version of the manuscript. The color bar of figure 3k will be modified to see more clearly the patterns.

4. Page 4594: Lines 7 – 10, This makes me wonder if a seasonal analysis of these results would show something different. Was that attempted? Figure 4 is potentially a very useful diagram to support the authors' arguments. But is does not do a very good job of illustrating the points being made on page 4594. It needs to be better annotated. This can be done without too much reduction of the simplicity of the diagram.

Seasonality is indeed a key issue in the system. We have thus made several diagnostics for the different seasons. We obtained the expected results that, for instance, a larger ice production in winter destabilize the water column or that sea ice convergence stabilizes it and increases locally the mean ice concentration. However, this was not easily related to the decadal trend of the ice extent. In the feedback discussed here, this is not each season individually but the interplay between the seasons that is suggested to play the dominant role, in particular the net convergence of sea ice on an annual basis or the net vertical transport of salt resulting from the sea ice formation in

winter and melting in summer. This is the reason why we have focused here on the annual means that describe this in a more synthetic way. The diagram of Fig.4 will be modified to illustrate better the main points of the discussion and the discussion of those important elements will be modified in the revised version (see for instance the answer to point 8).

5. Page 4595: Given the differences seen in geopotential height fields for each 30 year period (which is not surprising to see) did the authors also look at similar statistics for the variables shown in Figure 3? Those trends seem coherent but are they driven by similar variations in all of the periods or just a subset? The authors should give some idea of how coherent their trends in Figure 3 are. Figure 6 – only one of the plots in Figure 6 is referred to in the text. All of the others are therefore not necessary so the authors should redraft this diagram. Lines 21 – 24, Since trends are defined with respect to the mean state, is this an unexpected result? I'm not saying that it is not a useful outcome but it is something that I would expect. I think the authors should comment on whether or not this outcome is expected and how important it is.

The geopotential height is certainly the variable for which the difference between the periods is the largest. This is reflected by the fact that nearly no change is significant for the mean. For the other variables that play a key role in the mechanism discussed, more changes are significant. Those variables are also generally well correlated with ice concentration when analyzing separately each period as illustrated in Table 1. We have analyzed all the variables displayed on Figure 3 for individual periods. Going into the details in those aspects will require a lengthy discussion without bringing a lot of useful information to our point of view. In that framework, we will strongly reduce the number of panels of Fig 6 in the revised version as suggested by the Referee. Nevertheless, we will mention more explicitly that, for other variables, the averages proposed on figure 3 represent a better estimate of the situation during individual periods than for geopotential height, as seen for instance for the ice concentration.

Yes, it is indeed expected that the initial state influences the system. The 30-year trend is however defined locally (in time) in the way we compute it. So, only the mean over the 30-year period is taken into account, not the mean over the full 1000 year period. The result that the mean over those 30-year periods is more or less the same as over the 1000 year period was thus considered useful to mention but this is not central to our discussion. This will be specified in the revised version of the manuscript.

6. Page 4596: Lines 1 – 3, Since sea ice extent is defined from sea ice concentration wouldn't you expect that outcome? The rest of that paragraph needs to be edited for clarity. It is a bit difficult to follow. This difficulty is amplified when trying to associate Figure 7 with the text. The Caption in Figure 7 suggests that we are looking at averages but the legend (colour bars) suggest to me that we are looking at anomalies. Anomalies make more sense. The authors need to clarify this.

Yes, of course, sea ice concentration and ice extent are expected to be related. This will be clarified in the revised version. Thanks for noting the imprecision in the caption of Fig. 7. It should have been "Anomalies compared to the 1000-year mean averaged over the first ...". This will be corrected in the revised version. The paragraph describing this figure will also be edited as suggested by the Referee.

7. Page 4599: Figures 9 and 10 are very important for illustrating the points made on this page an on the one following, but the text does not do a good job of referring to or explaining the diagrams. Perhaps the diagrams need better annotation or the captions should have more explanation.

The discussion of those figures will be modified in the revised version and we will add in the captions one sentence explaining the main characteristics of the experiment. In particular, we will make a more explicit distinction between the processes occurring during the first years of the simulation and the equilibrium at the end of the simulation (see also the point below).

8. Page 4600: It appears that brine release has opposing effects on the stability of the water column on seasonal vs. interannual timescales. Is there a difference in

magnitude? How does a trend get established then?

The description of the role of the brine release on the stability of the water column will be modified in the revised version. To do so, we will include a third level in the simple model. On the seasonal time scale, brine release destabilizes the water column but the associated downward transport of salt induces a decrease in surface salinity. After a few years, this salinity decrease finally stabilizes the water column. The time scale of the stabilization depends then on the interplay between the magnitudes of the release of brine in winter, the annual export of salt out of the top level, and the exchanges with the deeper layers.

9. 4602: Lines 10 – 15, Despite the fact that the authors' intent is not to examine the details of the sea ice concentration (SIC) differences, it is true that SIC being what it is will moderate or have some impact on their results. I think the authors should at least comment on the direction of that influence. They can make a suggestion of that without straying from their main intent. Figure 12 suffers from the same problems as earlier Figures. The variables need to be rearranged to represent the order in which they are referred. On this page, Figure 12f is referred to after Figure 12a but before any of the others. And Figure 12f appears to show geopotential height, not wind. Lines 14–30 need to be edited for clarity and it would help if the legends for all of the variables in Figure 12 (on which this paragraph is based) be normalized somehow so that the reader can expect some consistency in the signs and the colours used to represent the signs. As it stands it is difficult to compare the figures as now presented. This is also true for earlier Figures, like Figure 3.

The figure 12 will be modified as suggested by the Referee. We will, however, keep the same color code for the signs to have the same ones as for Fig.3. Furthermore, the description of the simulated changes and the discussion of the impact of the difference between the simulated and observed trends will be reorganized and expanded in the revised version to make it clearer and to briefly mention the potential impact of the biases of our simulations on our analysis.

10. Page 4604: Lines 14 – 16. This conclusion should be couched in terms of “possibility” not “probability” to be precise, since establishing probability would require a more detailed statistical analysis that is done here.

As suggested by the Referee, we have removed the word "probability”. The sentence will be modified to "First, a low initial sea ice extent favors a subsequent large increase since a return to mean conditions would already contribute to this increase. This condition is, however, not required in all the cases analyzed."

References: Van Leeuwen, PJ is mentioned in the references but not in the text. Perhaps it belongs in the section on assimilation?

Is the citation Santoso and England 2006 in the text the same as Santoso, England and Hirst, 2006 mentioned in the references? They are different so the citation in the text should be reconciled with the references.

van Leeuwen 2009 was incorrectly referred to van Leeuwen et al. ,2009 in the submitted version. Santoso and England 2006 should have been Santoso and England 2008 and the corresponding reference should replace Santoso et al. 2006. This will be corrected in the revised version.

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