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Interactive comment on "Decadal trends in the Antarctic sea ice extent ultimately controlled by ice-ocean feedback" *by* et al.

Anonymous Referee #2

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This paper presents a feedback that the authors argue plays a role in the development of decadal trends in Antarctic sea ice extent. The argument is based on long the results from a long control run of the LOVECLIM model whose coarse resolution and lower complexity (compared to standard CGCMs) allow very long, low cost runs. From the last 1000 years of the control simulation they choose and examine 11, 30year periods during which the sea ice extent exhibit a positive trend. The compare these with the results from the same model where the latter has been constrained to follow observed changes in sea ice extent. From the outcomes of these comparisons they develop a simple 1D model to illustrate how the long term stabilization of the water column can occur as ice forms. Their two main conclusions are that the probability of a positive trend in sea ice extent is larger when sea ice extent is small and that larger sea ice extents occur along with a stabilization of the water column, increased storage

of heat at depth and a reduced vertical oceanic flux. The implied feedback in the association of these four variables is powerful enough to generate and maintain multi-decadal variations in sea ice extent. The work presented in this paper joins the debate on causes underlying the recent increase in Antarctic sea ice from the perspective of the ocean. The feedback discussed, represents a valuable and plausible input to this debate. The material discussed in the paper is very suitable for publication in Cryosphere but there are some questions that I think the authors should consider and respond to before the paper can be published. These questions concern the science of the paper as well as some structural (the Figures) issues.

The questions/comments follow and are present as they occur in the paper, not in order of importance.

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?

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?

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.

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.

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 given 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.

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.

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.

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 magni-

tude? How does a trend get established then?

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.

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.

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.

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