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Interactive Comment

Interactive comment on "Influence of the Southern Annular Mode on the sea ice-ocean system: the role of the thermal and mechanical forcing" *by* W. Lefebvre and H. Goosse

W. Lefebvre and H. Goosse

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Dear Dr. Stevens, dear reviewers,

first of all, my co-author and myself want to express our thanks to you and the reviewers for the swift response and the constructive criticism. Please find enclosed the response to the comments.

We will adress the referees' comments in the order we received the reviews.

Reviewer #3:

General comment: The first version of the text was probably not clear enough about the experimental design. As a consequence, we have modified section 2 (section 2.2, end of first paragraph) and section 3 (section 3.2, end of first and second paragraph) to make it clearer. We are not analysing the response of the system to SAM during a par-

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ticular period (i.e. 1980-1986) but the response of the ice-ocean system to an idealized forcing having the same characteristics as SAM, focussing on subdecadal time scales. The interannual variability of the forcing is kept only in order to have a simulation which is as close as possible to the one presented in Lefebvre et al. (2004), for an easier comparison with the results of that study. The goal of section 3 is to demonstrate that analysing 7 years is a good compromise and provides a robust response. Analysing, a different period (e.g., starting an idealized experiment later and focussing on years 1981-1987) would give similar results. Furthermore, as our forcing is deduced from regression with the SAM index over the period 1980-1999, it is reasonable to compare the response to this perturbation with observations over the same period (Figure 4).

Figure 2: OK. This has been done in the caption of Figure 2.

Figure 3: OK. Sorry, there was an error in the caption (see also Figure 5). It has been corrected. The standard output of the OPA model is 15 points per year. As the time interval has no particular influence on our conclusions, we have kept this standard output for simplicity.

Figure 4: A, b, ... have been incorporated on the figures as suggested. About the difference between figure 4 a and b: The difference between the model and the observations have been discussed in Lefebvre et al. (2004). They are in general small, see also the positive patches that are almost identical in model and observations. Nevertheless, we have added in the new version a sentence in order to recall the differences between the model and the observations (Section 3.1, end of first paragraph).

Figure 5: OK. Problem solved.

Sec 3 line 11: See general comment.

Sec 4: As suggested, this is probably linked to the differences between model and observations (see also discussion on Figure 4 and on the general comment). This has also been introduced in Section 4 (Line 6).

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Reviewer #2:

We agree with the reviewer that our experimental design has limitations. As suggested by the reviewer, we have added a paragraph in the conclusions to underline more strongly this point. In particular, we are not able to take into account any feedback between the ice-ocean system and the atmosphere. In our experiments, the anomalies are imposed without taking into account any atmospheric processes in the model. Nevertheless, as those anomalies are deduced from observations, they are obviously compatible with the atmopheric dynamics of the real world. Furthermore, we have to decide more or less arbitrarily the way "mechanical" and "thermal" forcings are defined. There is probably no perfect way to seperate them as in the real world they are linked. This can be direct (e.g. northern winds are usually warmer in the Southern Hemisphere) or indirect through feedbacks. We have made the choice here to relate the "thermal" forcing to temperature changes and the "mechanical" forcing to wind stress changes, a choice that appears natural to us because of the way the majority of sea ice–ocean models are driven. We are pleased that both reviewers consider this choice justified as both judge that the paper is interesting. (see conclusions, last paragraph)

On the subdecadal issue: OK. See the first sentence of the conclusions and the last sentence of the second paragraph of section 3.2.

No attempt was made to assess the significance of the regressions because we analyse here the response of the system to a perturbation. We have shown that this response is robust in section 3. Furthermore, we have shown that the response to this perturbation is very close to the anomalies associated to SAM in the model. A different issue, which is of course related, is to determine if the anomalies simulated correspond to a large fraction of the total variance of the system. This point was discussed in Lefebvre et al. (2004). This is now clearly mentioned in the new version of the text (Section 1, end of second paragraph). We also clearly stated in the paper (chapter 2.2, 4th alinea, 8th line) that the correlation between the SAM and the atmospheric temperature is low. So, it is not sure that the temperature changes associated with SAM for the last 20 years are robust. However, as we do not have reliable and complete data for the period before the satellite era, we have to assume that the pattern is the most plausible temperature pattern to be connected to the SAM.

On figure 6: As the differences in response to the initial conditions is limited to the first two years (whatever the begin year we take) (Figure 7), the feedback (non-linear) between the SAM trend and the SAM anomaly seems to be negligable. (see also Section 3.2, end of first paragraph)

Interactive comment on Ocean Science Discussions, 2, 299, 2005.

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