Atmos. Chem. Phys. Discuss., 12, C5952–C5956, 2012
www.atmos-chem-phys-discuss.net/12/C5952/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Implications of all season Arctic sea-ice anomalies on the stratosphere" *by* D. Cai et al.

D. Cai et al.

duy.cai@dlr.de

Received and published: 21 August 2012

We would like to thank the reviewer for refereeing our paper and providing helpful suggestions to improve the manuscript. In the following we expand on each comment explaining in which way the referee's advice has been considered.

"This paper addresses a topic that has been very little explored. I find the paper suitable for publication after some revisions. The manuscript would also gain by getting more polished, and I would recommend that a native English speaker proof-reads the article."

The complete manuscript has been revised; in particular we have gone over the grammatical style and wording. A native speaker has checked the text.

"Major comments Most studies (albeit not carried with a chemistry-climate model re-

solving the stratosphere) indicate a tendency for a more negative winter-mean NAO or AO with reduced Arctic SIE (e.g. Francis, GRL, 2009). This study on the other hand tends to show an enhanced positive AO, and a strengthened vortex, at least in November. This may indicate that the seasonality of the response is important. In fact, a recent study by Orsolini et al. (Climate Dynamics, 2012) dealing with the impact of the reduced SIE in 2007 in coupled ocean-atmosphere model, did show a response extending into the stratosphere in autumn. In November, their response also showed a weakened Aleutian Low, in rough agreement with this study. Consequently, I think that Section 3.5 should be improved focusing the discussion on what is statistically significant. The link with that study and previously mentioned studies should be made clearer."

Over the extended winter period (Nov-Mar) our model indicates a statistically significant change (at the 99 % level) in the NO-ICE sensitivity simulation towards a more neutral AO phase. Only in early winter (Nov) a statistically significant (at the 99 % level) shift to enhanced positive AO-index is identified. To our understanding the seasonality (i.e. temporal evolution) and the spatial pattern of SIE changes affects atmospheric response, including a modification of the NAO/AO index; the discussion about this has been extended in the revised manuscript. Published studies do not indicate a clear picture regarding expected change (shift) of the AO-index in a future climate with less sea-ice. A possible explanation is that in each of these studies a different approach regarding SIE changes (temporally as well as spatially) has been used. As requested, we have included a more detailed discussion about the statistical significance of our findings and relations to other similar studies. Moreover, we have added a comparison with results presented by Orsolini et al. (2012). We did not know their paper when writing our paper. Since there are obvious similarities with our findings, the consideration of the Orsolini et al. results and a comparative discussion is an obvious surplus value.

"The significance should be incorporated in the Fig 9 and 10. I gather that the Nov-Mar change in the AO distribution (indicating a tendency for a more neutral or negative

phase) is weakly significant, and that only the November distribution change is significant at the 95% level. The latter result is consistent with the rest of the paper. Why keep Fig 9 if it is inconclusive?"

As mentioned before, the frequency distributions of the NO-ICE and REF simulations differ in both figures (now Fig. 11a and 11b) at the 99 % (!) significance level, i.e. are both highly significant. This is the reason for keeping both figures in the paper. More detailed explanation of the results are given in the revised manuscript.

"The following three sections should be re-written for clarity of the result presentations. The section 3.4 on the meridional heat flux needs to be clarified. In order to follow the discussion on the heat flux influence, what should be shown are the separate stationary and transient contributions, if the latter is significant. The wording should be improved in that section. What the authors are discussing is the *zonal-mean* meridional eddy heat flux, and over a broad latitude range (40N-80N), not just over middle latitudes. The figures should be introduced less passively."

Sections 3.4 and 3.5 have been revised according to the referee's suggestions; particularly the wording has been edited. In section 3.4 (meridional heat fluxes) we extended the discussion to a possible influence of seasonality comparing our results to other studies including Orsolini et al. (2012). A better clarification of the meridional heat flux and the following discussion is gain by additionally showing the transient component. We acknowledge this comment. Using the meridional eddy heat flux as a measure of wave activity we follow the idea of Newman et al. (2001) utilising the mean value over the latitudinal range from 40° to 80°N; these values certainly do not only represent wave activity at mid latitudes; the wording has been changed accordingly.

"Summary. The summary would gain by being more ordered, avoiding back and forth presentations of the results."

Section 4 (Summary and conclusion) has been reworked. We splited this section into a a) discussion part (now Sec. 3.6) making clearer the major findings, including a

valuation of uncertainties and general conclusions which can despite all this be taken from this study - and b) a summary which conclude very short our main results and give a brief out look.

"It would also gain by stressing the differences with other studies, e.g. on the sign of the AO response. As an additional note, the key issue remains how to explain the weakening of the Aleutian Low by the SIE reduction, through transient eddies or other mechanisms. This is not clearly addressed by this paper. Could the authors comment on this?"

The revised manuscript includes now a more detailed discussion about the possible impact of the pattern of sea-ice extent changes on atmospheric response, namely the impact on AO/NAO etc.; a discussion about this topic has been also incorporated at different places in the manuscript (see particularly sections 3.5 and 3.6).

"Minor comments L13-15: Make clearer that only in November are results significant, and that the winter-mean AO changes are very weakly significant."

We change the wording to clarify the statistical significance.

"Figs.3 and 6: Please label the x-axis similarly in all figures, for ease of comparison."

Since the main interest of this paper are the dynamics we want to show a qualitative comparison to chemistry. Ozone concentrations are monthly means which is different to the other daily analyses, so we kept the old x-axis labels.

English / phrasing / typos L15: analogue -> use related or congruent with done; L32: temporal done; L42: to reduce planetary waves, while contrarily. . .. done; L43: Trends. . . are done; L54 : conditions. . .., a potential stratospheric feedback. . ... done; L63: remove *they cause* done; L104: invariant is not appropriate here. Unchanged is better done; L114: "North Polar sea" is not standard usage. Also *Sea of Okhotsk* done; changed North Polar sea into center of Arctic Ocean, Sea of Okhotsk is no standard usage but remains unchanged, since we gave an indication of its location.

L121: rephrase as unclear. *Analysis* done; L122: all-season is used twice done; L143: use *dual* rather than diametric done; L155: the seasonal changes in ocean-atmosphere temperature gradient done; L169: *descent* of reflecting radiation ? Use downwelling ? done; L171: as a consequence of done; L179: the well-understood temperature depencies done; L184: reveals or displays, not *offers* done; L185: which continues done; There are numerous grammatical and English mistakes in Sections 3.4, 3.5 and 5. See above

---

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12423, 2012.