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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

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We would like to thank the referee for the very detailed notes and comments and providing suggestions to improve the manuscript. In the following we expand on each point raised by the reviewer explaining in which way the referee's advice has been considered.

"In this study the authors consider the impact of Arctic sea-ice reduction on the dynamics and ozone chemistry of the lower and middle stratosphere. The results are based on two atmospheric GCM simulations with a repeated annual cycle of externally imposed forcings (e.g. SSTs and sea ice). The reference run, REF, employs forcing representative of the year 2000. The second simulation, NO-ICE, is identical to REF except that the average annual cycle of sea-ice representative of 2089-2099 from a

future scenario run of the HadGEM is employed. In the NO-ICE simulation, the sea-ice cover at all times is necessarily smaller than that in REF. Consequently, the issue of what to specify for SSTs in the "gap" region arises. In principle, since the forcings in all other locations are identical between the two runs, the change in properties of this gap region (e.g. albedo and SST) critically determine the model response analyzed in this study. While the change in albedo is straightforward, due to the design of the experiment, there appears to be no physical rationale to guide the decision for what to specify for SSTs in this critical gap region. For this reason the experiment seems to be ill posed. The authors state only that an interpolation of SSTs between year 2000 and 2089-2099 was used in the gap region and offer no further explanation or justification."

The reviewer is right that we have not explained sufficiently how we have filled the gap region with sea surface temperatures arising when reducing the sea-ice extent in the NO-ICE simulation. This has been made up for now in the revised manuscript. Moreover, we agree with the referee that the choice of sea-surface temperatures in the area where sea-ice is removed should affect the atmospheric response. A discussion about this point has been added in the manuscript. In particular, a deepened confrontation of our model results with those available in the literature may hopefully convince the reviewer that the choice of the lower boundary condition in NO-ICE is reasonable although artificial, but in this case not "ill posted". In addition, to nullify the referee's assumption that the NO-ICE experiment is useless because of the chosen SSTs in the "gap region" we have added a comprehensive comparison with respective results presented in Scinocca et al. (2009) and Orsolini et al. (2012) which offers additional hints that our conclusions are by all means plausible, at least in parts matchable with results from similar studies presented recently.

"The authors identify a previous study (Scinocca et al. 2009) where a fully coupled atmosphere-ocean CCM was used to investigate the impact of Arctic sea-ice loss on the dynamics and ozone chemistry of the stratosphere. That study suggested that the stratospheric perturbative response maximized in, and so was localized to, March.

The present study seems to be at odds with this previous work showing a stratospheric response throughout the annual cycle and arguably maximizing in November. The authors have suggested that, relative to this previous study they are showing the response in seasons other than March. However, they have not attempted to first explicitly verify that the two studies agree in their March response, which would go a long way to validating the authors' experimental design. Furthermore, the authors have not acknowledged the fact that the appearance of a response in all seasons is essentially at odds with the previous study of Scinocca et al (2009), nor have they offered an explanation for their differing result."

We do not think that our results are in conflict with the findings of Scinocca et al. (2009). Based on our (simpler) study, where only the SST/SIC conditions are changed in the Arctic region and not the GHG concentrations etc., we found the maximum (statistically significant) response in the polar stratosphere in November/early December. The changes between NO-ICE and REF identified for mid/late winter conditions are not statistically significant. Looking to the responses in March we found nearly the same temperature and ozone change patterns as Scinocca et al. (2009) – their Figure 2 (bottom figures): (1) A pronounced cooling in the Arctic lower stratosphere and a weak warming at mid-latitudes of the lower stratosphere, but in contrast to Scinocca et al. the temperature differences are not statistically robust; and (2) a decrease of ozone mixing ratios in the polar lower stratosphere, again very similar change pattern as presented by Scinocca and colleagues, but statistically not significant in our model. We have added the figures (see Fig. 10) from our analyses to the paper and discuss the similarities of the response presented in Scinocca et al. (2009) in detail. An additional mentionable result from our analysis is that the patterns of temperature and ozone differences (NO-ICE - REF) for February and April are very different supporting our finding that we do not have robust responses in the stratosphere in winter months to an Arctic sea-ice loss (in summer and autumn). Nevertheless, the fact that the both investigations (i.e. Scinnoca et al. and our study) show the "same" signatures in March is very interesting and may stimulate further analyses.

"My concern is that this study differs from previous work because it considers an ill-posed experiment where an arbitrary choice of SST is required in the gap region in the perturbed simulation. A response that depends on an arbitrary choice of SST in the gap region is not really interesting or publishable. For this reason I cannot recommend publication of this study in its current form. I recommend major revision before this study be considered for publication in ACP. My detailed comments follow. In these detailed comments I have also added a suggestion for a variation on the authors' AGCM experiment, which seems better posed."

As mentioned above, we do not think that our results are in general conflict with those of Scinocca et al. and other investigations dealing with consequences of SIE changes. (E.g. see also the comparison with the recently published work by Orsolini et al. (2012) which is now also included in our paper.) Although the model set-ups of Scinocca et al. and our study are very different, there are obvious similarities in atmospheric response (not only the March response, but also the near surface response). We hope to convince the reviewer that our numerical sensitivity study with E39CA is providing useful and interesting results which are worth to be published. The overall tropospheric response (especially the temperature response) to the changed lower boundary conditions is very similar to other studies and therefore the chosen model configuration with the adopted changes of boundary conditions should be suitable to estimate possible implications of all season sea-ice changes in the Arctic region. We agree with the referee that the choice of SSTs in the gap region is an issue because it has an effect on the NAO/AO and therefore stratospheric dynamics. But on the other hand the stratospheric response seems to indicate some robust features, e.g. features also popping up in the results of Scinocca et al. and Orsolini et al.. We are far away from drawing any general conclusions regarding future impact of a sea-ice free Arctic on stratospheric dynamics in summer and fall months, but nevertheless we believe that the experiences of our study regarding the stratospheric response to Arctic sea-ice loss are noteworthy. For clarification, our attempt is to investigate whether there are any existing stratospheric changes due to a large reduction of sea ice cover.

”Major Comments: 1) Validation: The Scinocca et al. (2009; S09) study was a fully self-consistent experiment. In changing the albedo of the sea-ice in the perturbed experiment, all aspects of the subsequent response were modelled: the ice actually melted and released its store of fresh water, the ice froze and remelted with each annual cycle, and a consistent SST response was modelled. In this study the authors use only an AGCM and impose a sea ice change (and arbitrary SSTs in the gap region) as the only perturbation. As discussed in my main comments, this is fairly artificial and potentially ill posed if the response is at all sensitive to the choice of SSTs selected for the gap region. In arguing that they are extending the S09 study the authors make the tacit assumption that the response in their model is consistent with that found in S09.”

We highly recognise that Scinocca and colleagues are using a comprehensive model system and compiling fully self-consistent numerical simulations. From this point of view our model approach is much simpler. This is now stated in our manuscript. As mentioned above, we do not agree with the reviewer’s statement that the choice of SSTs in the NO-ICE experiment, particularly in the gap region is simply wrong (although artificial) and therefore useless. Moreover, it was not our intention to say that we are providing an extension of the Scinocca et al. study. May be the wording was misleading. The text has been changed accordingly.

”The authors need to explicitly validate the response in their model against S09 by attempting to reproduce a few seminal figures for March (i.e. the zonal crossections of Fig.2 and the top-left panel of Fig.3 in S09). That would provide some assurance that this less realistic setup still captures the leading order response of the more complete system.”

Done! We have made illustrations respective to Figures 2 and 3 in Scinocca et al. (2009). The results are now shown in Fig. 10 and discussed in the manuscript (see also statements above).

”2) Experimental design: As described in my main comments, there is the potential for

the perturbative response in the present experiment to be sensitive to the choice of SSTs specified in the gap region. The authors first need to acknowledge this potential problem and then they need to investigate it by possibly trying several (very) different approaches to specifying the SSTs in the gap region. It’s not even clear what the authors have done by their one statement "These gaps were filled by interpolation of SST values of the present and the future." There are no present-day values for SSTs in the gap region. So the authors must somehow be interpolating in space rather than time. Whatever they have done, it is clear that the SSTs they have inserted into the gap region are much warmer than the ground temperature there in the REF simulation. This is due to the fact that they have been derived from SSTs that have been subjected to nearly 100 years of greenhouse gas warming. The large Nov-Feb response, discussed in some detail by the authors, would seem to be directly connected to the perturbative warming associated with their choice of SSTs in the gap region. (During this time of year, albedo changes would have little/no impact since there is little/no sunlight.) If so, then a different choice of SSTs would provide a different response at this time of year. Since there is no physical rationale for the choice SSTs in the gap region in this experiment, a response that is directly due to the SSTs in this region is by definition spurious.”

The “potential problem” and its possible consequences are now discussed in the paper, and hopefully the reviewer accepts our arguments. Certainly it would be a possibility to try some different approaches to fill the gap region. But this suggestion could be also given to many other studies (using specific sea-ice perturbation pattern to analyse its impact on meteorological values and parameters); we believe that our discussion following other papers, i.e. contrasting first the pattern of (observed or artificial) sea-ice changes to SLP or SAT modifications, is sufficient to demonstrate that our assumptions are by no means “ill-posted”. Moreover, the detailed comparison with results provided by Scinocca et al. and Orsolini et al. indicates that our findings for the stratospheric response are not baseless and are by all means plausible. But this does not (!) mean that there are no uncertainties in our findings and conclusions. We hope that the statements in the additionally written discussion section (see section 3.6) and at the end of the paper help to make our point clearer.

”One choice that comes to mind for the gap region is to keep the SSTs at the freezing point. That way the surface temperature would not really be a factor in the gap region and the impact would come primarily from albedo changes. However, this is just one arbitrary choice among many and it does not alter the central problem, which is the experimental design.”

Certainly this would be one possibility among several other alternatives to prescribe the SST in the gap region. We agree that a systematic testing of different sea-ice change pattern would be helpful. But repeating what we have said before, we do think that neither our choice of SSTs nor the experimental design is per se useless.

”3) Alternative experiment: I could think of an alternative experiment that the authors could perform with their current setup. It requires two similar simulations but the perturbed run is seemingly better posed. consider the two simulations: A) FUTR - repeated annual cycle run with all forcings set to the average over the period 2089-2099 including both the sea ice and SSTs from HadGEM. B) FUTR_REF_ICE - identical to FUTR but the sea ice cover for the REF period (year 2000) is used instead of that from the HadGEM Now, the perturbative response (A) - (B) represents the system’s response to the same loss of sea-ice but about the future rather than present climate. The advantage here is that one need not decide on SSTs in the gap region because no gap exists. The sea ice area in simulation B is everywhere greater than that in simulation A. This is not completely physical because the SSTs at the sea-ice edge in simulation B will be warmer than they might otherwise be, but it eliminates one of the main conceptual problems with the perturbation run of the current study. If the authors performed this pair of simulations and the response differs from that found in their present runs then they will have some serious questions about the meaning of their present results. Since the alternative experiment suggested here requires no arbitrary SST forcing to be specified, the hope is that it might better resemble the pre-

vious findings of S09 and provide the authors with a more viable experiment for their study. However, if this alternative experiment were more similar to S09, the response would be localized to NH Spring and there may not be anything new to publish here.”

We appreciate the reviewer’s suggestions for additional test simulations very much. Unfortunately we are not able to perform extra simulations in addition to the presented ones. The reason is that the computer we have used in recent years (the DLR NEC-SX6) was switched of some month ago, i.e. we cannot anymore run the E39CA code under comparable conditions. If the reviewer is standing on his/her claim for additional sensitivity simulations that would mean to repeat all simulations on a different computer (with all the required transpositions and tests of the model), and this would delay the publication of our results by several months. (In the meantime we are preparing a new upgraded model version, meaning that the “old” E39CA model will not be fostered any longer.) Hopefully the referee is satisfied with the revised manuscript, especially with the additional material, the discussions and subsumption of our investigations.

”Minor Comments: 1) p.12424, ll.7-8, What scenario is used for the period 2089-2099”

A1B;

”2) p.12424, l.17 change "internal" to "intra-annual" ”

done;

”3) p.12424, l.25 change "considerable" to "considerably" ”

done;

”4) p.12425 ll.20-22 and ll.24-25 this point needs to have a specific reference. ”

done;

”5) p.12426 ll.2-3 change "tropospheric circulation anomalies are of opposite sign" to "AO-index is negative" ” done;

”6) p12426 ll.19-21. S09 state that the largest response is in NH springtime (March). It was not stated that they never look in other seasons or other hemispheres. Have you asked any of the authors of S09 about this? This is an important point considering your response in Nov-Dec is arguably your largest response. Sea-ice albedo would not seem to account for this response since there is little sunlight at high latitudes at that time of year. The surface warming and stratospheric cooling seems suspiciously connected to the use of future (warmer) SSTs in the gap region in your experiments, leading one to believe that it is possibly spurious (see major points 1 and 2).”

See above answers related to the major objections raised by the referee. Additionally we have now discussed the similarities of the change patterns in March presented by Scinocca et al. and respective results taken from our simulations. In the original manuscript we only stated that in Scinocca et al. there is no further information about responses in other months and not that “they never look in other seasons or other hemispheres “(our message/info was meant as a statement and not as a judgment).

”7) p.12426 ll.25-27. Again, you should check with one of the S09 authors since the response was apparently negligible outside of NH Spring. This is an important point to get right since it changes your approach from filling in additional details to explaining apparent inconsistencies between the two studies.”

As mentioned already above we have discussed now our results in connection with other findings available in literature (including the very recent one) which is the normal way how to proceed. We have now made it clearer (hopefully) that the responses derived from our REF and NO-ICE simulations do not indicate statistically significant changes (e.g. in temperatures) in mid-winter to spring. The similarities in March (with findings presented by Scinocca et al.) are eye-catching, but in our analyses turned out to be not statistically robust.

”8) p.12427 ll.16-20 "The present used model version E39CA was part in the exten-

sive intermodel comparison and evaluation project CCMVal-2 (SPARC CCMVal et al., 2010). It has been pointed out an overall good model performance in the upper troposphere and lower stratosphere (Gettelman et al., 2010; Hegglin et al., 2010), which is an advantage when investigating tropospheric-stratospheric interactions." Yes, both Gettelman et al., 2010 and Hegglin et al., 2010 found the E39CA model to be one of the better models but their studies were focused on the upper troposphere/lower stratosphere region of the atmosphere. The UTLS region is not really critical to the present study. Is it? High-latitude stratospheric dynamics and stratospheric polar ozone chemistry are the relevant processes. In looking at the other reference (SPARC CCMVal et al., 2010; http://www.atmosp.physics.utoronto.ca/SPARC/ccmval_final/index.php) where the dynamics, and polar chemistry of E39Ca were analyzed and compared to other CCMs participating in CCMVal-2 (Chapters 4,6, and 10), the E39CA model was more accurately characterized as one of the under performers. I won’t quote the comments here as they are not flattering. They can be found on p.140, p.244 p.406 (i.e. Chapts. 4, 6, and 10 respectively) of SPARC CCMVal et al. (2010). This comment really needs to be change to more accurately characterize the evaluation of the E39CA model presented in Chapters 4,6, and 10 of the SPARC ozone report.”

It was not our intention to say that the CCM E39CA is in general a good model. As other CCMs it has obvious strengths and weaknesses. Nevertheless, E39CA performs well in reproducing tropospheric dynamics and the UTLS behavior incl. the coupling of the stratosphere and the troposphere; both are necessary conditions to carry out such a study. Moreover, the model is sufficiently able to recreate the main features of stratospheric dynamics and chemistry. As suggested by the referee, our comment about the performance of the CCM E39CA has been changed.

”9) p.12428 ll.12-13, The particular scenario needs to be quoted here.”

done; scenario A1B

”10) p. 12429 l.3 For "Meridional seasonal means" do you mean "Seasonal zonal means" ”

is corrected

”11) p.12433 ll.1-2 "...during November to February and hence potential heat release from open waters is comparatively high." Here is a clear indication that the choice of SSTs in the gap region are affecting the response (see major points 1 and 2).”

See above statements about this point.

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