Atmos. Chem. Phys. Discuss., 12, C4474–C4479, 2012
www.atmos-chem-phys-discuss.net/12/C4474/2012/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 10 July 2012

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 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.

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.

Major Comments:

1) Validation: The Scinocca et al. (2009; S09) study was a fully self-consistent experi-

ment. 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.

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.

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.

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.

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 previous 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.

Minor Comments:

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

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

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

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

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

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).

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.

8) p.12427 ll.16-20

"The present used model version E39CA was part in the extensive 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 wont 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.

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

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

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).

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