Anonymous Referee #1

We would like to thank referee 1 for his/her constructive and detailed review.

This is a mostly well written manuscript with interesting new results identifying a stratospheric mechanism impacting the Atlantic Meridional Oscillation. Therefore, it is potentially suitable for publication in the Earth System Dynamics journal. I have, however, a few concerns which I would like the authors to address before I can recommend the publication.

My major concerns are:

1. What would be the impact of aerosols? Your model does not include aerosol interactions, you just simply reduce the solar radiation. This seems a critical simplification to me. You should at least discuss how aerosol interactions would modify the AMOC response if taken into account in your model.

We agree, that the response of the coupled atmosphere-chemistry-ocean model to stratospheric aerosols is the next follow up question, which should be addressed. In this study, however, we focus on response of the system to a direct reduction in the solar energy input (i.e. total solar irradiance). A reduction of the TSI takes place during grand solar minima (e.g., Dalton Minimum) or in the case of solar radiation management techniques taking place in space (e.g., reduction of the TSI by mirrors in space). The model includes also aerosol interactions and indeed a number of modelling studies on the response to stratospheric aerosols from volcanic eruption have been performed earlier (e.g., Anet et al, 2013, Muthers et al. 2014 and 2015).

A comparison between both approaches, a reduction of the TSI space and through stratospheric aerosols is, however, highly relevant. We therefore discuss possible effects of radiation management by stratospheric aerosols at the end of the submitted manuscript.

"*The dynamical effect is expected to change, however, when the solar radiation is reduced in the Earth's atmosphere, for instance, by stratospheric sulphate aerosols. In this case a strengthening of the NH polar vortex and a positive phase of the AO may develop, analogous to the response to strong tropical volcanic eruptions (Graf et al., 1993; Kodera, 1994; Stenchikov et al., 2002; Muthers et al., 2014a, 2015). This effect of the positive AO phase may, in turn, lead to an intensification of the AMOC. Future studies shall address the influence of stratospheric sulphate geoengineering on the AMOC and the possible role of chemistry-climate interactions.*"

2. I think in reality the salt rejection from the sea-ice growth is rather small and mainly occurs north from the regions of deep convection. Therefore, it has only a minor importance to the deep convection and the AMOC compared to the heat loss and possibly the net precipitation (precipitation minus evaporation) at the ocean surface. At the moment, the reader is led to understand that the salt rejection is at least as important as the heat loss. The increase of the sea-surface salinity could also be due to a decreased net precipitation related to changing storm tracks, for example. To better support the salt rejection argument, you need to quantify the salt rejection to the surface density and compare it to other factors. Can you check the ocean surface fluxes from your model output and their relation to the T and S, not only density, anomalies? How realistic these modelled fluxes then

are, depend on your model skill and are related to your model configuration, such as the sea-ice salinity scheme.

Thank you for this comment. We will address the importance of salt rejection in the revised manuscript. Probably, our statements were a bit too strong at some points and may require clarification.

3. I have a problem when you treat the AO and NAO identically. Although the AO and NAO correlate, they are not identical, not even from the AMOC perspective. I agree that the AO behaves largely like the NAO in winter. If, instead of the AO, you based your analysis on the NAO, how would the results look like? What would be their significance after taking into account the possible year-to-year autocorrelation?

See in comments below.

Minor comments:

- Page 1, line 16. I would rather say that 'surface currents transport water into the northern North Atlantic' rather than to 'Northern high latitudes' which sounds more like to the Arctic Ocean.

We changed 'Northern high latitudes' to 'North Atlantic'.

- Page 2, line 7. I don't think the AO is the hemispheric equivalent of the NAO. The NAO is a regional index and correlates with the AO, but their definitions differ substantially.

We have deleted the phrasing 'hemispheric equivalent'

- Page 2, lines 18. '... by increasing SSTs and enhancing freshwater input ...'

Rewritten to: "An increase in the solar forcing has been found to weaken the AMOC by increasing SSTs and enhancing freshwater input (Cubasch et al., 1997; Latif et al., 2009; Otterå et al., 2010; Swingedouw et al., 2011)"

- Page 2, line 32. As you focus on the AO in this paper, would be clearer not to talk about the NAO, but the AO, after Page 2, line 7.

We agree and focus on the AO in the revised manuscript. Note, that the results are very similar, when the analysis is performed using the NAO index.

- Page 3, line 19. '... uses temperature data ...'

Done.

- Page 4. line 3. You provide very little details on the model configuration. For example, what was the time step you used? How about the sea-ice salinity, was it constant? Or what sea-ice thermodynamics scheme was deployed? This information is important to assess how realistically the sea-ice salt rejection was modelled.

The model used is a configuration of the widely used ECHAM5-MPIOM (COSMOS model), which has been applied in various modelling studies and the IPCC AR4. The only difference between our version and the COSMOS version is the coupled atmospheric-chemistry module and this configuration is described in great detail in Muthers et al. (2014b).

The requested information has been included in the model description of the revised manuscript:

- Sea ice thermodynamics: “Sea ice dynamics are based on the viscous-plastic rheology formulated by Hibler (1979).”
- “A constant sea ice salinity of 5 psu is assumed.”
- Time-step: ““The time-step of the atmospheric component is 15 minutes, with the radiation and the chemical computations performed every 2 hours.” Ocean: “The time-step of the oceanic component is 2 hours and 24 minutes.”

- Page 4, line 20. You should mention here how long model simulations continued after the 30 year SSR period.

Rewritten to: “The reduction of the solar forcing is switched on in year 5 of a simulation and lasts for 30 years when it is switched off and the simulation is continued for 25 years.”

- Page 4, line 27. Explain the acronym TSI.

The acronym TSI is now defined at its first occurrence.

- Page 4, line 32. Explain more in detail how the AO index was calculated and provide references. For example, a common way to calculate the AO is based on the PC1 of 1000 mb pressure height anomaly data north of 20N. Your method seems to differ from that. Why? How robust your results are based on the AO calculation method?

We compared both AO methodologies, the EOF based way and the simplified AO index using the sea level pressure (SLP) north of 70°N. Both indices are closely related, for the CHEM_CTRL we find a Pearson correlation coefficient of 0.81 (0.85 for NOCHEM_CTRL). When calculating the AO based on sea level pressure data a common approach is based on the difference in the zonal mean SLP between around 40°N and 65°N (e.g., Li and Wang (2003), Braesicke and Pyle (2004)). We also compared our index (SLP field north of 70°N) to the index using the SLP difference between 40°N and 65°N and found very similar results. We therefore conclude that the exact definition of the AO index is not important for the results of this study.

However, for a better agreement with previous studies we will think about applying an AO index based on the SLP difference between 40°N and 65°N in the revised manuscript. In comparison to the EOF based definition we prefer this approach for its simplicity. In this case we will update the result and figures in the revised manuscript.

- Page 5, line 18. ’... are related ...’

Done.

- Page 5, lines 21-22. This sentence is hard to understand. How is the slight initial reduction of the global mean temperature related to the initial conditions of the ocean when the ocean initial conditions are from a 1300 year long simulation? Why rather not related to the atmospheric initial conditions which presumably started from an observation based, physically less consistent initial state?

The oceanic restart file is identical in all ensemble simulations. The atmosphere is perturbed by slight time difference between the restart files. Therefore, there is a considerable amount of “memory” in the ocean, which dominates the behaviour of the AMOC during the first years. We rewrote the relevant sentences for the revised manuscript:

"A slight initial reduction of the global mean temperature is also found in the reference ensemble experiments and is related to the initial conditions of the ocean. With all ensemble simulations sharing the same oceanic conditions in the beginning, the AMOC evolution of the first years is dominated by the oceanic memory."

- Page 5, line 31. 'during the second half'

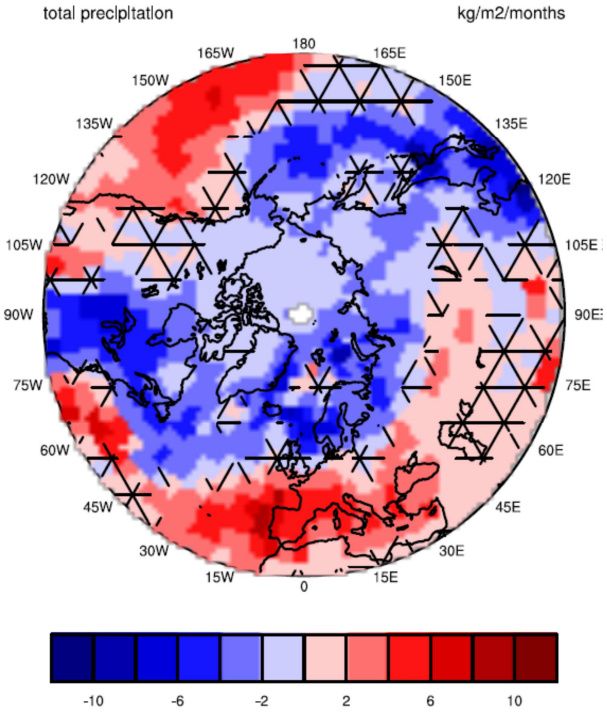
Thank you.

- Page 6, line 7. Do you mean that sea-ice patterns look similar but their anomalies are (presumably) weaker in the S1 experiments?

Exactly. Rewritten to: "In the S1 experiments similar but weaker temperature and sea ice anomalies are found and S1_CHEM experiment is characterized by an amplified temperature reduction as well (not shown)."

- Page 6, line 13. Is the reduction in precipitation related to a shift in the main storm track and, as a result, a colder and dryer atmosphere?

It is mainly due to the shift in the storm track. The negative anomalies in the North Atlantic and Northern Europe occur along with positive anomalies in Southern Europe (compare Fig. R1).



R 1: Precipitation anomalies in S2_CHEM relative to CTRL_CHEM averaged over the 30 year SRR period

We changed this sentence in the manuscript accordingly: "Additionally, a shift of the storm track and a significant reduction of precipitation is found in the North Atlantic, which further increases the salinity."

- Page 6, lines 15-16. I think you need to verify the significance of the salt rejection to the surface density. It is typically small compared to the cooling effect. Also, not much freezing occurs at the

eastern side of Greenland, but the Arctic ice flows south and melts along the eastern boundary of the East Greenland Current.

See above. We will address the issue of salt rejection and transport in detail when revising the manuscript. However, based on Figure S2 in the submitted manuscript, we are confident, that salinity (either through salt rejection, transport or reduced precipitation also play a role in the density changed.)

- Page 6, lines 26-27. You should mention that these density and mixed layer depth anomalies are not reflected in the AMOC.

We have added: "These changes during the first 15 years are not reflected in the AMOC index."

- Page 6, line 30. '... the North Atlantic (Fig. 3a).'

Done.

- Page 6, lines 33-34. You must mean 'the central North Atlantic' here.

Thank you.

- Page 7, line 2. The 'dominance' is based on very speculative assumptions. Just say 'Salinity changes, nevertheless ...'

Changed accordingly.

- Page 8, line 1. Add a literature reference that proofs the linkage between the downward propagating wind anomalies and the AO phases.

At this point in the manuscript we discuss Fig. S4, which shows a connection between the AO and zonal mean wind anomalies. The pattern of the zonal wind composite suggests downward propagation of the wind anomalies. A similar pattern has been found by Baldwin and Thompson (2009, compare their Fig. 11) and we have added this reference to the manuscript.

- Page 8, lines 13-14. You don't show this in Fig. 6, which should be mentioned, or plot CTRL_NOCHEM in Fig. 6.

We show boxplots of the AO index for all experiments in Fig. 6 c and d these show the behaviour we discuss in the manuscript. In the revised manuscript we have added an explicit reference to Fig. 6 d to the corresponding explanation.

- Page 8, line 16. '... which affects the wind ...'

Done.

- Page 8, line 29. '... the AO phase has a long lasting effect ...'

Done.

- Page 8, line 33. This should be '... the weakening of the Northern polar vortex ..', right?

Right, thank you.

- Page 8, lines 34-35. I suggest you to write '... dynamical changes decrease the density of the surface ocean waters South of Greenland, ...'

Applied as suggested.

- Page 9, lines 11-13. Don't these citations analyse the impact of the increase in GHGCs? Seems like you are cutting corners here. Wouldn't it be more correct to say e.g. '... Swingedouw et al., 2011). Related to increasing global greenhouse gas concentrations and associated surface warming, it is also one of the dominant ...'

We agree that our explanation was a bit oversimplified. We changed this as suggested to: "This response of the overturning to solar radiation changes has been identified in earlier studies (Cubasch et al., 1997; Latif et al., 2009; Otterå et al., 2010; Swingedouw et al., 2011). Related to increasing global greenhouse gas concentrations and associated surface warming, it is also one of the dominant mechanisms for the projected future weakening of the AMOC (Stocker and Schmittner, 1997; Manabe and Stouffer, 1999; Mikolajewicz and Voss, 2000; Gregory et al., 2005; Stocker et al., 2013)."

- Page 9, line 17. '... may reduce the projected 21st century ...'

Done.

- Page 9, line 18. '... stronger than in the late 21st century than [today?], when a grand ...'

We rewrote this to: "This is confirmed by simulations of Anet et al. (2013b). The AMOC is significantly stronger in the late 21th century, in an experiment including a grand solar minimum in the second half of the 21th century in comparison to simulations without a decline of the solar activity (Fig. S7)."

- Page 9, line 23. '... the AMOC by anomalous …

Done.

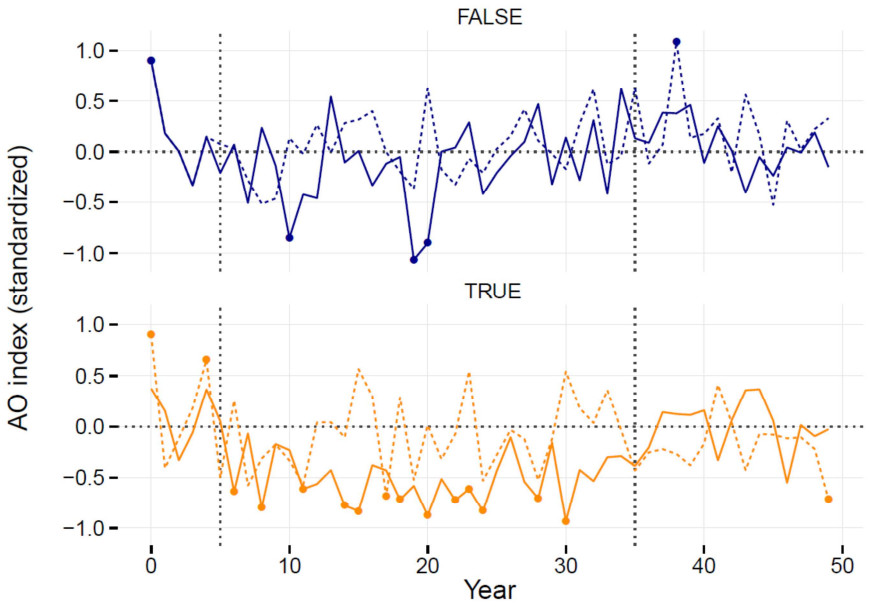
- Page 9, lines 23-25. This sentence is not clear to me. I suggest rewriting 'The dynamical effect is enabled by chemistry climate interactions, which result in amplified stratospheric temperature responses.'

We rewrote this to: "The dynamical effect is amplified by chemistry climate interactions, which result in amplified stratospheric temperature responses." We prefer the term amplified, since the same dynamical effects is also present without chemistry-climate interactions, but much weaker.

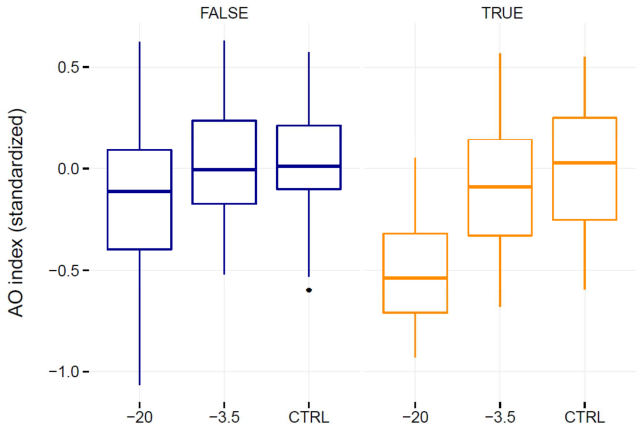
- Page 9, lines 28-29. The literature you cite here include three studies analysing NAO and only one analysing AO. This indicates to me, that NAO would have been a more appropriate index for this study as well, although its relation to the polar vortex is not as clear as the one of the AO.

For the analysis of the stratosphere troposphere interactions the AO index is the more appropriate parameter. The AMOC index, however, is stronger influenced by the NAO. In our study, the influence of the stratosphere on the AMOC is analysed and therefore, we have to decide for one of the two indices to draw a consistent picture, from the stratosphere down to the ocean.

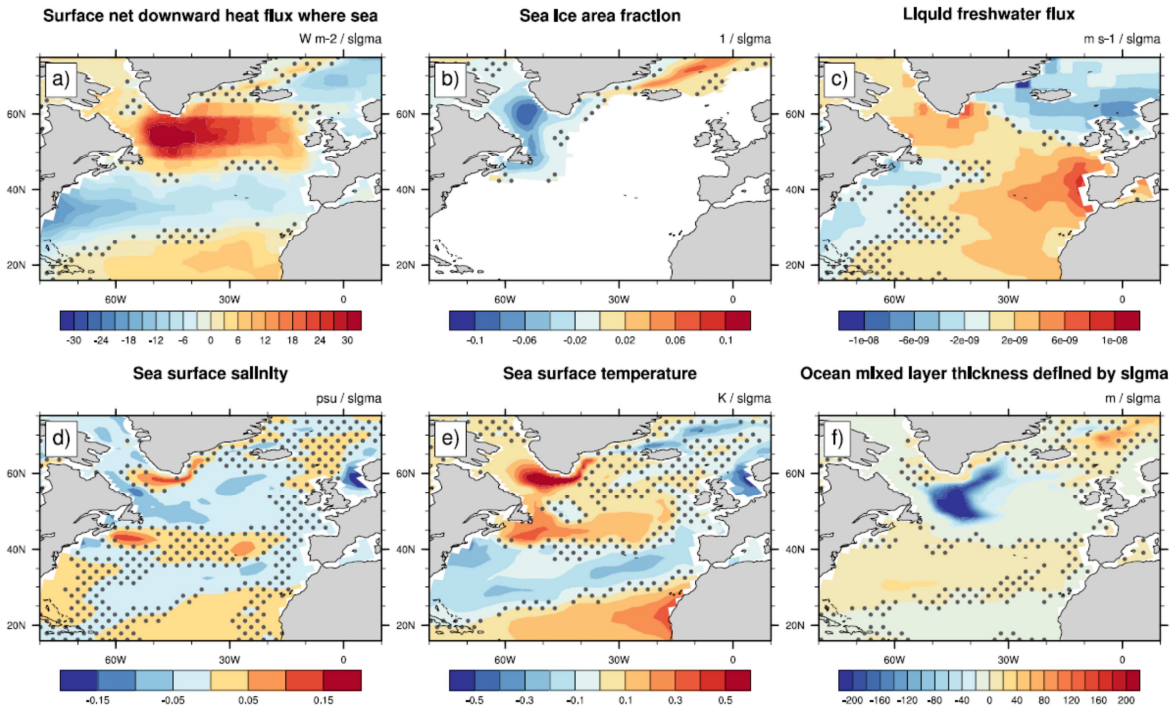
We argue, however, that the decision for one index or the other does not affect the conclusions of our study. Both indices are closely related in SOCOL-MPIOM. For CHEM_CTRL we find correlation coefficient of about 0.71 (0.68 in NOCHEM_CTRL.) For comparison we also performed our analysis with the NAO index (defined by the pressure difference between Iceland and the Azores). The results are given below and do not differ substantially between the results for the AO index.



R 2: Similar to Fig 6 a/b of the submitted manuscript, but for the ensemble mean winter (Nov. – Mar.) NAO index. The NAO is defined by the sea level pressure difference between Iceland and the Azores. Blue lines correspond to the NOCHEM experiments, orange lines to the CHEM experiments. Solid lines resemble the S2 and dashed lines the S1 experiments. Dots indicate winters with significant differences to the CTRL ensemble (Student t-test $p \leq 0.05$).



R 3: Similar to Fig 6 d/e of the submitted manuscript, but for the ensemble mean winter (Nov. – Mar.) NAO index. Blue boxplots correspond to the NOCHEM experiments, orange boxplots to the CHEM experiments.



R 4: Similar to Fig 7 of the submitted manuscript, but for the ensemble mean winter (Nov. – Mar.) NAO index.

- Page 9, line 31. 'the modelled response of the AMOC ...'

done.

- Page 10, line 5. '... weakening of the AMOC with climatic ...'

done.

- Page 10, line 10. What do you mean by 'Future studies'. Be more explicit. Are you planning to do this work?

We are currently not planning to study these questions, since generating an appropriate aerosol forcing for SOCOL-MPIOM is a complicated tasks which requires simulations with an external aerosol microphysical model Therefore, this is sentence is meant as a general suggestion to the community and we would like to keep this sentence as is.

- Figure 1. Write out the TSI acronym in the figure caption. As you used t-test for significance, did you check the autocorrelation or did you just treat each year as an independent variable? If years correlate, it affects your significance estimates. Explain more in detail what you did.

The TSI acronym is defined in the caption of the revised manuscript.

Dots in Fig. 1 represent year, where the SSR ensemble (e.g., S2_CHEM, 10 simulations) differs significantly from the control ensemble (e.g., CTRL_CHEM, 10 simulations). We therefore do a comparison of two data sets with 10 values each against each other. There is no autocorrelation, since we do not include any temporal information and the 10 experiments can be considered to be independent.

- Figures 2-4, 7, S1, S3, S5. Dots are not dark grey, but black. Better to say 'Black dots denote non-significant ...'

We replaced the figures with new versions using a lighter grey colour.

- Figure 2. More correct to say 'The sea-level pressure contour interval is ...'

Done.

- Figure S2. Indicate latitude and longitude locations of these T & S profiles.

We did not use a latitude longitude box to calculate the T & S profiles. Instead the grid cells were selected by their averaged mixed layer depth. We state this in the caption of Fig. S2:

"The deep water formation regions cover all grid cells with an annual mean mixed layer depth ≥ 250 m in the corresponding ocean basins".

References:

- Anet, J. G., Muthers, S., Rozanov, E., Raible, C. C., Peter, T., Stenke, A., Shapiro, A. I., Beer, J., Steinhilber, F., Brönnimann, S., Arfeuille, F., Brugnara, Y., and Schmutz, W.: Forcing of stratospheric chemistry and dynamics during the Dalton Minimum, Atmos. Chem. Phys., 13, 10951-10967, doi:10.5194/acp-13-10951-2013, 2013.
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