Anonymous Referee #4

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

———————- General comments ————————

This paper is presenting different sets of simulations that evaluate the impact of a decrease in Total Solar Irradiance (TSI) over three decades, with a specific attention to the AMOC. It is focusing on the impact chemical changes induced by such a decrease, through comparison of a model not including this process, and another one including it. In both models, the decrease of TSI leads to an AMOC strengthening in the decades following the onset of the decreased TSI. The authors argue that this strengthening is larger when the chemical processes are not accounted for. They attribute such an effect to the impact of stratospheric chemistry has on the AO response to TSI decrease. Indeed, TSI decrease may lead a negative NAO due to larger cooling in the stratosphere associated with ozone depletion, which when reaching the surface may affect air-sea fluxes and wind stress, decreasing in particular salinity, which may diminish salinity in the ocean convection sites, limiting AMOC enhancement.

As the former summary shows it, the amount of results shown in this paper is very significant. The topic is also of large interest, since the climatic impact associated with AMOC is well known as well as its good predictability a few decades ahead, and the TSI is also potentially largely predictable and is believed to decrease substantially in the coming decades. The impact of chemistry in the stratosphere was believed to potentially impact the AMOC response to TSI (e.g. Ottera et al. 2011), and this is the first study I see that tackle this potentially important process.

The paper is generally correctly presented, even though I have a large number of comments to clarify and better present the results. My main concerns are that:

1. the main effect analysed (i.e. the impact of chemistry on AMOC response to TSI decrease) is very small and maybe hardly significant;

We do not agree with this comment of reviewer 4. In Figure 1 c the differences in the AMOC behaviour between S2_CHEM and S2_NOCHEM is very clear and highly significant. Furthermore, significant (but weaker) AMOC differences are found between S1_CHEM and S1_NOCHEM, which confirms the results found in the S2 experiments.

2. the demonstrations are sometimes too rapid;

We clarified several steps of our analysis in the revised manuscript.

3. the amount of nice results is maybe too large, which may request to separate the analysis into two papers, i.e. two parts of the main analysis. The first dedicated to a better understanding of AO/NAO response, which is already largely depicted in the present paper, and constitute a very important results, even if not new. The second one will be dedicated to the analysed of the AMOC, which deserves a few more analysis, especially since it is the main topic of the present paper, but only have a few figures that are directly analysing the process involved in the presented changes.

We do not think that the results should be separated into two papers. The response of the AO/NAO to the stratospheric changes has been reported in numerous previous studies. The response of the AO to either solar forcing or the AO/NAO has also been reported previously. The two topics would therefore confirm previous results but would not present novel results.

The novelty of our study is that we can show that all these (previously reported) processes modulate the response of the AMOC to a reduction in the solar forcing and that, furthermore, interactive chemistry has a strong effect for the response. We are convinced this should be presented in a single paper.

Concerning the impact of the AMOC, I'm not entirely sure that the effect of chemistry leads to significant results. The ensemble mean of the simulation seems a bit different, but no error bar, nor statistical test are applied to confirm the supposed impact. Generally speaking, the differences between the two sets should be more systematically highlighted as in Fig. 4 (right panels), which is not the case everywhere, as well as the error bar associated with ensemble spread. Since this is the main result highlighted in the paper, this should be proven with more statistical confidence, or the main message of the paper should be modified.

We have added several figures showing the differences and significance tests for the differences to the revised manuscript.

For all these reasons, although I found the set of experiments very interesting and potentially improving our understanding of climate dynamics in response to solar forcing, I found the take-home message and general descriptions of the results and logical connections sometimes a bit rapid. I therefore consider that the manuscript need major revisions before to be published, and I will advise the authors to consider the possibility of splitting their results into two parts (and two papers) in order to describe properly the main results and mechanisms discussed.

We rewrote parts of the manuscript to improve the presentation of our results. However, we do think that splitting the paper into two parts is appropriate (see above).

———————- Specific comments: ———————-

- P. 1, l. 20: "is responsible for the temperature conditions in western Europe": there is a lot of debate on this specific topic: cf. Seager et al. (2002). The AMOC does not have only an impact on western Europe and cannot explain the whole climate of this region

We fully agree. We changed this sentence to:

"The surface branch of the AMOC transports heat from the Southern Hemisphere and the tropics towards the North, is closely connected to the Atlantic Multidecadal Oscillation, and contributes to the temperate climatic conditions in western Europe (Knight et al 2006)."

- P. 1, l. 22: "Meehl et al. 2009b": 2009a should come first.

This is corrected in the revised manuscript.

- P. 1, l. 23: add "in the past" after climatic changes"

done.

- P. 2: l. 13: “eruptions have been found to intensify the AMOC on decadal time scales”: this is not just a question of intensification, but rather of variability excitation cf. Swingedouw et al. (2015)

We modified the manuscript accordingly:

“Moreover, volcanic eruptions may excite the variability of the AMOC (Swingedouw et al., 2015).”

- P. 4, l. 30: “monthly mean”: This is a surprising choice. By doing so you include large part of so called Ekman wind-driven variability. Have you tried to remove this component, or to consider annual mean to limit its influence.

We have corrected this in the revised manuscript. We use the annual mean of the streamfunction to calculate the AMOC index.

- P.5, l. 3: what are the spread or error bar associated to the value given (since we are here considered ensemble of simulations.

The temperature difference between S2_CHEM and S2_NOCHEM is not a major point of our study. Therefore we did not present a detailed assessment of the differences between the two experiments. We have added two ensemble spread in the Table below.

Experiment	ΔT (K)	standard deviation (K)
S2_CHEM	-1.0	0.04
S2_NOCHEM	-0.9	0.07
S1_CHEM	-0.1	0.10
S1_NOCHEM	-0.1	0.07

The Student’s t test suggest that the differences between the S2 experiments are highly significant ($p=0.0009984929$), while the differences between CHEM and NOCHEM are not significant in the case of the S1 forcing.

- P. 5, l. 8: can you be more specific on the reference that gave the climate sensitivity of the model and the computation you have made. When you gave numbers, you have to be more specific on the way you compute them.

The climate sensitivity of SOCOL-MPIOM and the experiment are described in Muthers et al. (2014). A reference is given in the model description but we will add the reference to this point as well.

- P. 5, l. 19-20: why is outgoing longwave increasing when water vapour increases. Please clarify the process at play here.

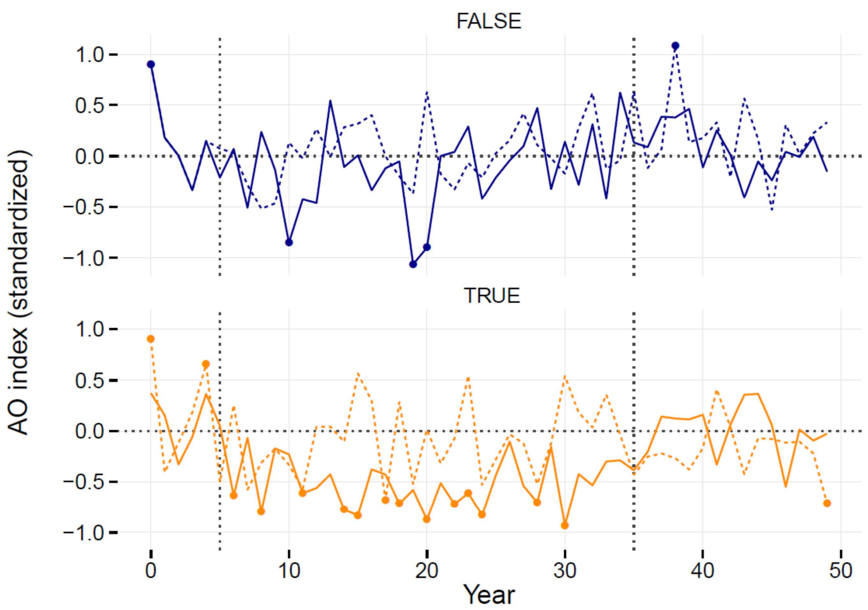
Stratospheric water vapour decreases in the SSR experiments, therefore OLR increases. The ordering of the sentences in the submitted manuscript was a bit misleading. We changed this to:

“In S2_NOCHEM, the largest anomalies (-15%) are found in the tropical upper troposphere, but stratospheric reductions exceed -10% almost everywhere (Fig. S1c). In S2_CHEM, the stratospheric reductions in water vapour are more pronounced (up to -35%), due to the effect of the solar forcing on the oxidation of methane, the most important in-situ source of stratospheric water vapour (Fig. S1b). Due to the greenhouse effect of ozone and water vapour, the outgoing long-wave flux increases more in CHEM than in the NOCHEM and leads to an additional cooling of the troposphere. The positive water vapour anomalies found in the uppermost model levels in the CHEM experiments (Fig. S1b and e) are related to the reduced UV photolysis of the water vapour molecules."

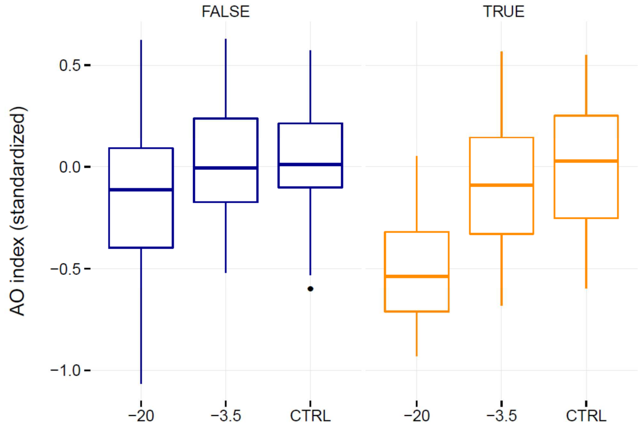
- P. 6, l. 1: why don't you look at the NAO rather than the AO, since you are looking at the North Atlantic region. The two are usually very much linked, but can you confirm this in your model?

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.

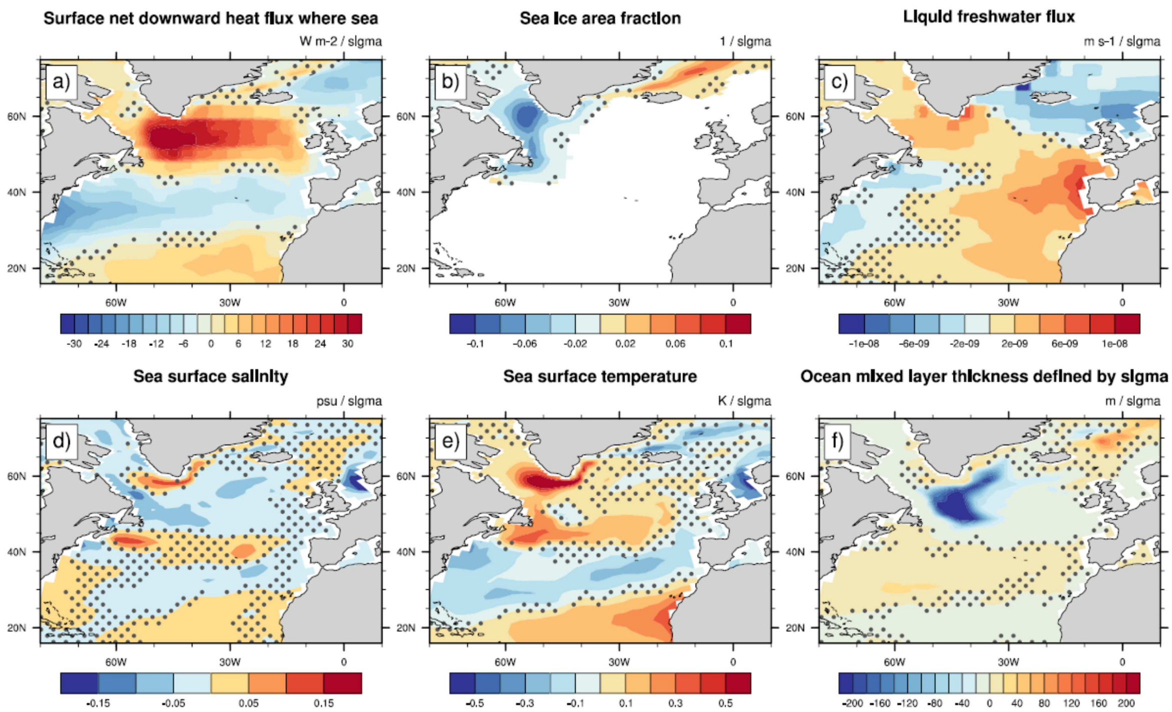
We argue, however, that the decision for one index or the other does not affect the conclusions of our study. Both indices are closely related in SOCOL-MPIOM. For CHEM_CTRL we find correlation coefficient of about 0.71 (0.68 in NOCHEM_CTRL.) For comparison we also performed our analysis with the NAO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



R 7: 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 8: 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 9: Similar to Fig 7 of the submitted manuscript, but for the ensemble mean winter (Nov. – Mar.) NAO index.

- P. 6, l. 9: "the sea ice differences": when? Try to be very precise on what you are talking about

We are referring here to the sea ice differences shown in Fig 2. which are averaged over the 30yr solar minimum period. We clarified this in the manuscript:

"The Arctic sea ice differences between CHEM and NOCHEM, which emerge in the last 20 years of the reduction period, and are related to the weaker AMOC in the CHEM experiments and the reduced heat transport into the Arctic."

- P. 6, l. 9: “therefore”: the logical connection is not very clear to me, please clarify it.

See our answer to the previous comment (therefore has been removed).

- P. 6, l. 17: “Nordic Sea”. I am usually seeing “Nordic Seas”, since it is a few seas that you are dealing with (Greenland, Iceland, Norway). The same elsewhere in the ms.

This has been corrected.

- P. 7, l. 8-9: I’m not convinced the anomalies between CHEM and NOCHEM are significant for the AMOC. Please provide appropriate statistics.

We are refereeing here to the differences in the AMOC index between CHEM and NOCHEM. A reference to Figure 1 has been added to the revised manuscript.

The differences between the AMOC in S2_CHEM and S2_NOCHEM are significant (Students t-test, $p < 0.05$) for the years 27, 28, 30, and 32, which corresponds to the end of the solar reduction period. In the revised manuscript we highlight years with significant differences between the CHEM and NOCHEM experiments in Fig. 1.

The significant differences between S2_CHEM and S2_NOCHEM are furthermore obvious from the latitude-depth cross section of the meridional streamfunction in the Atlantic. This figure is included as Fig. S2 in the revised supporting material. In the second half of the solar reduction period the differences are significant in a large region between about 800m to 3000m depth from the Southern hemisphere to 50°N. Between 30°N and 45°N the significant differences reach all the way up to the surface.

- P. 7, l. 11: can you provide a reference or an explanation to support this claim?

We have clarified the statement and we have added a reference:

“Chemistry-climate interactions are the most pronounced in the stratosphere (e.g., Dietmüller et al., 2014).”

- P. 7, l. 13: “28K” is this concerning only a grid points?

The term “up to 28 K” described the maximum temperature difference, which is found in one or a few grid points-

- P. 7, l. 20: “-43%”: when? Over the 30-year period?

Yes, -43% when averaged over the 30 year period. We clarified this in the manuscript:

“a reduction of -43% is found in S2_CHEM during the winter season (Nov. to Mar.) when averaged over the SRR period.”

- P. 7, l. 23: what is your definition for the “duration of the winter period”?

The winter period starts with the first day with a westerly daily mean zonal mean wind component at 60N and 10hPa after 1. October and ends with the first day with easterly wind after 1. April.

We clarified this in the manuscript:

“Furthermore, the duration of the winter period with predominant westerly wind is reduced in S2_CHEM by –30% and in S2_NOCHEM by –5% respectively, when defining the start of the winter period by the day with the first occurrence of a westerly daily mean zonal mean wind component at 60N and 10hPa after September and the end by the first day with easterly winds after March."

- P. 7, l. 24-25: a series of number are given, with very poor definition. Please clarify.

See above.

- P. 8, l. 3: "downward coupling": can you define this?

We changed this to "downward propagation".

- P. 8, l. 21: "freshwater flux": from which component? Precipitation? Evaporation? Sea ice?

We are referring to the total freshwater flux from all three processes. We clarified this in the revised manuscript.

- P. 8, l. 23: "export of saline water from the Nordic Sea by EGC". The EGC is a very fresh and cold current, so it is not exporting saline water! Do you mean the weakening of this current is increasing the salinity?

Thank you. We will give a detailed answer to this in the revised manuscript.

Please clarify.

- P. 8, l. 28: "instantaneously": thus, this is likely not related to convection but rather to wind-driven changes. Can you comment on that?

We will rephrase this paragraph. The word "instantaneously" may not be appropriate in this case.

- P. 8, l. 31: "weaker intensification": significant? At which level? (please account for autocorrelation when computing degrees of freedom, since the AMOC has very low variability.

At this point we summarize the previous results. The weaker intensification is shown in Fig. 1c. 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.

- P. 9, l. 11: "is also one of the": not really, since in projections, this is the longwave radiation that is mainly affected rather than the solar radiation changes.

Our description was a bit misleading. We clarified this in the revised manuscript:

"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)."

- P. 9, l. 20-24: while the impact on the AO is very large, the impact on the AMOC is very weak, why is that? Is it coherent with small effect of AO on AMOC in control? What is the regression value of the AO on the AMOC in this model? Lohman et al. (2009) can be an interesting references concerning long term of a positive NAO on North Atlantic.

The response of the AMOC is a combination of several factors. The AMOC responds to the temperature changes, which cause an intensification of the overturning. In the CHEM experiments the AMOC is furthermore affected by the AO, with the negative AO phase leading to a weakening. Therefore, the response of the AMOC to the AO is already counteracted by the effect of the temperature changes.

We discuss the study of Lohmann et al. in the revised manuscript:

"The influence of the dynamic effect on the AMOC may furthermore depend on the length of the solar reduction period. Lohmann et al. (2009) found a gradual weakening of the subpolar gyre response with time in ocean model simulations forced with a persistent negative phase of the NAO. Additionally, the response of the AMOC may be non-linear and an increase of the solar forcing may change the dynamic effect (Lohmann et al., 2009)."

- P. 9, l. 32: "importance": I think this is a strong statement for a very weak effect in the end. . .

We do not agree. Between S2_CHEM and S2_NOCHEM we find an AMOC difference of about 1 Sv, which can only be attributed to the interactive chemistry. This difference cannot be considered as weak. Moreover, the weaker forced S1 experiments also reveal a clear difference in the AMOC intensities between S1_CHEM and S2_NOCHEM.

- P. 10, l. 1: add "slightly" after "may"

see above, we do not think that the response is weak, therefore a "slightly" would reduce the importance of our study.

- Fig. 1: please compute a statistical test for differences between CHEM and NOCHEM anomalies.

See above.

- Figs 2,3, 7: please compute the difference CHEM-NOCHEM as in Fig. 4

For the revised manuscript we will produce a plot of the differences between CHEM and NOCHEM for these figures. If additional informations can be drawn from these figures we will include them in the revised manuscript, otherwise we will show the in our final answers to the reviewer.

- Fig. 7: This is a key figure when trying to understand what is going on for the AMOC, which should be the heart of the paper, given the title. Why is the projection so different than in 3? We want to see what is going in the whole Nordic Seas, including Fram Strait. What about circulation changes (barotropic stream function for instance)? Wind stress? Density? Thermal and salinity component of density? The demonstration of the processes affecting the North Atlantic should be more depicted.

We will address these questions when revising the manscript.

Figure S2 in this regard is interesting and should come in the main ms., but what is missing on this figure is an indication of the time frame. When are the changes occurring. Each point corresponds to a year from what I understand (with a smoothing of 15 years). Thus the anomalies are firstly thermally driven and then salinity driven. Why is there such a 10-year lag? (which is not clear from Fig. 8 where no time scale is shown).

Thank you, we will think about including this figures to the main manuscript.

On the time-lag question: Delworth and Zeng (2016) performed sensitivity experiments where the forced an ocean model by artificial atmospheric forcing. In one of their experiments they instantaneously switched the atmospheric forcing to an NAO positive state. Their results show that after about 5-7 years the AMOC responds to this forcing with strengthening of the circulation (compare Fig. 3 in Delworth and Zeng). This shift of a few years agrees with our results, although an exact timing is difficult to estimate from our results. In our Fig. 6 we see that it takes a few years before the AO shift towards a predominant negative phase in S2_CHEM (about year 10 of the simulations). Differences in the AMOC, however, emerge around the year 20 (Fig. 1c), so about 10 years after the AO shift.

We will discuss our finding in the light of the study by : Delworth and Zeng (2016) I the revised manuscript.

—————- Bibliography: —————-

- Lohmann K, H Drange, M Bentsen (2009) Response of the North Atlantic subpolar gyre to persistent North Atlantic oscillation like forcing. Climate dynamics 32 (2) pp 273-285

- Seager R, DS Battisti, J Yin, N Gordon, N Naik, AC Clement and MA Cane (2002) Is the Gulf Stream responsible for Europe’s mild winters? QJRMS 128 (5), pp. 2563-2586

- Swingedouw D, P Ortega, J Mignot, E Guilyardi, V Masson-Delmotte, PG Butler and M Khodri (2015) Bidecadal North Atlantic ocean circulation variability controlled by timing of volcanic eruptions.Nature Communications 6, pages: 6545