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Earth System Dynamics Discussions

Interactive comment on "Response of the AMOC to reduced solar radiation – the modulating role of atmospheric-chemistry" *by* S. Muthers et al.

Anonymous Referee #4

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——————- 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;

2. the demonstrations are sometimes too rapid;

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.

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.

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.

——————- 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

- P. 1, l. 22: “Meehl et al. 2009b”: 2009a shoud come first.

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

- 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)

- 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.

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

- 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.

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

- 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?

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

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

- 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.

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

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

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

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

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

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

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

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

- 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? 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?

- 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.

- 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.

- 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.

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

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

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

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

- 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. 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).

—————- Bibliography: —————-

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- 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

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