Review of „Response of the AMOC to reduced solar radiation – the modulating role of atmospheric-chemistry" by S. Muthers et al.

This paper examines the physical processes responsible for the AMOC response to reduced solar radiation and assesses the importance of chemistry-climate in modulating this response. By comparing two sets of climate model experiments, with and without interactive chemistry, the study demonstrates that climate models which do not consider stratospheric - namely ozone - chemistry may overestimate the sensitivity of the AMOC to solar forcing since the "top-down" influence (stratospheric influence on tropospheric circulation) is underestimated.

In my opinion, this work constitutes a very nice contribution for the broad climate research readership as it demonstrates, using the specific example of the AMOC, the prominent and complex connections between the different components which drive climate variability (going from the stratosphere chemistry to the ocean circulation). The work is well-framed in the current literature. I found the paper mostly clear, well written and scientifically sound. I think however that some improvements and clarifications could be made before publication. Please find my main comments/suggestions below:

Main comments/questions:

1/- It has already long been recognized that atmospheric chemistry interacts with dynamics and that its consideration in climate models is crucial to adequately simulate climate variability (e.g. influence of the ozone hole recovery on the SAM trends in CMIP3 simulations by Son et al. (2008)). As a consequence, historical and projection climate simulations in CMIP5 for models without interactive chemistry were designed by prescribing chemical fields that consider long-term trends (Cionni et al., 2011). For CMIP6, the ozone prescription fields should be even further improved. So I would say that the current question regarding chemistry-climate interactions is: do we really need interactive chemistry? or can it just be prescribed? The other question is then how to prescribe it in the most accurate way (see e.g. Nowack et al. (2015)).

In my opinion, given the frame, the results and the conclusion of the present study, I think that the introductory part of the paper should – at least partly – review the recent advances regarding chemistry climate interaction. A lot has been done already and should not be ignored.

2/- In the light of my previous comment, I would suggest the authors to explain more thoroughly how the combined UV+ozone effects modulate the heating rates in the stratosphere which is the starting point of the stratospheric mechanism discussed in the paper. The thermal modulation of the stratosphere through UV variations comes from two main effects: (1) direct shortwave heating through incoming UV absorption by ozone (λ~200-300 nm), (2) ozone change (λ<242 nm) which also affect shortwave heating rates. Both effects count significantly. Basically, and if I understood correctly, their NOCHEM experiment account for effect (1) only while CHEM account for effects (1) + (2). I think such clarifications are easy to make and necessary since they help understanding the basic difference between the two experimental configurations (at least regarding stratospheric ozone which is the major solar effect). In the present version of the paper too few information are given on UV-ozone-temperature interactions and their implication on experimental setting (e.g. P2L32-P3L1, P5L12-14).

3/- A very recent study by Chiodo and Polvani (2016) has just been released in *Journal of Climate* and deal with – somewhat - similar problematics. They performed simulations that also present some similarities with those perform in the present work. While both studies have their own relevance and focus on different aspects, they also nicely complement each other. The authors may consider comparing results of both studies: are they consistent?

4/- In light again of my first comment, there is currently a debate about the need of having interactive chemistry in climate model or if it is sufficient to prescribe chemistry. The concern is real given the heavy computational costs that interactive chemistry requires. This question could have been addressed here by using the chemistry outputs of the CHEM experiments as a chemistry forcing for a say "prescribed-CHEM" experiment with solar-induced ozone changes. Both effects (1)+(2) (see comment 2/) could thus have been considered without including interactive chemistry. Did the authors perform such experiments? If they have (and only if they have), it would be relevant to mention their conclusions in the paper.

Specific comments:

+ P1L6-10: "*In simulations with chemistry-climate interactions a second, dynamical effect on the AMOC is identified which counteracts the thermal effect. This dynamical mechanism is driven by the stratospheric cooling in response to the reduced solar forcing, which is strongest in the tropics and leads to a weakening of the Northern polar vortex. In simulations with interactive chemistry, these stratospheric changes are strongly amplified by the reduction of stratospheric ozone.*" The point made in these three sentences seems confusing. The first two sentences seem to suggest that the stratospheric cooling is found only in the chemistry-climate simulations while it is in fact found in both but amplified when ozone reduction feedback is included (as suggested by the third sentence) in addition to the direct radiative heating reduction. This may benefit of being clarified.

+ P2L12-13: "*The variability of the overturning circulation is furthermore influenced by external forcings (Otterå et al., 2010). Volcanic eruptions have been found to intensify the AMOC on decadal time scales (Otterå et al., 2010; Mignot et al., 2011).*" Since the study particularly investigates the mechanisms, I would suggest here to specify through which mechanisms volcanic eruptions influence AMOC (i.e. direct radiative cooling effect + tendency to induce positive NAO).

+ P2L21: change "*trough*" to "through"

+ P2L34-P3L1: "*This response is modulated by chemistry-climate interactions. In particular, stratospheric ozone reacts to the UV changes and amplifies the stratospheric temperature change (Baldwin and Dunkerton, 2005)*". I think that further explanations on the UV-ozone-temperature interactions may be needed given that they are the source of the difference found between the CHEM and NOCHEM versions of a same experimental scenario. Furthermore, the reference to Baldwin and Dunkerton (2005) might not be the best suited for this purpose. The authors could rather refer to the work of J. Haigh in the 1990s (Haigh, 1994 ; 1996). The authors could also refer to section 3.5 of the CCMVal report (and reference therein) which can be found at the following address http://www.sparc-climate.org/publications/sparc-reports/sparc-report-no5/. This chapter particularly details the implication that prescribing constant ozone (as in the NOCHEM experiments of the present study) has on shortwave heating rates associated with changes in the UV.

+ Section "*2.1 The model*": What about energetic particle effect? SOCOL-MPIOM has parameterizations that allow taking into account GCR and EPP effects (which are linked to solar activity variations) and are suggested to also have an impact on the Northern Hemisphere surface climate (e.g. Rozanov et al. (2012)) through the "top-down" mechanism and thus may also affect the AMOC.

+ P5L12-20: Here the authors may consider discussing their results in comparison with Chiodo and Polvani (2016).

+ P6L4-5: The sea-ice extension and the associated differences between S2-CHEM and S2-NOCHEM experiments are hard to see on Fig 2 which is already quite busy.

+ P6L13-14: "*Additionally, a significant reduction of the precipitation is found in the North Atlantic, which further increases the salinity*." Please indicate that this is not shown (in brackets).

+ P6-7: "*3.1 The thermal effect of SRR on the AMOC*": This part contains very interesting material and is very informative. However, I found quite hard to follow the text and figures together. While this is largely due to the fact that I am not used to examine ocean processes, I believe that some improvements could still me made. In particular, one of the key points relies on the differences, between the CHEM and NOCHEM configurations, of the timing of the anomalies development leading to differences in the AMOC response. In this regard, I think that, in addition to spatial patterns (Fig. 3), showing time series (similar to Fig. 1) of the key variables in the key regions may help understanding the timing issue.

+ P7L19-20: "*For S2_CHEM, a pronounced weakening of both polar vortices is found*." Please give the reference to Figs. 4d,e,f in the text and replace "*both polar vortices*" by "NH and SH polar vortices" for clarity concerns.

+ P7L25: Is it annual anomalies or only winter (NDJFM) anomalies which are shown in Figs 4 and S3? Please clarify.

+ P7L26-27: Again for clarity, one sentence to explain what a SSW is may be useful here.

+ P8L3-4: "*Overall, the downward coupling of wind speed anomalies does not differ substantially between the CHEM and NOCHEM control experiments*." Although it is written that the statement concerns "*anomalies*", I believe that this sentence might be misleading since it seems to suggest that the CHEM and NOCHEM downward influence of the stratosphere on the troposphere are the same. We thus may wonder why we should expect a difference in the AO strength (described in paragraph which follows, P8L5-14). Please make this point clearer (as it is a key point of this paper).

References:

- Chiodo, G., and L. M. Polvani (2016): Reduction of climate sensitivity to solar forcing due to stratospheric ozone feedback, *J. Clim.*, Doi:10.1175/JCLI-D-15-0721.1.
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