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Geoscientific Model Development Discussions

Interactive comment on "The coupled atmosphere-chemistry-ocean model SOCOL-MPIOM" *by* S. Muthers et al.

Anonymous Referee #1

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This study presents results from the SOCOL-MPIOM coupled chemistry-atmosphere-ocean model. Rather than focusing on validating the overall performance of the meteorology and chemistry compared to observations, the study focuses on the impacts of interactive chemistry on the atmosphere by comparing experiments with and without the chemistry module implemented. They go on to present historical all forcing experiments and focus on the effects of solar variability on surface temperatures. The paper is generally well written and the examination of the effects of coupling the chemistry will be of interest to the wider community, particularly in the context of current Earth System Model development activities. I therefore deem the topic of sufficient interest for the GMD community.

However, I think a number of aspects of the paper could be improved before I would

recommend publication. In particular, I think the discussion could be more focused onto some of the fundamental aspects of the model behavior, such as the climate sensitivity, rather than on a diverse range of topics that don't necessarily fit together well. I therefore think removing some of the topics, such as the lengthy discussion of the role of solar variability during the Maunder and Dalton Minima, would shorten the paper and help to increase the overall impact. I have made some suggestions for ways to do this below.

Recommendation: Major revisions.

Major points

Section 4.1: This part comes across more as an evaluation of whether the Shapiro et al. (2011) solar forcing dataset is plausible rather than anything specific to do with the SOCOL-MPIOM model. The conclusions are mixed, with poor model-proxy data agreement during the MM, but better agreement during the DM. There is also a strong solar induced warming during the early 20th century, which contributes to an overestimation of the temperature trend compared to observations. These results raise doubts as to whether the Shapiro dataset is plausible for use in climate models. This brings me to another point, which is that the authors offer little justification for why they have used the Shapiro et al. (2011) dataset (L9 3025) rather than another more moderate construction (e.g. Wang et al. (2005)). The authors state that the Shapiro construction is outside of the uncertainty range given in the IPCC and their results appear to confirm that this does not produce results that can be squared with observations (L9-14 3052). The reasons for including this particular solar forcing dataset in the model therefore needs more justification.

More generally, I am not convinced that the detailed discussion around the role of solar forcing during the Dalton and Maunder Minima really fits into this study. There are other more relevant aspects of the model evaluation that could be expanded upon (see below) and the solar specific aspects might be better suited in a separate paper. I would therefore recommend taking out most of Section 4.1. This would also help to shorten the paper, which in its current state feels a bit too long.

Section 3.3: The model is shown to have a too high Equilibrium Climate Sensitivity and Transient Climate Sensitivity. However, little attempt is made to explore the reasons for this. The Gregory et al. (2004) method could be used to separate out longwave and shortwave clear and cloudy components (see e.g. Andrews et al. (GRL, 2012)) and this could help to elucidate where the model's feedbacks come from. Some further analysis of this would help to strengthen discussion on L15-26 P3051.

I also suggest doing an ECS experiment for the NOCHEM run. The effect of interactive chemistry found here is smaller than that of Dietmuller et al. (2014) and much smaller than the 20% effect found by Nowack et al. (A large ozone-circulation feedback and its implications for global warming assessments, Nature Climate Change, submitted). This is an emerging area, and if this effect is as large as other models suggest it has the potential to be important for the wider climate modeling community and therefore dependencies on model/experimental design need to be understood. In the discussion (L15 3051 – L6 3052), the authors suggest that the apparently weaker effect of chemistry in SOCOL-MPIOM may be due to the small decrease in ozone in the tropical lower stratosphere, but this would suggest a weak Brewer Dobson circulation response. I think this effect needs to be better diagnosed to establish why the results shown here differ from other recent studies on the role of interactive chemistry in climate sensitivity.

Section 3 P3023 L6-20: The explanation that there is a model surface temperature drift and how it is corrected seems rather disconnected and it is not until P3026 L7-9 that we learn the reason for this is related to the choice of solar forcing dataset, which includes very different irradiances in the visible part of the spectrum. I think the discussion on P3026 needs to be moved to the point at which the model drift is discussed to make this whole issue clearer. Furthermore, on L20 the fact that the adjusted TSI ends up being comparable to Kopp and Lean (2011) is probably more due to luck than judgment, so I think this statement about the comparison with observed TSI needs to be toned down

or removed.

Minor comments

L28 3015 'Very strong' – this is vague and since we don't really know how stratospheric wind anomalies impact on the troposphere I suggest removing this and just saying 'Wind anomalies. . ..'

L5 3016 'unusual' – I suggest changing this to 'anomalously high' and adding a reference to e.g. L. M. Polvani and D. W. Waugh: Upward wave activity flux as precursor to extreme stratospheric events and subsequent anomalous surface weather regimes, J. Climate, 17, 3548-3554 (2004)

L15-17 3016 This sentence is unclear and confusing.

L25 3016 Add a reference e.g. Kolstad, E. W., Breiteig, T. and Scaife, A. A. (2010), The association between stratospheric weak polar vortex events and cold air outbreaks in the Northern Hemisphere. Q.J.R. Meteorol. Soc., 136: 886–893. doi: 10.1002/qj.620

L18 3016 Replace 'both' with 'the tropospheric annular modes'

L21 3016 Add a reference e.g. Baldwin and Dunkerton, 2001.

L5 3017 I'm not sure Meehl et al. (2009) is the best reference for the top-down pathway. Rather e.g. Kuroda and Kodera (2002), Matthes et al., (2006).

L10 3017 'Differently' → 'In contrast'

L29 3017 insert 'temperature' before gradient

L8 3018 'proven' → 'shown'

L8 3018 'essential' – they are not always essential, it depends very much on what you are interested in. I suggest changing this to 'important tool'

L10 3018 between THE ocean and atmosphere

L27 3018 The effect of atmospheric chemistry → remove 'the'

L7 3020 'the QBO input data'

L13 3020 'forcing' → 'effect'

L19 3020 What PSC scheme is used? Please give more details.

L14-15 3021 This discussion of vertical interpolation of tracers comes from nowhere and it is unclear to the reader as to the potential importance of this – can you clarify?

L21-23 3021: The parameterization of absorption in the Lyman-alpha, Schumann–Runge, Hartley, and Higgins bands in the CHEM run alone seems rather arbitrary and unphysical. This is shown to have impacts on the stratospheric climatology, but since it's unphysical to neglect this effect in the first place these changes seem rather spurious.

L21 3026 'including a' → 'which includes a'

L24 3022 What do you mean by 'scratch'?

L26-29 3023 How are other chemical species (CH4, N2O etc.) represented in the NOCHEM run? Do they follow the same treatment as for ozone? Please clarify.

L17 3024 'radiative flux imbalance'

L18 3024 'global mean surface temperature change'

L21 3024 'without changing' → with fixed

L19-20 3024 Experiment M1 has not been introduced by this point in the manuscript, so it is not clear what you mean.

L13-14 3025 'by a larger amplitude' what? 11 year solar cycle?

L3-5 3026 The lower UV irradiance in the Shapiro dataset must mean that stratospheric temperatures are lower in this version of the model? This issue is not mentioned at all,

but if it is the case it seems that it would be important and should be discussed.

L12 3028 You should be consistent here about the use of M and L that you introduced earlier for the solar forcing sensitivity experiments.

L20 3028 'used as the forcing'

L9 3029 'simulated global surface temperature increase'

L3 3030 'a' → the

L3 3030 'is' → are

L3 3030 'development' → evolution

L17 3030 'at a depth of'

L22 3030 'However, the oceanic temperatures are still not'

L27 3030 'not yet reached'

L28 3030 delete 'so far'

L21 3031 I think it is important to stress here that because the QBO is nudged there is limited potential for the ozone response to feedback onto the circulation.

L7 3032 'in austral spring, during the break-up of the polar vortex.'

L14/L15 Do you mean statistically significant? If so, please state at what confidence level and how this is calculated.

L14 3032 'on' → in

L18 3032 'are the result of a number of different processes'

L24 3032 'in summer (not shown)'

L26 3032 negative signal → cooling effect

L5 3033 undergoes → exhibits. Also add reference.

L8 3033 'day reach' → 'day can reach'

L20 3033: The findings are not really contrary to the results of Maycock et al. (2011), you have just done a different experiment altogether. I suggest rephrasing to:

'Maycock et al. (2011) reported a maximum cooling in the lower stratosphere after a uniform increase of the stratospheric water vapour; however, the cooling effect in SOCOL-MPIOM is strongest in the upper stratosphere and mesosphere. This is probably because the water vapour difference between CHEM and NOCHEM is not uniformly distributed and the largest differences are found in the higher stratosphere.'

L2 3034 'the differences in the zonal mean zonal wind reflect'

L12 3035 reflected in the NAM → 'reflected as a negative NAM index.'

L26 3035 Give numbers for the total SSW frequency in SOCOL-MPIOM. The error bars on the seasonal distribution in reanalyses are large, so I suggest removing the part about the seasonality of SSWs in the model being too uniform unless a more robust statistical comparison is made between the model and reanalyses.

L2-4 3036 Is this difference in SSW frequency statistically significant? You can use the t-test in the Appendix of Charlton et al. (2007; A new look at stratospheric sudden warmings. Part II: Evaluation of numerical model simulations. J. Climate, 10, 470-488, doi:10.1175/JCLI3994.1) to test this.

L20-21 3038 'is obviously' → are

L21-22 3039 This is not the formal definition of climate sensitivity.

L23 3039 'transient climate simulations of past and future climates.'

L28 3039 With 2.2 K the TCR of → With a TCR of 2.2K,

L8-10 3040 'In comparison to the MPI-ESM based on ECHAM5–MPIOM, the TCR

is the same but the ECS is considerably higher.' – why does only the ECS change between the model versions, but not the TCR? It is not clear to me why the effect should be so sensitive to the particular idealized climate change experiment used. This needs more explanation.

L21 3040 I suggest adding some discussion here about the comparison with Dietmuller et al. (2014). This is currently in the discussion, but should be moved here.

L27 3044 sufficient → larger

L1 3050 do you mean higher stratopause?

L4-5 3051 This sentence has been erroneously pasted in: With a transient climate response (TCR) of 2.2 K and an equilibrium climate sensitivity (ECS) of 3.7 K. Please remove.

Table 1 caption: → In column chemistry the usage of the interactive chemistry module is indicated.

Table 2 caption: → 'winter (DJF) zonal mean zonal wind at 50 hPa'

Figure 11 caption: What method have you used to account for the autocorrelation?

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