

“The coupled atmosphere-chemistry-ocean model SOCOL-MPIOM”
by Muthers et al.

Review

The manuscript describes the newly developed SOCOL-MPIOM atmosphere-ocean GCM with coupled atmospheric chemistry and its behavior in a number of climate simulations. As the authors correctly point out, so far, for climate applications mostly models without coupling to a comprehensive atmospheric chemistry scheme have been used. On the other hand, so-called CCMs, i.e. Atmospheric general circulation and chemistry models are mostly used without a coupled full ocean, and hence not in climate mode. Therefore, this model and its performance are a highly interesting topic and very well suited for publication in GMD. It is very nice to see that the authors evaluate the model performance using numerous standard climate simulations. Nevertheless I have a couple of comments which I think will partly require rerunning the described experiments (unless it is just the description that is flawed) and I also think that the priorities in the model description and other parts of the text are not always well chosen so that I would consider the necessary revision as major. One suggestion would be to considerably shorten the rather long manuscript. I will make suggestions where one could cut the text, but I do not want to impose this. Finally, the authors have to decide what they think is relevant to communicate.

I will list my concerns according to their appearance in the text. In addition I would like to see a careful reconsideration of the use of language in the manuscript which is often rather approximate. I will only give examples for this. 13 co-authors should be able to deal with language issues themselves.

Abstract: The abstract seems somewhat too lengthy for my taste. However, this should be up to the editor.

Introduction: The introduction needs about 4 pages until the goal of the paper is mentioned. This should happen much earlier. The first 4 pages contain mostly something like a review on troposphere-stratosphere coupling. The reader is totally left alone with the question which part of the content is relevant for this paper. The introduction should only concentrate on those issues and make their relevance clear. By this I think the introduction could easily cut by half.

3015L9ff: “In recent years the stratosphere has become more and more important for our understanding and proper simulation ...” The importance of the stratosphere has likely not changed much, rather the recognition of its importance.

3015L14ff: “importance of the vertical resolution” Most of these papers, as far as I remember, do not discuss the relevance of the vertical resolution in the stratosphere but of resolving the stratosphere at all.

3015L20: Why “furthermore”? Isn’t the chemical composition part of the stratosphere?

3016L8: “underlying mechanisms are still debated” Mechanisms of what? Of wave propagation?

The above four points exemplify the issues I have with the use of language. However, I will ignore all further language issues unless they really distort the content.

3018L13: Are we talking about a coupling of a GCM with a CTM, here or rather of the inclusion of a chemistry mechanism into a GCM?

3018L18: “coupling of an interactive ocean model is preferable” Why? I guess that depends on the scientific question.

3018L26: “evaluated using ... a pre-industrial control simulation” Such an evaluation is difficult due to the lack of comprehensive sets of observations. Please be more specific.

3018L31: “Finally, we close ...”

3019L1: “The coupled model consists of ... coupled to ...”

3019L7: Is SOCOL version 3 used here?

3019L10: Are really need five references to MEZON needed? This seems like excessive self-citing to me. If all are needed it should be said what for.

3020L12: “Chemistry climate coupling: ...” It sounds like the only coupling between MEZON and MA-ECHAM5 is done via the radiative effects of the trace gases. What about their transport? Isn’t this done by the winds calculated in the GCM. And why have this point separate from SOCOL? Isn’t that the coupled MEZON-MAECHAM5? Later it is mentioned that chemistry is calculated only every two hours. It would be interesting to discuss the potential error introduced by this. In the abstract (where it may not belong) it is already mentioned that NOCHEM has issues because of the missing diurnal cycle in ozone. If one prescribes low daytime ozone this may not be a severe problem. Instead using the high nighttime ozone for up to two hours after sunrise could cause problems. I would also like to know more about the coupling details (the way of operator splitting for instance). Additionally about transport: Is this done via the transport scheme of ECHAM or of the MEZON CTM. How is water vapor dealt with, which experiences phase transitions (in the GCM) and chemical reactions (in the CTM part). Maybe all this is discussed in earlier SOCOL publications. In this case, references would suffice.

3021L16ff “SOCOL does not use zonally averaged ozone concentrations ...” The meaning of this sentence gets only clear on page 3023 where it is said that even in the NOCHEM simulations 3D ozone is used? It is mentioned that the zonal structure is relevant for dynamics. However, prescribing 3D ozone means also that zonal structures in dynamics and ozone will be inconsistent occasionally. To me it is not a priori clear that this inconsistency is a priori preferable to the

inconsistency arising from the prescription of zonally averaged ozone. This needs to be discussed.

3021L18: “ozone forcing” I find the use of the word “forcing” excessive in this document. Maybe I’m wrong, but I would use it only in the case where a quantity is changing. Please check all the document.

3022L18ff: repetition

3022L24: What does “from scratch” mean?

3023L6f: repetition

3023L8/12: 1367 or 1368 W/m²?

3023L9: “positive temperature drift” Which temperature?

3024L12: “TCR is ... temperature change in the 20yr period” Or the difference between the mean temperature during this period and the initial temperature? And again: Which temperature?

3024L20: “All other forcings are constant at 1990 level” It was said earlier that the control run uses constant 1600 conditions. How can a climate sensitivity be calculated when boundary conditions other than CO₂ are changed? I think these simulations need to be rerun. Or at least a very thorough discussion would be needed to argue why this doesn’t matter.

3025L7: “Emissions are based on ... concentrations” How is this done?

3025L25ff: repetition.

3028L16: In view of the discussion of climate effects from the different types of forcing it would be useful to state what aerosol effects are included in the model.

3030L26: The MPI-ESM is based on ECHAM6 and has a preindustrial equilibrium temperature of about 13.5C. I guess the authors mean the ECHAM5/MPIOM model.

3031L3ff: I guess, again, not the MPI-ESM but ECHAM5/MPIOM is meant. Even then: Is it clear that the energy imbalance in the new SOCOL-MPIOM is the same? Could new imbalances have been introduced? E.g., through the coupling of water vapor?

3031L23ff. Why is it remarkable that absolute variability is small where absolute values are small? What is missing is a discussion of the maximum relative variability in the lower stratosphere.

3032L1: Do the authors really mean “intra-seasonal” variability? Or are they talking about inter-annual variability of seasonal means?

3033L1ff: This paragraph sounds like it is a general flaw of models without chemistry to produce too high temperatures in the mesosphere. Please see my earlier comments. I see no reason for prescribing daily averaged ozone concentrations instead of using daytime averaged values. On the other hand the potential flaws of calculating chemistry only every 2 hours should be discussed. (see comments above).

3033L6: It is claimed that during nighttime ozone levels increase due to upward transport. I would consider photochemistry as more important. Please provide references or discuss in more detail.

3033L14f: With interactive chemistry I would guess that another process causing differences is the photodissociation of water vapor.

3.1.3 Dynamics: It is an important question if dynamical variability is significantly altered by chemistry coupling. The results here are interesting because they seem to indicate that chemistry coupling plays a minor role. However, the discussion remains superficial and I would either suggest to make this into a major topic of this manuscript or to shorten it considerably.

3034L1ff: Interesting differences between CHEM and NOCHEM are the strengthened jets and lower polar temperatures. It is true that a stronger polar vortex would better isolate the polar air masses. On the other hand, winds and temperature are tightly connected through the thermal wind relation. So there could be a radiative origin of the stronger polar vortex. Why can one be certain that the stronger vortex comes first and what would be the cause for it?

3034L16ff: Tropical variability should not be discussed as it is mainly determined by the nudging.

3035L8ff: The discussion of the SSW frequency is lengthy including some unnecessary introductory remarks on their relevance for tropospheric climate. I would summarize the result saying that statistics for CHEM and NOCHEM are both very similar to those for ECHAM5. This can be shown and said in a much shorter way. When comparing preindustrial simulations to ERA data the different periods/climate states should however somewhere be mentioned as a caveat.

3036L14ff: Also the part on differences in zonal wind variability should either be shortened or the discussion should be done more thoroughly. One may e.g. speculate that polar ozone provides a positive feedback for the vortex strength in spring. While results for the NH seem to confirm this, the SH results do not. Why?

3.2 Tropospheric and surface changes: The discussion of the cirrus cloud effects in the SH high latitudes is interesting. Are there any other studies on the potential influence of stratospheric water vapor on high-latitude cirrus? The rest of the section where mostly small differences between CHEM and NOCHEM are reported could be shortened significantly. In particular the last part on surface variability could become one sentence.

3.3 Climate sensitivity: As said in earlier: If really the 1%CO₂ and 4xCO₂ simulations differ in boundary conditions other than CO₂, they cannot be used to estimate climate sensitivity. If the climate sensitivity stays high in corrected experiments it would be interesting to discuss reasons for the discrepancy to the ECHAM5/MPIOM. A potential candidate are cloud feedbacks. This could be analyzed easily by calculating cloud radiative effects.

3040L2: "second double CO₂ simulation" It is confusing to name the 1%CO₂ simulation "double CO₂" (also in table 1).

3040L8: Again: ECHAM5/MPIOM is not equal to MPI-ESM.

3040L21: The title of this section is almost equal to the title of subsection 4.2.1.

3040L24: “surface air temperature” In earlier sections the terms “2m temperature” or just “temperature” was used. I would prefer to talk about SAT, but anyway, please be consistent.

Section 4.1: I’d suggest to cut this section. It is not without interest to see that the large difference between the solar forcings of the L and M simulations do not really matter much, but the section is very descriptive and I think the aim of the paper to present the new model wouldn’t suffer from omitting this part.

3044L20: “The UV variability is not important for the surface climate.” It is often claimed that the UV variability would impact the surface response pattern by top-down mechanisms. In case the authors decide to keep this section, a more thorough analysis of this phenomenon (and why it seems not to act in this model despite the large solar forcing) would be needed.

3044L25: It is a very strong statement that “a significant anthropogenic influence on global mean temperature starts ... with the beginning of the industrialization”. Attribution studies using fingerprint methods have often indicated that the anthropogenic influence is only evident since about the 1980s.

3045L25 “TOA”: This may cause confusion. Isn’t TSI usually given for 1AU? Radiative forcing is in general defined for TOA. Maybe it would be useful to already at this point estimate the radiative forcing from this TSI difference.

3045L25f: This is interesting. Is there any reason why the ocean heat uptake is so different?

3046L14: Why give the temperature change in K/100yr. Why not simply as a difference between the selected periods?

3047L3: It is very interesting that the 20th century warming simulated by SOCOL-MPIOM is much larger than the average simulated by CMIP5 models while apparently the future projected warming is not. Why doesn’t the high sensitivity act in the future?

3048L3ff: All relative contributions to warming are probably estimated with some error margin. Could it be that the error margin of the total 87% includes 100% so that no additional feedbacks would be required?

3048L27ff: I guess an albedo of 0.3, not 0.7 was assumed. In the estimation of radiative forcing, was stratospheric adjustment considered? I guess that quite some percentage of the TSI variability comes from the UV that does not necessarily warm the troposphere.

3049L9: Unit of sensitivity is wrong. Please check all over the document.

3049L17: What does “inclusion of the additional RF from ozone” mean? Additional with respect to what was used in other CMIP5 models? Many of those have used the Cionni et al. ozone climatology which includes a trend. How different is the forcing in SOCOL from this standard dataset?

3051L4f: Please check the bracket.

3051L23: Is the ECS really “commonly assumed to be independent of climate state”? I wouldn’t think so. At least for very different climate states I don’t know of this common assumption.

3051L27ff: Is there an explanation for the increased TCR caused by chemistry coupling in the SOCOL-MPIOM? If the ozone change is less than 1/20 of that described in the cited paper, it must have a different cause in SOCOL-MPIOM.