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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of prescribed SSTs on climatologies and long-term trends in CCM simulations" *by* H. Garny et al.

H. Garny et al.

Received and published: 1 May 2009

Response to reviewer comments on 'Impact of prescribed SSTs on climatologies and long-term trends in CCM simulations'

Firstly, we would like to thank all referees for their comments and suggestions that certainly improve our paper. The reviewer comments are answered in detail below:

Anonymous Referee 1

Received and published: 5 March 2009

Review of ACPD manuscript 9-4489-4524-2009 Impact of prescribed SSTs ... by Garny et al. This manuscript describes two simulations of present day climate that use different boundary values of Sea Surface Temperature (SST). The model used is well documented in other papers, and the nature of the simulations is identical to that *prescribed in SPARC/CCMval. The main result of this study is that two simulations with the same identical model but different SST produce similar overall climatology but different trends when examined on shorter time spans (Fig. 9). The main concern I have about this study is that a result like Figure 9 could be misleading if not supported by additional information. I wonder if using another SST, one that is not consistent with the physics of that model, is in fact the cause of this pathology. I am not aware of how this model was tuned in the first place, but I know that when a model is put together the clouds, convective parameterizations, hydrological cycle etc are all tuned with some input dataset. Maybe the model used in this simulation in concert with one of the two SSTs is bringing the model climatology off track, and without proper re-tuning it gives rise to those different trends. To me it looks like that in one case the model is trying to compensate for an internal balance in the first couple of decades and then it finally catches up with the expected trend. Have the authors verified that global energy budgets (in/out, net, top/bottom) are correct and comparable in the two simulation? I think this is extremely critical and once this is properly shown I think the results would be much more robust.*

First of all, we want to note that the usage of different SST data sets with the same model is common practice in the chemistry-climate modelling activities, and as the reviewer noted correctly our simulations follow the set-up as defined for the SPARC/CCMVal projects. Furthermore, if we would retune a model when changing the SSTs, it would no longer reveal the sensitivity of the modelled atmosphere on the SSTs, since differences could as well be due to the different model version. Of course we do not want to mollify the reviewer's objection with the fact that this proceeding is common practise. Instead, we followed the suggestion to verify the global energy budgets of the two simulations. The global mean budget of radiation at the top of the atmosphere averaged over the 40 years of model integration evaluated in the paper is given by:

mean REF1/SCN2 short wave: 238.7430/ 238.6631 W/m2

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mean REF1/SCN2 long wave: -237.0033/ -236.0702 W/m2

mean REF1/SCN2 sum: 1.7397 / 2.5929 W/m2

The difference of 0.85W/m2 in the total radiation budget between the two runs can be considered as small and the lower outgoing long wave radiation in SCN2 is consistent with generally lower SSTs and a colder atmosphere (see Figure 3a). Of course the net radiation budget of +2.6W/m2 in SCN2 is not ideal, but we hope that the reviewer agrees that this does not justify the workload of retuning the model. The offset between REF1 and SCN2 in radiation remains very similar when considering only the first or the last decade of model integration, indicating that both the simulations are well in equilibrium after the 10 years of spin-up that preceded the analysed periods in both runs.

also, the authors need to indicate with supporting evidence from observations which trends are realistic.

We agree with the reviewer that the evaluation of results from chemistry-climate model simulations is of major importance for interpreting the results. The REF1 simulation used in this study was compared to observational data recently in Stenke et al, 2008b (Stenke, A.; Dameris, M.; Grewe, V. Garny, H.: Implications of Lagrangian transport for coupled chemistry-climate simulations, Atmos. Chem. Phys. Discuss., 2008b, 8, 18727-18764). During the development of this paper, we did plan to include observational data at the start, but put this plan aside due to the following reasons:

1. It proved that the 40-year trends in the two simulations, REF1 and SCN2 (shown in Figure 6), are not statistically significant from each other. Therefore, a comparison to observations could not prove one simulations being closer to reality than the other one.

2. It would be very desirable to include observational time series in Figs. 7 and 8, but the availability of data that starts as early as 1960 is poor. Even time series that start in the 1970s are too short as to make any interference about trends for the period 1960

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to 1980. If it would be possible to evaluate the anomalous trend behaviour in REF1 in the first half of the simulation against observations, it would certainly add value to the paper. However, as discussed in the conclusion, we were not able to do so at this stage of the work.

3. The overall aim of the paper is not to evaluate the model but to study the robustness of the model behaviour against the prescribed SSTs. The evaluation and comparison with other CCMs will certainly be the subject of future publications.

Until then I cannot recommend the manuscript to be published.

Detailed comments:

Page 4495. The term on the rhs of the X710;2 expression should be squared.

Thank you for the correction, the expression will be changed accordingly.

Figure 2. How many ENSO are forced from each dataset? The global averages are useless in order to assess actual and relevant variability. Suggestion: plot NINO3.4-like index.

The ENSO time series of the two SST data sets will be included in the revised version of the paper as a new figure (see also other referees comments).

Figure 3. Ozone has such a small concentration in the troposphere that even statistically significant anomalies below 732;100 hPa are questionable since they apply to near zero values. I think water vapor would be a much more useful constituent, both as a tracer in the UTLS region (tape recorder) and in the troposphere to highlight regions of enhanced/inhibited tropical convection. In fact, I would be curious to see the tape recorder in the two simulations.

We agree in it would be interesting to show water vapour in addition to ozone and temperature. However, since we have to limit the extent of the paper we want to focus on temperature and ozone for the following reason: The reviewer remarks correctly

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that ozone is mostly useful as an indicator of differences in the stratosphere and less so in the troposphere. However, the focus of the study is on the stratosphere and on ozone in particular since our main interest is in these simulations is the projection of the development of the ozone layer.

Figure 7 and 8. Page 4504, line 4. How do you reconcile this statement with the error bars that are overlapping in most cases?

We agree with the reviewer on that we have to be very careful with the interpretation of the time series shown in Figure 7 and 8, since the trends are not statistically different from each other when the error bars that represent the uncertainty on the trend coefficients overlap. The error bars are the larger the shorter the time series, which makes it even more difficult to make statements about the difference in trend for this period. However, in the northern high latitudes and in the tropics (upper and middle panel in Figure 7), for total ozone, the error bars do not overlap for including about 18-21 years, i.e. for time periods starting 1960 and ending in 1977 to 1980, and the statement on the trends in total ozone for that period is based on this result. In terms of temperature, the difference between the trends in REF1 and SCN2 are indeed only significant for northern high latitudes, and we changed the text to make that more clear.

Figure 10. This analysis is predicated on the assumption that stationary PW are the only players. The authors are neglecting the transient components.

In the calculation of the EP fluxes as performed for this study, the transient wave components are included as well as the stationary wave components, and we added a sentence clarifying this in Section 3.2. Therefore, Figure 10 does take into account both stationary and transient waves, so that the trend pattern refers to possible changes in both transient and stationary waves.

Page 4506, line 23. Why? Transient PW are not prevented from propagating into the summer easterlies

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The reviewer is right in that transient waves do not generally have to be prevented from propagating into easterly winds. However, it is documented in many studies that the transient wave flux in the summer stratosphere is strongly inhibited, see for example: Randel, W. J. Held, I. M., Phase Speed Spectra of Transient Eddy Fluxes and Critical Layer Absorption. J. Atmos. Sci., 1991, 48, 688-697. (especially their Figure 8). The climatology of EP fluxes for the model used in this study (see Stenke et al. (2008b)) shows that EP fluxes (again including transient waves, see above) into the summer hemisphere are strongly reduced, resembling results from reanalysis data.

Page 4507, lines 1-8. The chicken and the egg problem. Are the changes in EP resulting from different zonal mean zonal winds, or are the the waves in fact causing the changes? the question cannot be answered easily without further analysis.

Thank you for commenting on this very important and crucial point. Indeed, the question on the underlying mechanisms of changes in EP fluxes and associated changes in zonal winds is not easy to answer, and needs detailed and thorough analysis. We chose to not go too far on analysing the cause-effect relationship between wave fluxes and zonal winds in this study, since this would go beyond the scope of the paper. But in fact, the analyses of mechanisms of changes in EP fluxes are the subject of ongoing work and we hope to be able to draw some more light on the matter in a future publication.

Page 4509, line 5. I don't understand, aren't the two simulations set up identically for GHGs, the only difference is the use of the SST dataset.

This paragraph was formulated in a misleading way, and is changed in the revised manuscript to:

As discussed above, the decrease of the BDC in REF1 in the 1960/1970s seems to arise from SST-driven changes in PW generation. However, from our current findings we can not conclude on SSTs being the drivers of the positive trend in the BDC as found in SCN2 and in REF1 after 1980. Other processes could be responsible, like

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changes in the mean zonal winds due to changed GHG concentrations that lead to increased extratropical PW activity. What can be concluded on is that using different SST data sets results in differences in trends in the BDC, so that the SSTs seem to be capable to modulate the BDC.

Conclusions. What is the practical implication of this work? Should we stay with observed SST and avoid fully coupled models? Or these result suggest that the use of SST/sea-ice data sets coming from other models is to be avoided?

The intention of this study is to assess the uncertainty in the projections of atmospheric quantities introduced by the uncertainty in the boundary conditions, in this case the SSTs. Since chemistry-climate models are so far commonly not coupled to an interactive ocean-model, the simulations are subject to the use of prescribed SST data sets. For future projections, the only choice is to use SSTs simulated by a Climate Model. So the answer to the second question is no, we should and can not stay with using observed SSTs. Obviously the simulated SSTs for the future will never predict exactly the future development in reality, and we do not know what the 'error' in the simulated SSTs is. This is first of all due to our limited ability to model the climate system, but in addition we know that since we deal with a chaotic system, there are many possible 'realisations' of the future, meaning that even if we would be able to model the climatological state of the atmosphere and ocean exactly according to reality, there would be differences in the year-to-year variability (for example the years in which El Nino events occur). However, let's assume that the understanding of the climate system, and therefore the ability of modelling it, will apply in the same way to the future as to the past. In that case the difference between the modelled SSTs and the observed SSTs (i.e. 'reality') will be roughly similar in the future than in the past, both in terms of climatologies (i.e. 'offset' between the data sets) and variability. So by analysing the dependency of, for example, stratospheric ozone trends on the underlying SST data set we can gain an estimate of the uncertainty in the ozone trends due to uncertainties in the SSTs. If, for example, the trend would be significantly positive when using modelled SSTs but

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significantly negative when using observed SSTs, how can we know that the trends we estimate for the future (obviously using modelled SSTs) will have anything to do with what happens in the real future? Luckily, the results of the study indicate that trends in ozone and temperature do not differ fundamentally from each other. Therefore, the conclusion of the study is that the long-term trends in the analysed quantities are robust against the use of a different SST data set, but the trends can differ on decadal time scales. We added this discussion to the paper to avoid misleading conclusions of our work.

Anonymous Referee 2

Received and published: 31 March 2009

Garny et al. investigate how prescribing different sea surface temperatures (SSTs) in Chemistry-Climate models (CCMs) leads to different temperature and ozone climatologies throughout the atmosphere. Figure 3a demonstrates that colder SSTs lead to colder climatological temperatures throughout the troposphere, and warmer climatological temperatures throughout the stratosphere. It is interesting to see this result on the climatological time scale. However, the idea that colder SSTs lead to colder tropospheric temperatures is well known, for example: E. Yulaeva and J. M. Wallace, 1994: The Signature of ENSO in Global Temperature and Precipitation Fields Derived from the Microwave Sounding Unit. Journal of Climate, 7(11), 1719-1736, and the authors might like to include this or other relevant references. In fact there are also a few papers that show colder SSTs lead to warmer temperatures in the stratosphere, for example: Figure 3 of A. J. Clarke and K-Y. Kim, 2005: On Weak Zonally Symmetric ENSO Atmospheric Heating and the Strong Zonally Symmetric ENSO Air Temperature Response. J. Atmos. Sci., 62(6), 2012-2022. Figure 4 of R. García-Herrera, N. Calvo, R. R. Garcia, and M. A. Giorgetta 2006: Propagation of ENSO temperature signals into the middle atmosphere: A comparison of two general circulation models and ERA-40 reanalysis data. J. Geophys. Res., 111, D06101, doi:10.1029/2005JD006061 Figure 2 of S. C. Hardiman, N. Butchart, P. H. Haynes, and S. H. E. Hare, 2007: A note on *forced versus internal variability of the stratosphere. Geophys. Res. Lett., 34, L12803, doi:10.1029/2007GL029726 and this fact should also be made clearer.*

Thank you very much for pointing out these valuable references to us. The correlation of ENSO and the meridional circulation, as discussed in the above references, is very relevant for our study, and we decided to include another short section on the Influence of ENSO on tropical upwelling in the revised version of our paper. Furthermore, we added appropriate references on the general influence of SSTs on the temperature climatology and a remark on the agreement of the difference pattern we found in our paper with previous studies.

The paper goes on to consider the impact of SSTs on the ozone climatology, making the point that the Brewer Dobson Circulation is key to the redistribution of ozone throughout the stratosphere, and touching briefly on other mechanisms affecting stratospheric ozone. However, no mention is made of the positive ozone anomaly found in the Southern Hemisphere polar stratosphere (Figure 3b). I think it likely that this is due to the positive temperature anomaly there (Figure 3a) leading to fewer polar stratospheric clouds and thus less heterogeneous ozone destruction, especially since the positive anomaly is largest in September/October (Figure 4a). A comment to this effect would be illuminating, if the authors can compute a time series of polar stratospheric cloud area (from daily September temperatures, for example) to confirm this is the case. Figure 6 is also consistent with this idea, showing a greater negative temperature trend in the Southern Hemisphere polar stratosphere in REF1 than SCN2 and a corresponding greater trend in ozone loss there in REF1 than SCN2. Again, confirmation that the model is near the threshold temperature for PSC formation around this region would be very interesting.

We thank the reviewer for this interesting comment on another aspect that we did not discuss in our paper. The mechanism of temperature differences or trends affecting ozone depletion in high southern latitudes seems very plausible, and we are grateful the reviewer drew our attention to this feature. Concerning the difference pattern shown in Figure 3, we want to note that the differences in ozone are statistically significant only in a small region near the model top, while the positive temperature differences in high southern latitudes are not significant anywhere above 100 hPa. Also the trends in Figure 4 are not statistically different from each other in the stratosphere. For that reason we prefer to not comment on the differences in the polar southern hemisphere in the paper.

Section 4.3 goes into a fair amount of detail about the structure of the EP flux anomalies shown in Figure 5. Whilst it has been shown in various publications that colder SSTs lead to less planetary wave propagation into the stratosphere, it would be nice if the authors could say something about the robustness of the more detailed features of the EP flux anomaly to changing SSTs that they discuss (perhaps by means of an ensemble of integrations, or a proposed mechanism for these more detailed features).

The reviewer is right in that we have to be careful in interpreting the details of the EP flux difference pattern. In this case, however, the differences are statistically significant and also occur not only in January (shown) but as well in other months (December, February). Also, ongoing studies with time slice simulations reveal similar trend pattern in the troposphere in response to SST changes. Unfortunately we do not have an ensemble of simulations available to check the robustness. Also, we prefer to not speculate on possible mechanisms at this state, but this will certainly be the subject of further work.

In section 6 the authors state that "the question of how SSTs can affect the excitation of planetary waves is poorly understood". Perhaps, however, there also ought to be reference to the paper: S. Ineson and A. A. Scaife, 2009: The role of the stratosphere in the European climate response to El Niño. Nature Geoscience 2, 32-36, doi:10.1038/ngeo381. which states that: "In the Pacific, the middle-latitude surface response to El Niño is a persistent signal through winter consisting of a deeper Aleutian low shifted towards the west coast of North America. This signal is well known from observations and models and has an equivalent barotropic structure. Its large horizontal

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scale projects strongly onto the first zonal harmonic or wave 1 ... in winter, it interferes positively and strengthens the stationary waves above their climatological amplitude"

Thank you for calling our attention on the ENSO related response in wave activity. As mentioned above we will include some more on this point in the revised version of the paper. The statement on the poor understanding of the response of wave excitation on SSTs was meant to refer to the response on extratropical SST anomalies (that are not connected to ENSO), and we will change the sentence accordingly.

Overall, Garny et al. is a good paper, giving a detailed study of how SSTs affect temperatures and ozone concentrations through their impact on the wave driven circulation. The conclusion that these effects are secondary to those of GHGs and ozone depleting substances is an important one for communities modelling 21st century stratospheric climate change and ozone recovery. Typographical errors:

Thank you for the corrections, the typographical errors are corrected in the revised version.

Global replace: except of ! except for Line 19 of page 4498: "However, due the relatively" ! "However, due to the relatively" Line 21 of page 4499: "month" ! "months" Line 14 of page 4501: "divergences" ! "convergences" ? Line 27 of page 4506: "descend" ! "descent" Line 2 of page 4507: "hypotheses" ! "hypothesis" Line 14 of page 4509: "de detectable" ! "be detectable" Line 7 of page 4510: "in theses" ! "in these" Line 16 of page 4510: "Holton, D." ! "Holton, J."

Anonymous Referee 3

The paper is investigating the sensitivity of the DLR chemistry-climate model to different sea-surface temperature and sea ice coverage data sets. A model integration "along" the HadISST data set is compared to an integration using the Hadgem1 calculated sea-surface temperatures. The paper documents the biases and changes in trends in the two different integrations. The subject of the paper is suitable for ACP.

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The paper is largely well written and is an interesting assessment, relevant for many CCMs. A number of small issues (detailed below) require clarification before publication. Subject to minor revisions I would recommend the paper for publication.

P4490L20: This result ? This sentence requires clarification. Is it just stating that the temporal behaviour is different in both runs? P4490L22: Be more quantitative about the trend reversal. When is it happening?

The sentences were changed to make them more comprehensive.

P4492L27: Even though ENSO impacts are cited no further discussion relating to the modelled low latitude response is provided later. Maybe the authors would like to reconsider this?

Thank you for the suggestion to address the impact of ENSO in more detail. We included references and discussions at several points, as well as an additional section analysing the influence of the ENSO on tropical upwelling (see also comments from referee 1 and 2).

P4496L09: "2" missing in Coriolis parameter.

Thank you for the correction; it is incorporated in the text.

P4499L18: "delete" respective

Thank you for the correction; it is incorporated in the text.

P4499L27: Clarification: maximum in column ozone Structure regarding 4.2 and 4.3: 4.2 introduces an idea that has been well explored with methodologies based on the mechanism introduces in 4.3 - I would suggest to swap the order of the subsections and to cite relevant literature (e.g. Fusco and Salby, 1999 and follow up work) to strengthen the argument, notwithstanding the fact that both "mechanisms" are not independent.

Thank you for confirming the validity of our argumentation on the influence of differences in the BDC (and therefore wave fluxes) on ozone anomalies. We follow your suggestion and add some citations of previous work on this matter. However, concerning the order of the subsections, we prefer to leave them as they are for the following reason: As the reviewer stated correctly, the results in section 4.2 are effects of the differences found in 4.3. Section 4 starts with discussing differences in ozone, and continues to seek for possible causes for the ozone anomalies in differences in the BDC (in section 4.2). Thereafter, Section 4.3 shows the differences in EP fluxes, as to explain the differences found in 4.2. So the chain of argumentation in Section 4 follows seeking causes for effects we diagnose, rather than the other way round, which would of course be another as valid possibility.

P4502L08 What does "mean" mean? HadISST is a monthly mean data set (as is Hadgem).

In this sentence, the 'Northern Hemisphere mean HadISST data' refer to the spatial mean of the HadISST data over the Northern Hemisphere rather than to a time mean. We will clarify the sentence with that respect.

P4502L20 Some more explanation is required with a stronger link to the figure. I assume the number in each panel is the correlation coefficient? The following discussion might also be the place for mentioning the ENSO link. (Hadgem1 has only a weak ENSO, but the HadISST ENSO should be a stronger constrain/external forcing).

The correlation coefficients are given in Figures 7 and 8 (see Figure caption). As stated earlier, we will add an additional Subsection on the Impacts of ENSO, and included a cross-reference to the section at this point in the text.

P4507L18 delete the "there's opening" for points 1 and 2.

Thank you for the correction; it is incorporated in the text

P4509L01 briefly summarise the Deckert and Dameris result.

We added a sentence on the key finding in Deckert and Dameris, 2008 at this point in the text.

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P4509L04 EP does not propagate! The refractive index is changing and wave propagation properties are changing and this is reflected in EP flux differences.

Thank you for the correction; it is incorporated in the text.

P4509L13 Be more specific about the trend reversal. The main finding in this paper seems to be related to the tropics, but the argument is supported with NH high latitude data. Obviously within the BDC framework the two regions are not independent, but try to be clearer (spell out the regions, refer the reader back to figures 6 and 9, etc.).

We follow the suggestion of the reviewer and clarify the argumentation of this discussion in the revised version of the paper.

P4510L11 Make it clear that you refer to TRENDS and DIFFERENCES between different decades in a consistent run, and not absolute values at a given time in the future.

The sentence is changed to clarify the statement according to the suggestion of the reviewer.

Remove some colloquial language: "reality perfectly", "used to learn", "fortifies", "don't"

Thank you for these corrections.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4489, 2009.