

## **Author response to reviewer' comments on "Role of external factors in the evolution of the ozone layer and stratospheric circulation in 21st century" by V. Zubov et al.**

First of all we would like to thank all reviewers for their suggestions which will be carefully taken into account to improve the manuscript.

### **Anonymous Referee #1**

#### **General remarks:**

1. The description of the numerical experiments was not very clear to me. That section might be easier to understand if the description of the runs were structured somehow differently (maybe start with the fact that five times 2-year runs were done, with a 10 year spin-up)

We have clarified the experiment description in the following form: "Then, the two year long run is repeated five times starting from the same initial conditions (the end of spin-up run) to generate five ensemble members using slightly (within  $\pm 0.01\%$ ) changed  $\text{CO}_2$  mixing ratio unique for each run."

2. One main concern for me is the fact that although five ensembles have been calculated for each experiment, only two years were calculated per ensemble. This seems not enough to capture the full range of inter-annual variability. As far as I recall, in the climate modeling community, time slice experiments are calculated for tens of years to be able to obtain statistically sound results. What was the reasoning behind the chosen two years? And how big are the differences between the two years in each experiment? How big are the differences between the different ensembles of the same experiment? I would like to see some information about this somewhere in the description of the experiment set-up.

According to the CCMVal recommendations (SPARC CCMVal, 2010, Chapter 2) time-slice experiment should consist of 20 years or more. For example ten years of the spin-up calculation are followed next 10 years of calculation to be able to obtain statistically sound results. We suppose that "statistically sound" means that according to ergodic hypothesis the last 10 years of the calculation are considered as 10 realizations of the climate system state. In other words ten year long experiment is considered as ten ensemble terms with the 1 year time duration. According to the theory of probability it is very desirable that the ensemble terms would not depend on each others. Certainly the consecutive calculation of the ensemble terms does not fully meet this condition because the ensemble terms follow each others in time. We think that our approach to the ensemble generation allows us to partly improve the above mentioned method. We have got ensemble terms with the smaller dependencies on each other because they last 2 years and are calculated in parallel mode but not one after the other in time. The parallel mode diminishes the ensemble term dependence from each other, and the 2 years duration of ensemble run decreases the dependence from the initial conditions.

Following sentence has been added to the text of manuscript: “We suppose that the applied scheme of the ensemble generation provides higher degree of independence for the ensemble terms against the ensemble generation way based on the consecutive calculation”.

3. It was very confusing for me to read about sea ice distribution and their impacts on tropical upwelling. I assume the SST/SI prescription is included in one set of boundary conditions, and therefore the explanation of the upwelling changes just includes the SI distribution. However, the changes in the ozone, zonal wind and temperature in the tropical lower stratosphere are, most likely, driven by SST rather than SI in the Arctic/Antarctic region. Maybe the authors could comment on that somewhere in the manuscript.

We have included in the text of manuscript the requested comment.

4. Although contour lines are given in the figures, the figures would be easier to understand if either a color bar was also given, or a short comment about the reddish colors being positive etc. in the figure caption.

Done. We have added a short remark to the figure captions: “Positive changes are shown in reddish colors while small or negative changes are colored in blue/violet”

5. Although the manuscript is written in a clear and understandable way, some small grammar and stylistic problems remain in the text. I would recommend the have a native speaker go over the manuscript.

We will check the language again. We guess extra check will be done by the publisher.

### Specific comments:

1. p. 28476, line 7-8: it is not totally clear that the cool ice covered surfaces in the Antarctic are actually the GHG molecules’ surfaces. Please rephrase.

We have rephrased this sentence in the following form: “In this case the cooling over Antarctica is much more pronounced because the additional GHG enhance the radiative exchange of the atmospheric layers with very cold surface of the Antarctic”

2. p. 28476, line 8-9: The sentence “Below 20 hPa ” refers to the tropics, I assume. This is not clear from the sentence. Please rephrase.

We have rephrased this sentence in the following form: “Below 20 hPa the SST/SI factor plays dominant role in the long-term temperature changes, especially in tropics and in the NH”

3. p. 28476, line 16: “the drop of ODS: ” implies a sudden change in ODS where it is actually gradual. Please rephrase.

We have rephrased this sentence in the following form: “The decline of ODS concentration leads to the ozone increase followed by a small heating which partially compensates GHG induced cooling in the stratosphere”

4. p. 28476, line 28: sentence seems to be only fractional (“ upper flanks in vertical direction, and.”)

We have finished this sentence in correct way.

5. p. 28486: Please specify in the table heading why some numbers are in bold.

Done. Bold font marks the factor/year, which contribution is studied in an experiment.

## A.Yu. Karpechko (Referee) #2

### Comments:

1. P28468L20: ‘...some differences between the simulated results could be caused by the applied SST/SI rather than by the CCM’s deficiencies.’ I suggest rewriting: ‘...some differences between the results by different CCMs could be caused by the applied SST/SI rather than by the CCM’s deficiencies.’

Done.

2. P28470L25: ‘They operate in the atmosphere via the acceleration or suppression of the main physical and chemical processes, which play only secondary role.’ Do you mean changes in timescales of the ‘main physical and chemical processes’? Please rephrase this sentence.

We have rephrased this sentence in the following form: “They operate in the atmosphere via the acceleration or suppression of the internal physical and chemical processes in the climate system.”

3. P28470L27: ‘Thus the attribution of the atmospheric changes to the external anthropogenic factors is the more important task in comparison with any other types of the attributions.’ Please specify which other types of attribution you mean.

We have rephrased this sentence in the following form: “Thus the study of the atmospheric response to the external anthropogenic factors is more instructive than the attribution to the internal processes such as temperature changes, photochemical loss/production and transport (e.g., Oman et al.,2010a), because these factors are independent on the atmospheric state”.

4. P28471L4: ‘...however the reasons for this uncertainty have not been clearly identified.’ Please discuss here the role of the internal climate variability. Can the uncertainty in the future ozone layer be to some extent attributed to the internal climate variability?

We have added some discussion to the text: “As it was shown by Charlton-Perez at el. (2010) the ambiguity of the future ozone evolution depends mainly on the model and scenario uncertainties, while the contribution of the internal model variability is small”

5. P28471L10: ‘However, it is not the case for the future SST/SI distributions taken from different models participated in IPCC AR4 assessment (IPCC, 2007) which are characterized by substantially different magnitude and pattern of the future climate change.’ I think you need to acknowledge here that the use of different future SST

changes allows, to some extent, sampling of the uncertainty due to atmosphere-ocean GCMs deficiencies, as well as the uncertainty due to climate variability. I agree that using different SSTs hampers CCM intercomparisons; however you need to differentiate the cases when the use of different SSTs is beneficial.

We have added some discussion of this issue in the manuscript.

6. P28471L28 Could you please be more specific what is meant by 'ensemble approach' here.

We have decided to remove the sentence, because the ensemble approach used in the study is presented in Section 2 in more detail.

7. P28474L21 'Then, the last two years of the run are "recalculated" five times with the slightly (within  $\pm 0.01$  %) changed CO<sub>2</sub> mixing ratio to generate five ensemble members.' I do not understand this. How many years does each of the ensemble members have?

We have clarified the experiment description: "Then, the two year long run is repeated five times starting from the same initial conditions (the end of spin-up run) to generate five ensemble members using slightly (within  $\pm 0.01$ %) changed CO<sub>2</sub> mixing ratio unique for each run."

8. P28476L22 'The SST/SI factor has quite different effects on the zonal wind in the SH and NH which consist of the eastward wind acceleration in the middle latitudes of the SH and deceleration in the NH.' Do you mean at about 10hPa and above? Below 20hPa the situation is symmetric between the hemispheres, at least qualitatively. Please be specific here.

We have done the recommended correction in the text of manuscript: "The SST/SI factor has quite different effects on the zonal wind in the SH and NH which consist of the eastward wind acceleration in the high and middle latitudes of the SH (except polar lower stratosphere) and deceleration in the high and middle latitudes of the NH."

9. P28476L28 '...extending their upper flanks in vertical direction, and.' There is a full stop after 'and'. Did you finish the sentence?

Done. We have corrected this sentence.

10. P28477L15 '...the tropospheric air with rather low ozone mixing ratio and its additional accumulation over the northern extra-tropics (Fig. 3c).' Put comma between 'ratio' and 'and' to separate the different parts of the sentence.

Done.

11. P28477L27: I suggest rewriting the paragraph discussing the RES term. The model simulations used here are not very long and cannot capture the internal climate variability at decadal and longer time scales, which can be significant. Therefore the RES term may represent not only interactions between the different forcings, but also decadal climate variability. I'm not sure which of the two plausible effects dominate. Overall, the figure demonstrate that the RES term is very small when compared to the dominant individual terms in TEM, U and O<sub>3</sub>, which should be the main message of the figure. It is not very

clear how the RES compares to the other terms in total ozone, because the units in Fig. 5D (DU) are different from those in Figure 4 (%).

The decadal climate variability could contribute substantially in RES term if it is pronounced in the FULL experiment, but not in the cumulative effect of ODS, GHG and SST/SI experiments. It is difficult to find a satisfactory explanation of such model behavior. The climate variability at decadal and longer time scales can result mostly from the changes of the ocean circulation and atmosphere-ocean interactions. The mentioned changes are included in the model experiments through the SST/SI variations, which are taken into account in both FULL experiment results and sum of the ODS, GHG and SST/SI effects. In Figure 5d we show RES term of the total ozone century changes in percents, but made a typo in Figure caption. Now this typo has been corrected.

12. P28478L1: Which polar stratosphere is meant here – southern or northern?

It is the polar stratosphere of the SH. We have clarified this in the text.

13. P28478L10: The ozone hole occurs during austral spring while the significant differences seen in Fig. 5D are in austral summer and autumn. Please correct.

Done.

14. P28478L20: 'SST/SI factor also has dominant contribution to the deceleration of the stratospheric westerly winds in the extra-tropical latitudes of the NH from 2000 to 2050 (Fig. 6a).' The location of the node separating the acceleration/deceleration regions is altitude depended. For example below 30hPa, the deceleration is only simulated in the polar latitudes north of 60N while there is an acceleration in the extratropics south of 60N. Please be more precise here.

We have added more accurate analysis of Fig. 6a: SST/SI factor also has dominant contribution to the deceleration of the stratospheric westerly winds northward of 60°N from 2000 to 2050 (Figure 6a).

15. P28479L21: Add 'by the GHG factor' to: 'The upper stratospheric westerlies are accelerated BY THE GHG FACTOR mainly in the SH.'

Done.

16. P28479L21: 'All these atmospheric changes are responsible for about a half of the century ozone concentration increase in the upper stratosphere and ozone decline over the Antarctic area.' There is no ozone decline over the Antarctic in the 21 century! The GHG alone would lead to a decrease in the Antarctic ozone according to Fig. 3B, but this decrease is much smaller than the increase due to the ODC removal.

We have clarified this sentence of manuscript to remove the inconsistency in the following form: "All these atmospheric changes induced by GHG are responsible for up to 50% of the ozone increase in the upper stratosphere. GHG factor is also responsible for some ozone decline in the lower stratosphere in tropics and over the Antarctic area (Fig 3b)."

17. P28480L13: '...in the second half of THE century'  
Done.
18. P28480L15: '...to the relevant column ozone changes' ! '...to the TOTAL column ozone changes'  
Done.
19. P28480L19: '...rather than by model features.' ! '...rather than by CCM features.'  
Done.
20. P28481L3: I think this sentence need to be rephrased, see also my comment on P28471L10. If the goal of CCM simulations is a model intercomparison then CCMs should be run with the same SST/SI. If the goal of CCM simulations is to obtain future climate projections then the use of different SST/SI is beneficial because it allows better sampling of different sources of uncertainty. If both goals are desirable (which is usually the case), then, ideally a separate set of CCM simulations should be planned, in which all CCMs are run with the same forcing including the SST/SI. Also, future model generations may include interactive ocean, which will likely have an effect on planning of model intercomparisons. You may want to comment on what implications your results possibly have for the evaluations of CCMs with interactive ocean.  
We have added suggested discussion to the conclusions.
21. P28480L28: The results of the sensitivity experiment with NCAR-ESM SST/SI should be reported in the Results section. I suggest moving this sentence to the Results section.  
Done. We have added the description of this additional simulation and moved this discussion to the Results section.

### Anonymous Referee #3

#### Comments:

P28471L10 "However, this is not". The SST/sea ice from GCMs used for the REF-B2 simulations also covered the past (1950-2100). On the other hand, for REF-B1 all models used the same HadISST sea surface forcing (for 1950-2007). Is it then the case that the inter-model spread is notably smaller in REF-B1 than in REF-B2?

The recommended experiments REF-B1 and REF-B2 are differed not only in SST/SI scenarios. REF-B1 includes the variability in the solar cycle, volcanic eruption effects, the variation of the ozone and aerosol precursors and the QBO effects which are absent in REF\_B2. All these factors are treated differently by the models providing additional source of the spread in the model results. Therefore, it is difficult to establish that SST/SI is responsible for larger spread in REF-B2 with high confidence level, however it well could be that it is the case.

P28474L21: Why do you recalculate things in 2-year time slices? The straightforward approach might have been to use the last 10 years of a 20-year simulation and evaluate that. Also

depending on your model and initial conditions, 10 years might be insufficient to achieve complete spin-up. Have you established that there are no remaining spin-up effects in your runs? Ten years of the spin-up calculation are recommended by CCMval community for time-slice experiments (SPARC CCMVal, 2010, Chapter 2). We have checked the time evolution of several quantities during the spin-up run and found a good convergence. We have also performed the FULL experiment with the 20 years spin-up calculation. The experiment results are very virtually identical to the results of FULL experiment with the 10 years spin-up computing.

Minor comments:

We have implemented a number of minor corrections suggested by the reviewer.

P28470

L18: “cannot be applied to an evaluation of the contributions of the main external anthropogenic factors”. Why not?

We have rephrased this sentence in the following form: “However, the MLR method has not been applied to the analysis of the main anthropogenic factors contribution simulated with CCMs”

P28471

L10 “However, this is not”. The SST/sea ice from GCMs used for the REF-B2 simulations also covered the past (1950-2100). On the other hand, for REF-B1 all models used the same HadISST sea surface forcing (for 1950-2007). Is it then the case that the inter-model spread is notably smaller in REF-B1 than in REF-B2?

The recommended experiments REF-B1 and REF-B2 are differed not only in SST/SI scenarios. REF-B1 includes the variability in the solar cycle, volcanic eruption effects, the variation of the ozone and aerosol precursors and the QBO effects which are absent in REF\_B2. All these factors are treated differently by the models providing additional source of the spread in the model results. Therefore, it is difficult to establish that SST/SI is responsible for larger spread in REF-B2 with high confidence level, however it well could be that it is the case.

L24: I take it that the CFCs count as ODSs but not as GHGs. Or do you take a more sophisticated approach and treat them separately for chemistry and radiation?

Yes, the GHG group includes CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O only, but not CFCs. We have refined the sentence.

L28: “The ensemble approach offers an opportunity”

We have revised and moved the sentence to Section 2.

P28474

L21: Why do you recalculate things in 2-year time slices? The straightforward approach might have been to use the last 10 years of a 20-year simulation and evaluate that. Also depending on your model and initial conditions, 10 years might be insufficient to achieve complete spin-up. Have you established that there are no remaining spin-up effects in your runs?

Ten years of the spin-up calculation are recommended by CCMVal community for time-slice experiments (SPARC CCMVal, 2010, Chapter 2). We have checked the time evolution of several quantities during the spin-up run and found a good convergence. We have also performed the FULL experiment with the 20 years spin-up calculation. The experiment results are very virtually identical to the results of FULL experiment with the 10 years spin-up computing.

#### **Anonymous Referee #4**

##### **General comments**

The authors analyse the different factors governing the development of the future ozone layer and stratospheric dynamics. By performing model experiments where ODS, SST and GHG concentration is varied combined and exclusively, they derive the corresponding contribution to temperature and ozone trends. The authors' main conclusion is that SST is a dominating factor for the future ozone development, and that future model intercomparisons should carefully plan this boundary condition to control its impact on model results.

This statement is not completely correct, because the reviewer suggests that we deny the importance of the ODS removal, which is obviously not true. In the abstract we emphasize that SST plays dominant role in the ozone evolution only in the tropical and northern lower stratosphere.

In addition, their study allows to evaluate the importance of non-linear interaction between ODS, GHG and SST, showing its role to be minor. Whereas the authors present a clear and focussed study of the influence of main factors of future stratospheric ozone development by itself, it is not clear to which extent their main finding is new or if this study really brings any progress to the understanding of the underlying mechanisms.

Our results concerning the role of SST in the ozone evolution are novel and may foster the understanding of the underlying mechanisms. We think it was not really recognized that the ozone evolution in the tropical lower stratosphere is so sensitive to the ocean temperature changes. The experiments performed in the framework of the SPARC CCMVal campaign do not allow attribution of these trends, because the SST changes were considered together with the GHG changes in the atmosphere. Our results suggest that the process oriented model evaluation should include not only radiation, chemistry and dynamical processes in the stratosphere, but also the forcing and feedbacks in the troposphere which are responsible for the warming of the ocean surface and upper troposphere.

For example, even simple analysis of the strength of the BDC for the different scenarios is not undertaken in the paper (via mean age of air or stream functions), nor any analysis of planetary wave activity is presented. The authors do not put their results in context to other work and do not discuss possible differences or agreements.

We disagree with this statement. First of all we have analyzed the evolution of the atmospheric dynamics in the 21th century in terms of the residual circulation concept (residual velocities, planetary wave sources, divergence of Eliassen-Palm flux) using the same version of the CCM



SOCOL and similar model set-up (Zubov et al., 2011). The BDC response to combined GHG and SST forcing was studied in details in several recent publications which were cited in our paper (e.g., Calvo and Garcia, 2009; Deckert and Dameris, 2008; McLandress and Shepherd, 2009; Shepherd and McLandress, 2011). We do not think that the addition of BDC analysis to our paper will provide some new insights. According to the reviewer suggestion we have added some discussion about the agreement of our zonal wind changes with the previously published analysis.

In addition, the authors themselves remark that the used SST in their experiments is somewhat extreme, leaving the reader with a mixed feeling how conclusive this study is.

We have also run the experiment with the SST acquired from NCAR ESM, which was mentioned in the manuscript. The obtained results showed that the acceleration of the tropical upwelling (and consequently the ozone depletion in the tropical lower stratosphere) became smaller, but SST influence still dominates in the area.

I therefore cannot recommend publication of the paper in its present form. The study needs in my opinion additional experiments and a deeper analysis of underlying dynamical mechanisms to be publishable in ACP as a regular article. If the authors intended to bring to attention how important the SST/STI setup for model intercomparison are, one could also consider to present the result to the community as a technical note.

We think that the reviewer' opinion is strongly biased. Obviously, the cause of the tropical upwelling intensification is very important problem. There were several publications (e.g., Oman et al., 2010; Lin and Fu, 2013, doi: 10.1029/2012JD018813) where the increase of the tropical upwelling was identified, but not properly attributed. Charlton-Perez et al., (2010) analyzed the sources of uncertainties in CCM and did not consider SST as an external factor. Our results can help to better formulate the set of experiments for the upcoming SPARC CCMII campaign, therefore we think that our paper cannot be considered as pure technical.

## **2 Specific comments**

### **2.1 Some general aspects**

The general importance of the SST for the strength of the BDC is not a new result and many aspects have been discussed in the literature (in addition to Deckert and Dameris 2008, or Kodama et al. 2007, see also eg. Rind et al. 2002, Garcia and Randel 2008, Olsen et al. 2007) applying similar analysis as presented by the authors. The authors should put their results in this context.

SST acts via planetary wave generation, propagation and breaking in the stratosphere on the strength of the BDC which itself affects ozone transport. None of these factors is analysed in the paper, and even an analysis of the model's BDC for the different realisations (for example using a mean age air tracer) is missing.

The goal of our paper is to evaluate contributions of the main anthropogenic factors to the stratospheric ozone and dynamics changes during 21th century, but not the detailed study of the connection between SST variations and BDC strength. We have decided to focus on the analysis

of the ozone, temperature and zonal wind changes because they provide enough information about the evolution of the atmospheric dynamics. The analysis of the atmospheric dynamics variation for 21st century in terms of the residual circulation concept (residual velocities, planetary wave sources, divergence of Eliassen-Palm flux) with the current version of SOCOL has been performed and published by Zubov et al., (2011), where the most publications mentioned by the reviewer were included. In the revised version we will also include the publications requested by the reviewer in the Section 3. We have to admit that the logic of the reviewer is not clear. We know that BDC accelerates in warmer climate. We know that the cooling and ozone depletion in the tropical lower stratosphere is the result of BDC intensification. We tried to understand which part of the model is responsible for this and recognized an important role of SST, which is unfortunately an external forcing for the majority of applied so far CCMs. What kind of new information we can get if we analyze EP flux (for example) in more details. Would it change the conclusion about the SST role?

For model intercomparison projects like CCMVal the authors point to the role of SSTs and that differences found between the models could strongly depend on the chosen SST. The clarification of the impact of SST and their implementation is therefore of substantial interest for such projects. For SOCOL however, the SST used is an extreme example. It would have been of more relevance esp. in the context of the CCMVal activity to use a SST in SOCOL which other model had applied and to analyse if SOCOL then is closer to the model mean. Perhaps the additional experiment with a different SST distribution (NCAR ESM) can be checked for that. The extreme SST realization used here calls for further analysis, for example comparison of the SST field with a more typical realization in CCMVAL. Experiments are desirable where a moderate SST is applied and to see if the conclusions drawn in the paper are still valid, for example with regard to the RES term. In addition, it would be of further interest to study how sensitive results depend also on spatial patterns (low-lat/ mid-lat gradients) in the SST.

We have run the model using SST/SI from NCAR ESM and got substantially smaller increase of tropical upwelling but the conclusion about the SST contribution to the tropical ozone evolution remains valid. This fact was mentioned in the conclusion, but in the revised version it will be moved to the Results by A. Karpechko (Reviewer#2) request.

The paper lacks a discussion of the results with respect to previous work. It remains unclear if the conclusion presented in this paper are somehow new or if they are just in line with common knowledge.

We have included in Section 3 of the paper the discussion concerning findings of the previous works.

## 2.2 More specific aspects

The model setup is hard to understand. Does it mean that after 10y spinup CO2 is slightly varied, and then the run is continued for two years, five times using the same start conditions? Do the five results of the second year represent the ensemble, or the two years are used? Why did the

autors choose this approach? Can the authors exclude a preconditioning of the ensemble member which could make the results not representative?

Please, read our answer to reviewer#1 general comment 2 and reviewer#1 comment 7. We have clarified the experiment description in the text. We have chosen this approach because it is more computationally efficient for our not parallelized model and availability of 10 CPUs. The results obtained using the applied experimental set-up were compared with the results obtained using more typical set-up (10 years spin-up and 10 years time slice) and it was found that the model response is robust, therefore the applied scheme is representative.

Inspecting Fig. 3-6 of WMO-report 2010 the SOCOL models and EMAC show a strong decline of tropical total ozone, despite they seem not to use the same SST data set (NIWA uses HadGEMI). This hints to a specific property of ECHAM type models. Can you comment on that?

The problem here is that SOCOL-NIWA and EMAC did not provide the data for REF-B2 experiment in time (WMO report, Table 3-1) but with a delay. Therefore it is not clear which SST/SI data sets was applied for future simulations (WMO report, Table 3-2). From our experience we may conclude that they used recommended SST/SI from ECHAM-MPIOM experiment similar to the one used in SOCOL.

### 3 Technical comments

Generally, the paper would strongly benefit from a correction by a native English speaker. Very often articles are missing, and expressions are sometimes unusual.

Page Line Comment

28468 L2 contributions

Done.

28468 L5 the atmospheric ...

Done.

28468 L6 you may write here: ..., and the prescribed SST ... and leave the next sentence out

Done.

28468 L10 It was found

Done.

28468 L16 expression

Comment is not clear.

28468 L20 which simulation do you mean?

We have rephrased the sentence.

28469 par 1 Just leave that paragraph out

We have kept the paragraph 1 on page 28469, because the presented properties of CCMs are important for the study.

28469 L20 The SST must be considered as an external factor because SOCOL and the majority of models participating in CCMVal have no interactive ocean.

Done.

28470 L9 y or years

Done.

28470 L11 use of article: the total tendency of the ozone layer and the dynamics throughout the 21st century; here and at many other places

Done.

28470 last para start with line 1 on p28471

Comment is not clear.

.28471 l6 by some differences

We have removed "some".

28471 l25 latter

Done.

28472 Section 2.1 Has the model a (spontaneous or forced) QBO?

SOCOL has a prescribed QBO.

28475 l14  $\text{abs}(x) > 0!$

We have revised the sentence to:

...factors enhances ( $|\text{FULL}| - |\text{GHG} + \text{ODS} + \text{SST/SI}| > 0$ ) or compensates ( $|\text{FULL}| - |\text{GHG} + \text{ODS} + \text{SST/SI}| < 0$ )....

28476 l6 this conclusion cannot be drawn without analysis of the radiation budget, esp. analysis of vertical velocity

The distribution and magnitudes of the residual vertical velocity over the high latitudes cannot provide the reasonable explanation of the GHG temperature changes. The velocity values change their sign from the poles to equator. Thus adiabatic compression/expansion heating from vertical motions cannot explain the latitudinal homogeneity the GHG temperature changes in the stratosphere. We have included this analysis in the text of manuscript.

28476 l10ff as before this conclusions are not drawn from material presented here. Please give a reference, or the necessary analysis.

Besides the convection, the radiation transfer and the turbulent thermal conductivity can redistribute the warming of the sea surface to the middle and upper troposphere. However, the distribution of the GHG induced temperature changes in the tropical troposphere ((maximum of the temperature increase locates in the upper troposphere)) cannot be explained by two latter processes. Thus, convection is the most likely way to transfer the energy from the sea surface to the middle and upper troposphere in the tropics. See also the discussion in (Rind et al., 2002; Olsen et al., 2007). We have added this citation in the Section 3 of the manuscript.

28476 l25 what do you mean with SST forcing?

SST forcing means the influence of the SST changes over the century on the atmospheric and land surface.

28476 l28 sentence ends abruptly.

Done.

28478 l8 Fig. 5d shows negative RES values in the polar SH. That means TOZ is smaller in the full run compared with the sum of single runs. Why is then the recovery accelerated?

We have replaced the misprinted Fig 5d to the right one.

28478 l28 ..., that the resulting ozone reduction compensates completely ozone increases due to reduced ODS.

Done.

28479 l25 vanishing instead of dissipation

Done.

28487 Figure a - d is not given in the Figure itself. Please use SI units.

The temperature changes are given in Fig. 1 (page 28487) in Kelvin or briefly K. Kelvin is SI unit. We hope that a little bit out-of-day "mb" does not misrepresent the result.