

## **“Northern winter stratospheric ozone responses to ENSO inferred from an ensemble of CCMs”, Response to the reviewers**

We would like to thank the two reviewers for their constructive comments that have helped us to clarify the presentation and the interpretation, improving the manuscript.

### **List of major changes in the manuscript:**

- The interpretation of the  $\Delta Z$  index scatter-plots is refined and extended (see response to reviewers).
- Figures 3, 5 and 6: The format of the scatter-plots is modified, in order to highlight the clustering of the ENSO response for the majority of the model simulations.
- An Appendix is added, in order to clarify the relationship between the 500 hPa stationary eddy geopotential height anomaly and the 100 hPa heat fluxes.

### **Response to Referee 1**

#### **Major comments:**

(1) First we need to notice that our motivation of Figure 3 is to possibly identify a bias in the modeled ENSO response in the troposphere of the CCMs. Second, we have not used heat fluxes and/or EP fluxes also because available only for too few models. Third, we need to keep in mind that all the indices defined in the manuscript have been derived from the average of the four strongest ENSO events with respect to the average of the NEUTRAL years: The spread in the  $\Delta Z$  index in Figure 3 is not a measure of the ENSO forcing, but a measure of the (modeled and observed) averaged tropospheric midlatitude response to the ENSO forcing (i.e., the anomalous sea surface temperatures in the Equatorial-Central Pacific). We agree with the reviewer that it would be of interest to investigate a linear relationship between a form of the

$\Delta Z$  index and the vertical EP flux across the tropopause. One way we envision we could do that would be by comparing ENSO events of different intensity (for example weak, medium, and strong). A large number of ENSO events for each of the categories, each one with its own  $\Delta Z$  index, would then be needed for this investigation, outside the possibility and scope of this work.

Within the limits of the available data and within the scope of our work, we can instead show, for example for one simulation from the MRI model (for which the heat fluxes are available) in Figure A: (top) The correlation between the February-March polar cap averaged temperature at 50 hPa and the January-February heat flux anomaly at 100 hPa averaged between 45-75N, for each single year of the MRI simulation; and (bottom) The correlation between the January-February heat flux anomaly at 100 hPa averaged between 45-75N and a new  $\Delta Z^*$  index (calculated in the following way: at 500 hPa, 50N and for the  $\Delta Z$  index longitude, for each year of the 1980-1-999 period, the difference between the DJF averaged stationary eddy geopotential height of that year and its climatology computed from the NEUTRAL years (NEUTRAL years defined in Table 2)). This implies that the average of  $\Delta Z^*$  for the 4 warm ENSO years is the  $\Delta Z$  index used in Figure 3. In grey are the signatures for the 4 ENSO events used in Figure 3 and the two additional ones used in Figure 2. Figure A (top) shows that three events (1983, 1992, 1998) of the four strongest events lie at the up-right side of this scatter plot: For three of the 4 largest ENSO events, the temperature and heat flux are grouped together as the strongest ones. Figure A (top) therefore supports the fact that the heat flux is high for warm ENSO. Figure A (bottom) shows that there actually appears to be also a relationship between the January-February heat flux anomalies at 100 hPa and the  $\Delta Z^*$  index for the MRI simulation, and that it is in agreement with our expectation: ENSO years are characterized by large heat fluxes and  $\Delta Z^*$  indices (look at the grey signatures).

Going back to Figure 3, we remark that each model signature depicts the response to the strong ENSO events averaged together. In this sense, they describe the general response of each model to an averaged ENSO forcing. We thank the Reviewer for his/her question, because we now realize that this is why we should not necessarily expect a linear relationship between the  $\Delta Z$  and  $\Delta T$  indices (hence the regression lines

are removed Figures 3 and 6, and the text modified accordingly). Figure 3 can only serve to affirm that if the model simulations do not have a strong enough extra-tropical ENSO teleconnection pattern in the troposphere they are not supposed to have a response in the stratosphere. Conversely, for the majority of the model simulations that have a significant ENSO teleconnection pattern in the troposphere, the response in the stratosphere is shown to be positive (evidenced also by the histograms) but with a large spread. We therefore define a ‘good agreement’ between the models and ERA40 in the ENSO response the points lying in the upper-right quadrant of new Figure 3 (positive  $\Delta T$  index, larger than 1-standard deviation ERA40  $\Delta Z$  index). In particular, in new Figure 3 we show that only two models have a non-significant  $\Delta Z$  index (filled circles), indicating that the tropospheric teleconnection pattern for the observations and for the majority of the model simulations is robust and significant, whereas the response in the stratosphere is dominated by interannual variability.

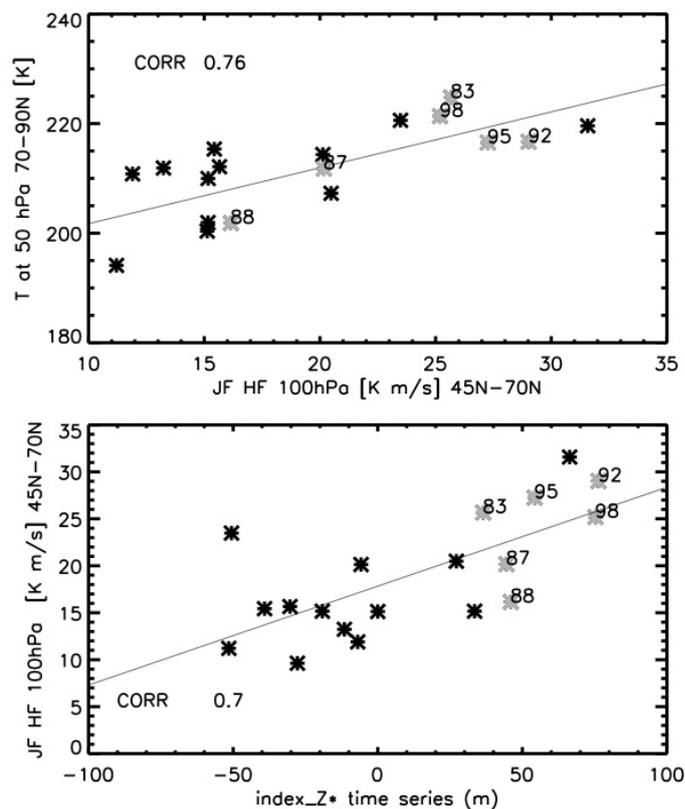


Figure A. Scatter plot of (top) the February-March temperature at 50 hPa averaged between 70°-90°N and the January-February eddy heat flux anomalies at 100 hPa averaged between 45°N-75°N; (bottom) the January-February heat flux anomaly at 100 hPa averaged between 45°N-75°N and the new  $\Delta Z^*$  index. Grey stars represent the ENSO years. Data from one simulation of the MRI model.

(2) Figure 5: The text has been rewritten in order to clarify that this figure shows the relationship existing between the north polar cap ENSO response in the temperature and column ozone fields in February-March, that is always expected to occur, not only during ENSO years. The motivation for this figure is (i) to investigate if the ENSO response in  $\Delta O_3$  and  $\Delta T$  for the model simulations is clustered in the upper-right quadrant, where the signature from the observations is; and (ii) to identify a cause of the spread of the modeled ENSO response. The fact that the expected relationship exists in Figure 5 demonstrates that the cause of the spread in the modeled responses is physical (as opposed to due to unphysical biases in the models), and, in particular, that it is due to internal variability. To corroborate this point, we have computed the ozone-temperature relationship between ERA40 (polar cap temperature at 50 hPa, March) and NIWA (polar cap column ozone, March) from the pool of all the individual years, 1980-1999 period. We find that the slope of this ERA40-NIWA relationship (characterizing interannual variability) is 6 DU/K (it reduces to 5.4 DU/K if we exclude the anomalously cold year 1997), in agreement with what we find in Figure 5.

Minor comments:

1. Reference added.
2. The reviewer is right, we meant that: “the ozone anomalies found in spring are directly related to the anomalous ozone buildup during the previous winter and not to an accumulation from previous years (see also Fioletov and Shepherd, 2003)”. The sentence has been rephrased.
3. Corrected.
4. Model and observations are in agreement within the uncertainties (clarified in the text). Please note that the mean of all model simulations is in large part removing the internal variability whereas the SSU and ERA40 are only one single realization of 4 cases, hence interannual variability is still present to a larger degree in the observed ENSO signature (‘bearing in mind the internal stratospheric variability’). With “qualitatively consistent” we meant a positive relationship for both simulations and observation, as shown.
5. Thank you. The new discussion of the  $\Delta Z$  now emphasizes this point.

6. This section has been rewritten. Thanks to the Reviewer for this comment, we have refined the interpretation of the results associated with the  $\Delta Z$  index, and removed the text on the linear relationship.

7. This reference has not been added here because Garny et al. (2009) is mainly concerned about the differences in long-term changes between their runs with modeled and prescribed SSTs and section 5.3 of Garny et al. (2009) ends by concluding that differences in ENSO between the two SST time series does not lead to a discernible influence on the long-term changes.

8. Done

All typographical errors have been corrected, thank you.

## **Response to Referee 2**

### Major comments:

(1) The Reviewer is right. However, by substituting the original N-members timeseries with their ensemble mean, the nature of the original timeseries is changed, because in the ensemble mean time series the influence of the internal variability is reduced (and this would be clearly so for the 9 SOCOL simulations). This is indeed why we have chosen not to average the SOCOL and MRI simulations in the majority of the Figures (except for Figure 2 left, the multiple realizations have been averaged just for helping to read the figure). We agree that it is not straightforward to consider together results from models with one single realization and from models with multiple realizations. One possibility is in fact to randomly select 1 or few realizations, however in this case we would have lost information coming from the availability of the multiple realizations. Moreover, following our approach we can attempt to distinguish the role of internal variability and model biases, by comparing intra- and inter-model spreads (as we did, in the interpretation of the Figures, where appropriate). We have therefore retained our approach.

2. Thank you for this comment. Unfortunately, heat fluxes and/or EP fluxes are available only for too few models. See also the response to Referee 1, major point (1).

3. Figure 5. Please see response to Referees 2, major point (2). Taken into account.

### Minor comments

1. Thank you. Sure, we cannot discriminate the spread of the responses in terms of simulation design. Text modified.

2. The comment about cold ENSO refers to previous works. The purpose of this manuscript is to base the analysis on known ENSO signals in the stratosphere. Therefore, we have restricted our analysis to warm ENSO events.

3. We refer again to table 6, Eyring et al. (2006).

4. A relationship between sudden stratospheric warmings and ENSO has been addressed before (for example, Taguchi and Hartmann (2006) and Cagnazzo and Manzini 2009). Here we did not look at this aspect, beyond the scope of the manuscript. Yes, we agree with the reviewer about the cooling response.

5. The SST index (NCEP/CPC) for the selected 4 strong cases is about 2 STD of the SST anomalies, whereas the 2 additional cases are about 1 STD (<http://www.cpc.noaa.gov/data/indices/>). Figures 1 and 2 are therefore consistent with the fact that using the index based on the 4 strong cases, the response is stronger than using the index based on the 6 cases.

6. Rewritten.

7. The negligible contribution of the dynamically-induced chemical effect is due to the fact that ENSO years are anomalously warm at polar latitudes.

8. See response to Referee 1, major point (2).

All typographical errors have been corrected, thank you.