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Interactive Comment

Interactive comment on "The 1986–1989 ENSO cycle in a chemical climate model" *by* S. Brönnimann et al.

S. Brönnimann et al.

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Reply to referee 1

In a general comment, referee 1 states that the model simulations come across as rather arbitrarily chosen and that the motivation and justification for using a GCM and a CCM is not clear. We have tried to improve this part of the manuscript. It is only partly true that we used the runs "because they were there". We selected the El Niño and La Niña events based purely on scientific criteria. The goal was to choose events that were most similar to what (according to the literature) can be called "classical" ENSO events. This is the case for the 1987 to 1989 cycle. For instance, choosing the 1983 or 1992 El Niño events, it would be very difficult to separate ENSO and volcanic effects.

The CCM SOCOL is our main tool. Having selected the two ENSO events, we per-

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formed the S1 and S2 ensemble simulations (they were not “already there”). The focus of our paper clearly is on stratosphere-troposphere coupling and stratospheric ozone, hence, the CCM is the tool of choice. However, addressing stratosphere-troposphere coupling requires a well-reproduced tropospheric signal. The reason for using MRF9 is to show that the chosen cases (1987 and 1989) are well reproduced by a standard GCM (other GCMs also reproduced it; see references in the text), and not only by SOCOL. This is important with respect to the robustness of the tropospheric signal.

In the revised manuscript, we tone down the role of MRF9, but make our motivation clear. The focus is clearly on chemical climate modelling.

1. The observed features refer to the difference between 1987 and 1989 and not to either one of them. This sentence is changed.

2. The statement that the polar vortex is well reproduced refers to geopotential height, whereas the next statement refers to temperature. Temperature is much more variable than geopotential height (e.g., major warmings produce a temperature increase of 40°C within days), hence the two statements are not contradictory. We have omitted the sentence from the abstract, as suggested by the reviewer.

3. We have changed the sentence.

4. The ensemble simulations S1 were performed solely for this paper with a standard model set-up that was also used in another project (see references in the text). While analysing the results, modifications had been made to the model set-up in the context of other projects. We decided to also perform an ensemble (S2) with the new model set-up to allow more comparisons and perhaps a more robust result (also, this gave us the chance to write out 12-hourly instead of monthly mean ozone fields). Again, the S2 runs were solely performed for this project. It was planned to more specifically address the differences between S1 and S2 in the paper, but we found that the differences are insignificant and can not easily be attributed. In the revised manuscript, we add some more information (see below). Only for the MRF9 simulations it is true that they were

“already there”. Still, we found it important to compare the results to another model (see reply to general comment). It is not the “lack of something” that motivates the comparison between MRF9 and SOCOL, but rather the robustness of the tropospheric response between different models and model set-ups.

In the revised paper we address the motivation more clearly and also make clear what was already there and what was done specifically for this study. Also, we are more specific with respect to the reasoning behind the model set-up. The set-ups are described in more detail, including the question of detrending. In addition, we address the differences and similarities between S1 and S2 more specifically, concluding that there are differences, but overall the close agreement can be interpreted in terms of robustness of the modelled signal.

5. The MRF9 simulations were already there and hence we could not choose the starting date. However, 45-member ensemble simulations were available for different starting dates (1 November, 1 December, and 1 January). We analysed all of them and found no large difference in the results, but present only those starting on 1 November (we would have preferred runs that start on 1 September, but in this case data availability was the major factor). In the revised manuscript, these other results are briefly mentioned. We were surprised how well a model with an upper lid at 50 hPa is able to reproduce the ENSO response up to (for GPH) 100 hPa. However, this is not the focus of the paper. In general, MRF9 does not allow us to analyse the stratosphere (e.g., the Brewer-Dobson circulation). For the SOCOL simulations we chose September as the starting date. It is important that the spin-up of the model includes the formation phase of the stratospheric polar vortex. The map projection is different for precipitation because some of the features are of a smaller spatial scale than, e.g., in the SLP field and because absolute anomalies are stronger over the tropical oceans. Both together would make it difficult for the reader to focus on the signal in Europe.

6. Even though the point was over land, it is true that the grid point is very close to the ocean where SSTs are prescribed. In the revised paper we use an additional grid point

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(Moscow) that is more continental. In fact, results show a higher variability for MRF9, which could be due to this fact. We still keep the grid point over Sweden because it represents the location of the maximum signal.

7. It is true that S1 and S2 are very similar in the troposphere. Nevertheless, we think that it would not be sound to lump them together for statistical purposes, as we know that they stem from different distributions. For instance, in the plot with the zonal mean wind, the fact that S2 has a prescribed QBO and S1 not makes them incomparable in the tropical stratosphere. Also, in an absolute sense, ozone values are different in S1 than in S2 (this is addressed in the revised manuscript). We have added some clarification on this in the text.

In the case where we did lump S1 and S2 (Fig. 3), we now distinguish between S1 and S2 by using different hatchings in the histogram in the revised manuscript.

8. The point is defined as 3.8E, 87.2N.

9. We agree that showing streamfunctions is a good tool and have added a new figure that shows the zonal mean meridional mass streamfunction (based on the residual wind). Here we show the whole globe (90S to 90N) and focus on the whole stratosphere (100 hPa to 1 hPa), as suggested by the reviewer. We still keep the EP flux figure as it is because some of the conclusions directly relate to EP flux divergence. This is the case for the comparison with the two dipole patterns described by Chen et al.. One of these dipole patterns is defined in the troposphere, hence it is important to show both troposphere and stratosphere.

10. The difference in ozone between TOMS/CATO and SAGE is due to a regridding and sampling problem (the high latitudes had only SAGE data for March, but CATO data for all months). We reprocessed the data and only kept latitudes south of 50N where we have coverage for all months. The agreement between the plots is now much better. We have changed the manuscript accordingly.

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