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Interactive comment on "Long-term changes and variability in a transient simulation with a chemistry-climate model employing realistic forcing" *by* M. Dameris et al.

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First of all we would like to thank the three referees for their sympathetic and helpful comments. We will consider all annotations when creating the revised manuscript. We believe that the paper will strongly benefit from their comments. Thanks also for a lot of useful suggestions for minor changes.

In the following we will briefly address the most important remarks and suggestions given by the reviewers.

It was proposed by Ref #1 to give some clearer statements about the prescribed changes of emissions used to drive the troposphere. We have added some text with

regard to this point, in particular we complete the description of the applied chemistry module, which does not include NMHC chemistry. Additionally, we discuss the effects of this neglect, especially that this can cause some underestimation of tropospheric ozone changes. But we have also mention that this should not affect the conclusions of this paper significantly.

We have tried to clarify the possibilities and the limitations to estimate the deterministic behaviour of the model system with the used approach. Due to the fact that we only investigate one model simulation and not an ensemble, this restriction obviously has an influence on our conclusions. Nevertheless, the conspicuous similarities between the model results and observations provide indications of individual cause and effect relationships which we think are worthwhile to be pointed out. Certainly, based on this investigation only, we are far away of having evidences for a deterministic response of the model system due to the prescribed forcing. We hope that the additional text passages in our manuscript express this point clearly enough to meet the critical remarks of Refs #1 and #2 in an appropriate way.

When writing our first manuscript for ACPD, we were aware that the section of the response to the solar cycle is based on rather simple diagnostics. We agree with the comments of Refs #1 and #3 that a deeper analysis is required for reliable conclusions. The point we would like to make in this paper is that, similar to the observed evidence published in WMO 2002, there is an interesting similarity between solar cycle and the behaviour of the ozone layer. Higher ozone values are found around solar maxima and lower values during solar minima. The model results do not only confirm the results presented in WMO, i.e., for the years after 1978, it also show a corresponding behaviour in the years before. To consider the remarks of the referees, we add some text on it, and we also announce that we are working on a separate paper especially dealing with this topic. This will also contain a comprehensive discussion about the possible restrictions caused by the upper boundary of the model system. Nevertheless, we add a “cautionary note” as requested by Ref #3, that their might be some restrictions

ACPD

5, S1356–S1358, 2005

due to the low upper boundary of E39/C.

The paragraph describing the use of pre-calculated heating rates in response to volcanic aerosols has been extended. We agree with Refs #1 and #3 that it is rather unsatisfactory not to discuss the reasons for calculating the heating rates directly in our model simulation. Therefore, we put together the arguments for doing it in such a way. Certainly, this is not a proper and ideal strategy, but for our (first) transient model application this was the only feasible way. We know this and hopefully this can be improved in up-coming simulations.

The reviewer #1 got it right that we do not consider geographically different changes of NOx emissions during the model simulation. At the time of starting the model run we did not have reliable estimates available which take into account different developments in different countries. For the whole period (1960-1999) we got a mean increase of 2.6%/year (not 1.6%/year as mentioned in the original manuscript, typo!): In the transient simulation, we assume a piecewise constant rate of 2.5%/year for 1960-1979, 1.5%/year for 1980-1989, and 3.9%/year for 1990-1999, to represent a slowdown of emissions in the 1980s in Europe and a stronger increase in Asia during the 1990s. This information has been added in the revised manuscript. In the meantime we have access to more realistic data sets which can/will be used in forthcoming simulations.

Since our model E39/C has its upper boundary centred at 10 hPa, the investigations and the discussion of results mainly concentrate on the UTLS region. As we did in previous papers (e.g., Hein et al., 2001; Schnadt et al., 2002), we always try to identify the limitations, e.g., by comparing our results with respective outcome of middle atmosphere CCMs like MA-ECHAM/CHEM. Nevertheless, following the suggestion of Ref #2, we add some notes on this topic, especially with regards to the effects driven by the QBO and solar activity.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2297, 2005.