

Interactive comment on “The atmospheric chemistry general circulation model ECHAM5/MESSy1: consistent simulation of ozone from the surface to the mesosphere” by P. Jöckel et al.

Anonymous Referee #1

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General Comments

This manuscript presents simulations of the troposphere-stratosphere from a newly developed model ("ECHAM5/MESSy"). This is a modular model and includes representation of both tropospheric and stratospheric chemistry. The manuscript includes some important and interesting results, and the model appears to in general perform well. However, I think the manuscript is too long, has far too many plots, and is not well focused. I think material is worth publishing but needs major revisions before it is suitable for publication. I think the authors should think seriously about splitting the

manuscript into 2.

Specific Comments

1. The paper aims to examine meteorology, transport, and chemistry in both the troposphere and stratosphere of 2 simulations, and also presents the modular nature of the model. In all cases each issue is only touched on and not examined in detail (see some further comments below). I think the manuscript needs to be focused and go into more detail of different aspects. This will I think require the paper to be split into 2 papers.

2. The abstract and Sections 1 and 2 make a big deal about the modular capabilities of MESSy, but this isn't actually discussed in any detail in the remainder of the paper. I don't see anything in the paper that really takes advantage of the modular aspect. I can see the reasons for developing a modular model, but this is not presented in this paper. In current manuscript the discussion of each module in the model just takes space and makes it more difficult to determine what was actually done in the runs presented.

3. The paper includes 32 figures with a total of 170 individual plots. This is far too many. Even if there is no page limit, this is too many figures for any reader to take in. The authors need to decide what are the key points, and to choose the few plots that show these points. Also, does each site need to be shown when a comparison is made with ozonesonde data?

4. The paper jumps between topics, and I think it would be better to re-order some of the sections. Why is there a section on ozone distribution and budget before the discussion of tropospheric and stratospheric tracers and chemistry? It would make more sense to move section 4 to after section 6. Actually is this section needed? The paper is long and this discussion does not really help the assessment of the model.

The discussion of 2002 SH warming is out of place. The figures show chemical species and there is little discussion of the meteorology (even though it is in the meteorology

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subsection).

5. The discussion of simulations S1 and S2 is very fragmented. Also, S2 is really a sequence of 3 simulations with different changes and it is not always clear which part of S2 is shown (without looking back to section 2). There are only a couple of places where S1 and S2 are directly compared, and most of the time S1 is used for one period and then S2 for another. Also, it is often stated that S2 is better than S1, but this isn't clearly shown. I think there needs to be explicit comparisons of S1 and S2 (maybe in separate section) to show what the impact of changes between S1 and S2 is, and whether the better agreement is because of reduced nudging in stratosphere or changes in gravity wave forcing (or both).

6. On pg 6997 it is stated that "first time ... reproduces the steep ozone gradients across the tropopause, both in the tropics and extra-tropics, without prescribed boundary conditions." Where is this discussed in the paper? I didn't see a big detail made of this in the main body of the paper and was surprised to see it as one of major conclusions. This good agreement is probably shown in the numerous small ozone profile plots but I don't think it was highlighted. Also, what is meant by "without prescribed boundary conditions"; Aren't boundary conditions prescribed in your simulations?

Minor Comments

1. The use of relaxation in the troposphere rather than just running in totally free mode needs to be justified in the beginning of the paper. As I read the paper I wondered why the authors had made this discussion, and it wasn't until the final section that this is justified (line 1-3 on pg 6994). This justification should be in the introduction.

2. The emissions and boundary conditions used need to be more clearly presented. This information is in the discussion of OFFLEM and TNUDGE modules, but should be in discussion of simulations performed (pg 6967-6968).

3. The sentences on pg 6968 discussing where simulations were performed and

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amount of CPU should be a separate paragraph.

4. pg 6975, line 13-15: Where is it shown that underestimate of transport causes an underestimate of reactive halogens and ozone depletion? This is important and needs to be shown or a reference to another study.

5. On pg 6977 it is implied that ozone for full 8 year period is shown in figure 10, but only a 4 yr average is shown.

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