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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CHEM2D-OPP: A new linearized gas-phase ozone photochemistry parameterization for high-altitude NWP and climate models" *by* J. P. McCormack et al.

Anonymous Referee #1

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General comments:

The manuscript describes the ozone photochemistry parameterization for applications in climate and numerical weather prediction models. The development and validation of simplified chemistry codes is relevant to the scope of ACP. Although linearized ozone chemistry approach is not novel, the authors describe this approach with a great deal of details not readily available from the previously available publications. They also made an attempt to evaluate and compare the performance of several parameterizations using sophisticated 3-D NRL model. The description of the experimental set-up is clear and can be reproduced by other scientists. The applied methods and assumptions

have been clearly presented in the paper. The manuscript is well written and structured. The authors give proper credits to related publications. The authors concluded that with the proposed ozone chemistry parameterization the model is able to simulate realistic distribution of ozone during short-term (6 days) and 1-year long runs. They also claimed that despite of some problems the performance of their parameterization is superior in comparison with other applied schemes. However, I think that the results are not sufficient to support the interpretations and conclusions, therefore the paper can be recommended for publication only after revisions.

Specific comments:

1. The evaluation of the model performance is based on the comparison of the simulated ozone distribution with satellite observations. The authors implicitly postulated that in the case when more detailed chemistry treatment is applied the agreement between simulated and observed ozone would be perfect. However, the deviation of the simulated ozone fields from the observed substantially depends on the model dynamics and accuracy of the transport code. It well could be that due to deviation of the simulated meteorological fields from the reality and some inaccuracy of the transport code the performance of the model with detailed chemistry scheme will be worse than with parameterized ozone chemistry. Therefore, I think that the author's evaluation of the parameterized schemes is not fully convincing. I would propose to carry out yet another experiment using the model with the chemical code from their 2-D model. The comparison of the ozone fields simulated with parameterized ozone chemistry with this "reference" case will provide more solid basis for the conclusions.

2. Another missing aspect is how good the model simulate non-zonal features of the ozone field. The proposed linearized ozone chemistry is based on the zonal and monthly mean ozone destruction rates, therefore the simulation of non-zonal features would be the weakest part of the model, because in the 3-D case (for example during boreal winter-spring over the northern middle-high latitudes) the distribution of ozone destroying radicals could be not homogeneous along the latitude. Would it be possible

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to proof or reject this by comparison with the observation data?

3. The authors carried out five identical hindcast experiments with NOGAPS-ALPHA model. Taking into account model non-linearity, it is hard to say how robust are the presented results. Therefore, it would be important to show that the obtained results are statistically significant using the results of ensemble simulation at least for one chemical parameterization.

4. page 6631, "ozone production occurs primary"

This means that there are some secondary sources of the ozone. What other sources of the ozone have been included in the model?

5. page 6632 - “assumption”

This assumption is not completely correct. Even if Cly is constant in time the ozone destruction rate could be different depending on the ratio between different members of chlorine family. For example, if HCl dominates then the ozone destruction is rather small, but when all chlorines are in active form then the ozone destruction rate will be higher. I guess, it could happened not only as a result of heterogeneous chemistry (which is not considered in the manuscript), but also when the air mass from vortex area crosses the terminator.

6. page 6639 - “The values are small”

It is not clear why 3 ppmv/month is small. I think it is comparable with the ozone mixing ratio (see Figure 7).

7. page 6640 - “repeats the calculation”

The description of the procedure needs a little bit more explanations. The authors said that 2-D model is fully interactive. Therefore, I do not quite understand how the perturbations in the temperature and overlying ozone column were introduced. Did the authors repeat all calculations with full model with changed reaction coefficients or

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some other approach was used?

8. page 6642 - “The column term”

More explanations are necessary to understand why the column ozone is affecting only oxygen photolysis. The production of atomic oxygen (both O3P and O1D) also significant for the ozone balance. For example, any enhancement of local O1D production (i.e., decrease of the overhead column ozone) leads to NOx and HOx increase followed by the ozone decrease. Therefore, it would be interesting what is possible contribution of these processes.

9. page 6642 - “radiative heating is updated every 2 h” In GCMs I know the radiative heating is updated every time step, while the full radiation calculations are carried out less frequently. Is it really not like this in NOGAPS?

10. page 6646 - “time of the day”

It is not clear how to interpolate monthly mean parameters to the specified time of day. Probably the use of the monthly mean values for the night time is not very accurate, especially in the upper stratosphere.

11. page 6650 - “range from 215-0.46”

It was mentioned that MLS data represent 215-0.46 hPa range, but in Figures 12 the MLS data are shown for much wider area (1000-0.1 hPa). Some explanations are necessary.

12. page 6658 - about application for climate As far as I understand the parameterization is based on the suggestion that ozone destroying radicals are close to the basic state of 2-D model. For the long-term runs the chlorine loading (for example) will not be constant. Therefore, it is not clear how the application of linearized ozone chemistry for long-term runs can be justified.

13. Conclusions

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I would advise to work more on the conclusions. At the moment it contains many repetitions of the already discussed results. With my point of view, too much attention was also paid to the future plans. I think, it would be enough to enlist the new elements, which the authors intend to implement in the nearest future and move the description of new approaches to the future papers.

Technical corrections:

14. page 6646 There are two W.A. Lahoz in the authors list. Is it correct?

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