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Interactive
Comment

Interactive comment on "Upgrading photolysis in the p-TOMCAT CTM: model validation and assessment of the role of clouds" *by* A. Voulgarakis et al.

A. Voulgarakis et al.

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We thank the 1st referee for his useful review. Below we respond to the specific comments in the order given:

General comment: 2-D offline-calculated photolysis rates is what our model is currently using. The results of this paper demonstrate how the model performance changes when moving to a state-of-the-art scheme. And how important or not important clouds can be when evaluating a CTM. Is there an effect when changing cloud handlings in a model, and if yes, is it what we expected based on previous knowledge?

The main disadvantage of the standard scheme, when it comes to variability, has to do with the treatment of clouds. Off-line photolysis rates were calculated using temporally

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and zonally averaged cloudiness. In a study like ours, except from the demonstration of the improvements in the model, we believe that the comparison between OLD and RAN provides information on how cloud variability is important when simulating chemistry. The approach resembles the one taken by Tie et al. (2003) for Fig. 20, with the difference that now we look at cases/schemes with different cloud variability, and not at clouds vs no-clouds comparisons in the same model.

These are questions which would not have been answered, had we focused on the differences between a two-stream scheme and Fast-JX. Such a study would also be very useful, but would require a totally different approach and model set-up.

To underline that the scheme against which Fast-JX is being compared is the one currently in use, we replace "old" when referring to it with "standard". In the same way, OLD is replaced by STD in plots and on tables. Also, we do not refer to the scheme as "the two-stream code", as the two-stream nature of the scheme is not exactly what we focus on in this paper. It is the cloud variability, how it is represented by different codes and how it affects composition that we are focusing on.

Specific comments:

Page 347 (line 21): See previous paragraphs.

Page 347 (line 28): We agree that this is not a full validation of our CTM and this is not actually the purpose of this study. A more thorough validation paper is among the aims of the authors for the future. In the meantime, and since there has been no other manuscript documenting some new developments in p-TOMCAT (including the implementation of Fast-JX), we think that this brief evaluation will be a good reference for other studies using this version of the model. We change the title of the manuscript from "...Model validation..." to "...Model evaluation..." in order for the exact subject of this study to be more accurately reflected. This change has also been applied to the text.

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Page 351 (line 12): The sentence was confusing and has been removed.

Section 3.1: We repeated the calculation for exactly the same conditions as was done in Liu et al. (2006): overhead sun, 300 DU overhead ozone column, equatorial atmospheric conditions, surface albedo equal to 0.1, cloud liquid water content equal to 0.1 g/m^3 and cloud fraction equal to 0.50. The clear-sky values now are very similar to the ones calculated in Liu et al. (2006). This better comparison is attributed to the use of an identical overhead ozone column. Currently, any differences in the simulated effect of clouds between the two models, arise either from the fact that we are using RAN while Liu et al. (2006) used MRAN, or because we use a slightly different representation of the effective radius (see Section 2.3). The agreement in general is very good. We modified the text related to this figure in order to reflect the changes made.

Section 3.2: The main point of these figures is the comparison between Fast-JX and the observations. The comparison to the standard scheme is included to demonstrate how the new treatment of photolysis (and effectively clouds) has improved our calculations. The standard scheme reads-in offline photolysis rates which have been calculated using seasonally and zonally averaged cloud fields. This has been made clearer in this section by adding the word “offline” to the sentence reminding the reader what the standard scheme is doing.

Section 4.1, Page 357 (line 20): It should write “in the lower part of Table 1” instead of the reference to panel c. That was an error also pointed out by the 2nd referee and has been corrected. The clouds in AVG are only seasonally averaged, not zonally. This configuration and the comparison to the other two runs, help examine what is more important for capturing variability in photolysis rates/tracer concentrations: temporal or spatial variability of clouds. We make clearer in the text that clouds are only seasonally averaged in AVG. Please see the first paragraph of this response message for the explanation of why we chose to perform the experiments this way. What we are mainly interested in is variability.

Section 4.2: We have replied to the other related comment (the one on Page 347). This study is not a detailed validation of the model. It uses the same dataset that was used for the validation performed for the RETRO project and shows examples of how a better representation of clouds can improve tracer concentration variability as captured in the model. It already includes data from a variety of stations around the globe. We think that an extension of the study with the use of datasets beyond the ones used for the RETRO project would be a useful addition, perhaps in a future work, focusing on a thorough validation of the model.

Mace Head CO: We agree that the results for Mace Head CO reveal important weaknesses of the simulation. However, as we state in the text, we believe that the reason why the simulation of northern hemispheric CO remains too low is the unrealistically low anthropogenic emissions, a fact also pointed-out by other studies. This is true for almost all stations examined in the northern hemisphere. On the other hand, as outlined in Table 2, the seasonal cycle is better captured when using Fast-JX for almost all the stations in the Northern Hemisphere. We agree that the sentence referring to European outflow is misleading and we remove it. The general comment is that finally it seems more likely that the hemispheric underestimate of anthropogenic CO emissions is much more important than any regional influence.

Any improvements in budgets?: Yes, the tropospheric ozone burden is now 298 Tg, which is an improved value in respect to the ACCENT average when compared to what it was in p-TOCAT at the time of that study (247 Tg). The full methane lifetime is now around 7 years, which is close to the low end of the ACCENT range (it used to be an outlier with a too high lifetime - 12.5 yrs). A detailed analysis on the effect of clouds on the global and regional ozone budgets is presented in a paper of ours which has been recently submitted for review.

We expect that the inclusion of aerosols in the radiative transfer (and thus photolysis) calculations and also the use of more realistic CO emissions, could increase the methane lifetime in the model and bring it even closer to the values estimated from

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average present-day models.

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