

Interactive comment on “Improvements to stratospheric chemistry scheme in the UM-UKCA (v10.7) model: solar cycle and heterogeneous reactions” by Fraser Dennison et al.

Anonymous Referee #1

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This paper presents improvements made to the stratospheric chemistry part of the UM-UKCA chemistry-climate model. First, the photolysis scheme is improved by accounting for the 11-year solar cycle variability. Second, the heterogeneous chemistry scheme is improved by including or updating reactive uptake coefficients on sulphuric acid aerosols and polar stratospheric clouds. The effects on total column ozone of these model improvements are assessed with a focus on the Antarctic. Some of the model ozone biases are found to be reduced by these improvements. Overall, the results are interesting and helpful to stratospheric ozone modellers. The paper is well written and clear. Therefore, I recommend publication. However, the authors might wish to consider and even take on board some of the questions, comments and sug-

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gestions listed below.

* General comments

- It is difficult to understand why the photolysis and heterogeneous chemistry simulations have different set-ups. In the case of heterogeneous chemistry simulations: l9 p8: ‘In order to examine only the direct effect of the modifications, feedbacks between the chemistry (ozone, methane, nitrous oxide and water vapour) and radiation have been switched off for these runs (i.e. these runs are identical in terms of their meteorological variables).’ That is a very good set-up to check the effects of changes in the chemistry scheme without dynamical feedbacks. Then the authors do additional runs with fully coupled chemistry-dynamics (i.e. model-calculated fields of ozone, methane, nitrous oxide and water vapour used in the radiation scheme) to see the full effects on ozone. That is a very sensible approach. In contrast, in the photolysis simulation, only short (only 15 years for 11-year solar cycles) fully coupled simulations are run. As a result, the analysis and interpretation are difficult because of the dynamical variability (differences in temperatures and winds between the 2 runs). The authors are well aware of these limitations: l1, p5: ‘We only plot the results for the 60S–60N region here because the variability in ozone is quite large in the polar regions, masking the impact of solar variability.’ or l14, p5: ‘Figure 2 suggests there may be some phase shift between the solar cycle and its effect on stratospheric ozone. Although, the model runs presented here are too short to verify’. The authors cannot conclude with this model set-up. I would suggest to run 2 additional simulations without the dynamical feedbacks (i. ‘runs are identical in terms of their meteorological variables’). It means using up computing resources but the runs are short (15 years) and this would really help the analysis.

- The heterogeneous chemistry part contains a nice latitude and altitude-resolved analysis. I don’t know why the photolysis part is so short with no latitude or altitude-resolved analysis. Again, I would suggest to follow not only the heterogeneous chemistry set-up but also the structure of the analysis. I am sure that readers would be interested by seeing the 11-year solar cycle signal in the global zonal mean ozone distribution.

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- I would suggest to move subsections 3.1 and 4.1 into the very short section 2 which is devoted to model description. sections 3 and 4 should focus on presenting and discussing the results. This would facilitate the flow of the reading.

* Specific comments

l7, p2: Not correct. Solar radiation also varies on solar rotational timescales (about 27 days) with amplitude just slightly lower than the 11-year cycles. It also varies on longer time-scales.

l10, p2: not only early 2000s. Chipperfield et al, Nature, 2017 study (along with the supplementary information) pointed out the effects of the 11-year solar variability (not only about the early 2000s but also about the mid-2010s) on ozone trends.

l19, p2: constellations? configurations

l14, p3: "...using a combination of the FAST-JX scheme (Wild et al., 2000; Bian and Prather, 2002; Neu et al., 2007) and look-up tables." In terms of computing costs, it does not seem to be very efficient to combine a look-up table and an on-line photolysis model. Why not use either a look up table or Fast-J?

Figure 2, p5: solar variability does not simply impact the ozone production (molecular oxygen photolysis), it also impact ozone destruction via the change in Ox partitioning (notably the photolysis of O₃ to atomic oxygen which is a key reactant in the ozone-destroying catalytic cycles. I would suggest to show also the O₃-to-O photolysis.

Figure 2 caption: 'the flux of the oxygen photolysis'. Do you mean the solar flux below 240 nm, the solar flux below 240 nm weighted by the O₂ cross sections, or O₂ photolysis rate ?

l18-20, p6: 'It may be possible to extend our treatment of solar variability to the UM radiation scheme although it is unlikely to have a large effect given the smaller amplitude of the solar cycle at the relevant wavelengths.' for the temperature, it is not at all small Swartz et al., ACP, 2012. I know that the direct heating continuation to ozone changes

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is much smaller but not negligible. It is better to provide a reference for this statement.

l14, p8: it is expected. remove 'as'.

l9, p9: no need to be so cautious. Figure 6 (nice way to compare CCM with T biases and observations) proves it. It clearly demonstrates that, at least over the 210-215 K interval the new model performs much better in terms of Antarctic ozone October climatology. An hypothetical correction of T biases would be equivalent to running a nudged version of the model and surely getting better results with the new version.

l2, p11 and so on: I suggest to add chemical when talking about flux in order to avoid confusion with other fluxes (e.g. dynamical).

Figure 8 caption: is it global mean?

l4-5, p13: unclear to me. The bromine heterogeneous reactions convert BrONO₂, HBr and HOBr into bromine radicals (Br and hence BrO). Why does BrO decrease and HBr increases?

l9-10, p13: Figure 5 shows the opposite. I think that the most likely explanation is that less O₃ means less heating and hence longer lasting vortex. It is a classic example of an ozone bias resulting in a temperature bias.

l11-13, p13: I suppose that this statement is not just a guess. A reference for it?

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