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Interactive comment on "Implementation of the Fast-JX Photolysis scheme into the UKCA component of the MetUM chemistry climate model" *by* P. J. Telford et al.

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We thank the anonymous reviewer for their comments. We provide more detailed responses below.

The manuscript by Telford et al. discusses the main features of atmospheric composition changes resulting from the use of an interactive photolysis scheme in a global chemistry-climate model, as opposed to using an ofïňĆine photolysis scheme. The analysis conïňĄrms results from previous studies, and adds new dimensions to such evaluation efforts by performing comparisons against satellite observations,

and by analysing the effect of using the interactive treatment on individual important photolysis reactions. The manuscript is well-structured and useful to the community, as it explores important interactions in the composition-climate system and explores thoroughly aspects of the behaviour of a widely used chemistry-climate model. It should be published following the minor revisions listed below.

Page 3218, Lines 4-5: Please change "chemistry climate" to "chemistry-climate".

This is changed here and when the phrase occurs elsewhere in the text.

Page 3218, Lines 17-18: Arguably photolysis is important pretty much everywhere.Perhaps rephrase to "especially in regions with large variability in cloud and constituent optical depths".

We agree that photolysis is important everywhere in sunlight. We intended to expresst that the interactive photolysis is important where there is variability in optical depths. We adopt the suggested phrasing to clarify this.

Page 3218, Lines 19-end of paragraph: This information seems a bit out of place here. It would ïňĄt better in the model description section. The introduction section should include a bit more information on the effect of different components of the climate system (clouds, aerosols, overhead ozone, surface reïňĆections) on photolysis, with some further references provided.

As suggested by the referee we add some details of how various constituents of the climate system affect photolysis. However given that this is a model description paper we feel that it is relevant to describe the model in the introduction and we retain this description.

Page 3219, Lines 3-4: It should be made clearer what an "idealised atmosphere" is in this case.

We clarify that this idealised atmosphere is the one used in the CCMVal PHOTOCOMP photolysis model intercomparison (Chipperfield et al., 2010).

Page 3219, Lines 9-12: Wouldn't it be reasonable to use a whole-atmosphere version of the model for this analysis? Please comment.

We agree that, in an ideal world, this would be desirable and, in parallel with this implementation of the interactive photolysis a whole atmospheric chemistry scheme is being developed. However model development and validation involves trade offs, and it is impossible to wait for everything to be finalised before validating each component. A description of a version of the whole atmosphere chemistry scheme with interactive photolysis is being prepared (Archibald et al., 2012), albeit without a detailed investigation of the photolysis.

Page 3219, Line 11-12: This sentence kind of raises the expectations that a short evaluation on how photolysis treatment improves stratospheric performance may follow in the manuscript. It should be made clear from here that the inïňĆuence of photolysis on stratospheric performance is not analysed in this study.

We do perform a run using the stratospheric scheme, demonstrating its ability to capture total ozone column and ozone profiles, so we disagree with the statement that there is no analysis of stratospheric performance. However we concede that this section is only extremely brief and modify the text to emphasise this.

Page 3220, Line 1: Please change "These" to "ERA-Interim". Page 3220, Line 4: Please remove the ïňĄrst "and".

Done

Page 3221, Line 2: Is the number given for lightning emissions in Tg(N) or Tg(NOx)?

As mentioned to the replies of the first referee, this is Tg(N), which has been added.

Page 3222, Line 1: Please change "two stream" to "two-stream".

Done

Page 3222, Line 8: Why "Apart from the stratospheric ozone"? Was stratospheric ozone inïňĆuencing photolysis in the old scheme? And if yes, how?

In the offline stratospheric scheme overhead ozone column was used to scale photolysis rates. More precisely the ratio of a climatological ozone column to the modeled ozone column was used to scale rates. In the offline tropospheric scheme there is no such input, though the offline stratospheric scheme is used in the upper troposphere. We alter the text to make this clear. One of the advantages of Fast-JX scheme is that it allows us to use the same scheme throughout most the atmosphere, with only a small addition for low wavelengths near the top of the atmosphere.

Page 3222, Lines 15-16: Suggested rephrasing: "Recently, interactive photolysis schemes have been developed that are fast enough to be incorporated into global models." Fast-J is also mentioned in the next sentence.

Done

Page 3222, Line 21: Please put a "," after "code".

Done

Page 3222, Line 25: Fast-JX does not exactly combine Fast-J and Fast-J2. Fast-J2 is an improved version of Fast-J (that is more suitable for whole-atmosphere models).

We rephrase this to avoid implying this.

Page 3223, Line 4: Please change “this will now be” to “thereafter”.

We have changed this to hereafter.

Page 3223, Lines 15-24: What quantum yields have been used?

Quantum yields have been taken from the same sources. We clarify that, apart from for ozone, what we call cross sections are actually the products of cross sections and quantum yields.

Page 3225, Lines 1-3: What are the sulphate ïňĄelds that have been used? Do they vary seasonally/interannually?

The sulphate field is taken from the model, as it includes the UK Met Office’s CLASSIC aerosol scheme (Bellouin et al., 2007). This varies seasonally and interannually, albeit with only climatological emissions from the Year 2000. We accept that this is not ideal and, as we mention in the Discussion, we are looking to couple theÂă MODE aerosol scheme (Mann et al., 2010) to fast-jx to improve on the existing situation.

Table A1: Suggested change in caption: “Photolysis reactions employed in MetUM Fast-JX and the source of their corresponding absorption cross sections.”

Done

Page 3225, Line 19: Please change “the photolysis code” to “the standalone photolysis code”.

Done

Page 3225, Line 25: Please change “TES satellite” to “TES satellite instrument” (also in other places in the text).

Done

Page 3226, Line 10: I would suggest renaming the standard scheme to “ofïňĆine” or “standard” rather than “climatological”. I see “climatological” as a better name for an interactive photolysis scheme using climatological ïňĄelds of clouds, aerosols etc.

We change this to offline to avoid this confusion. There is an element of online modification in the stratosphere, but only relating to ozone, so we feel the use of the term offline is clear.

Page 3227, Line 3: Both this paragraph and the next more or less start with “First”, which makes the ïňĆow a bit confusing.

Changed.

Fig. 1: Presumably the photolysis rates are on a logarithmic scale. This should be clear on the axis.

Done. The axis labels have been modified accordingly.

Page 3227, Line 17-19: What are the possible implications of the NO *photolysis discrepancy?*

NO is photolysed to N and O(3P). In the model the N is then either locked up into N_2O or N_2, or turned into NO_2. The removal of NO and creation of odd oxygen probably acts to increase ozone, which would affect heating rates and thus atmospheric circulation. However, as can be seen by the good agreement of the models total ozone column with observations, the overestimation of $j(NO)$ is not having a catastrophic effect.

Page 3227, Line 24: Please change “schemes” to “scheme” and add “standalone” before “photolysis”.

Done

Page 3228, Lines 5-9: This is not clear: Why is the better ? agreement reïňĆecting improvements in the ERA-Interim reanalysis? If I understand correctly, the improvement is in comparison to the results of nudging with ERA-40 versus the ERA-40 reanalysis itself.

We speculate that the improvements in the description of potential temperature, which are mainly in the stratosphere may be as a result of the improved stratosphere in the ERA-Interim analyses (Dee et al., 2011; Uppala et al., 2005). However, as we note in our replies to the first referee, much of the differences may relate to changes in the versions of the UM used. We alter the text to reflect that the changes in the nudging performance are not unalloyed improvement, with the introduction of a small bias in zonal wind, and note that the changes may well reflect model changes as much as those in the analyses, rather than suggesting that there is obvious improvement which is related solely to changes in the analyses.

Fig. 2: The fonts of the text in this ïňĄgure are a bit too small. This is true for several of the ïňĄgures.

We change the orientation of the pictures to increase their size, helping the captions be more readable.

Page 3228, Paragraph starting at Line 23: Other weaknesses should be mentioned as well, such as the poor comparison over the US/north Atlantic in DJF. Also, are there any known issues associated with the ERA-Interim data that can drive some of the main model discrepancies in clouds? Are there any features of the TES comparison shown later that can be explained by the cloud discrepancies seen here?

We agree that there are further discrepancies which we expand upon. We are unaware of any issues in the analyses that could drive the discrepancies, and we suspect that

the model parameterisation matters more than the nudging. A good illustration of this is given in Russo et al. (2011), where despite models being driven by the same re-analyses the cloud properties exhibited large differences. There are no obvious links between discrepancies in the optical depth and the TES observations, though, as noted in Voulgarakis et al. (2011), the causes of the correlations are complex, making it difficult to identify such links.

Page 3229, Line 4: Please change "is" to "likely is", as you have not demonstrated this.

We believe this is probably true, but concede that we provide no evidence for it, so make this change.

Table 2 is very useful and informative, but you should provide more information on how many ïňĆights were used. Perhaps all? If yes, provide date range or something equivalent.

All flights are used. We clarify this and add a date range in both the text and the table caption.

Page 3230, Line 24 and onwards (till end of paragraph): The difference in the HONO performance is interesting, and attributing it to using more up-to-date cross sections in the model (i.e. implying that the measurements are not very trustworthy) sounds valid. However, the explanation given for the fact that the ofïňĆine model also performed poorly is not very clear or conclusive. What does "old photolysis measurements" mean and how do we know that "much can be attributed to an overestimation of the stratospheric ozone column"?

The term 'old photolysis measurements' is vague as there is no clear documentation where the rates are taken from. However the rates are known to have not been updated in some time. As we note when discussing HONO, for some species there has been a considerable improvement in the understanding of their photolysis, which can explain some of the discrepancy. We do not believe that this can explain everything, most notably the 50% bias in jo1d as the measured photolysis rates of ozone have not changed by such a large factor in the recent past. Therefore there must be some other factor. As the low bias in offline photolysis rates is larger for species where the lower wavelengths are more important (e.g. the bias for jo1d is larger than the bias for jno2) we conclude this is probably a result of a high bias in the ozone climatology used to derive the offline photolysis rates. We expand the text to clarify this.

Page 3231, Line 11: Suggested rephrasing: "This is as expected as" to "This is explained by the fact that".

Done

Page 3231, Line 19: Does "average bias" refer to global?

The average refers to an average over all the flights. We clarify this in the text.

Page 3231, Line 20-23: You can change "However" to "Although" and remove "though" from later in the sentence.

Done

Also, the separation of "that of using all 18 wavelength bins" from the rest of the sentence using commas is not ideal, as it lengthens the sentence too much. Maybe use parentheses?

Agreed parentheses is the solution we used.

Page 3232, Line 11: Please change "those" to "that used". Page 3232, Line 12: "six hourly" -> "3-hourly". Page 3232, Line 14: Please remove one "we".

Done

Page 3232, Lines 15-16: Please brieïňĆy mention what this method involves and what it achieves.

OK. We have expanded a discussion on this section.

Page 3232, Lines 27-28: Please explicitly mention the model versions (past and present) that you are referring to, for the sake of accurate documentation.

Done

Page 3233, Line 1: Have the authors tried to make comparison maps for the upper and lower edges of the vertical region that they analyse (400hPa and 800hPa)? As these regions often (though not always) lie above and below clouds, correspondingly, the differences may be larger. Although the weighting of the datasets towards the TES averaging kernel may be smoothing out differences even in this case.

That might be interesting. However Voulgarakis et al. (2011) did investigate this and found only a small amount of sensitivity to the level chosen, which they attribute to STE rather than changes related to photolysis.

Table 3: Please state what tropopause has been used.

We use the combined isentropic-dynamical tropopause (Hoerling et al., 1993) which we clarify in the text.

Page 3233, Lines 16-17: I would not agree on this. The comparison between the ofïňĆine and the interactive scheme results is a clear demonstration of how interactive photolysis affects methane lifetime, since everything else is kept the same. Possibly in the model version used in Morgenstern et al. (2012) photolysis rates would be dramatically high had they used an ofïňĆine photolysis scheme.

We have checked with the authors of Morgenstern et al. (2012) and they indicate that this wasn't the case, with the methane lifetime being less than a year longer when the offline photolysis scheme was used. Interestingly in the whole atmosphere scheme used by Archibald et al. (2012), which is derived from the model set-ups used in this paper, a large increase in OH is also modeled when switching from the offline to the interactive photolysis schemes, albeit only in preliminary results. This suggests to us that there are other factors, apart from the photolysis, are driving the high bias in OH, and thus the low bias in methane lifetime, which is what we intended to suggest. We alter the text to clarify this.

Fig. 6: What does the standard deviation indicate?

This is the standard deviation of the values in the grid-pointsÂăused to calculate the average and is used to provide a measure of variability. We clarify this in the text.

Fig. 7: What is the frequency of the ozone CO data that have been used to calculate correlations? Please state in the caption.

There is no exact frequency, with, as we state in the text, the model being sampled at the time and location of the satellite observations, rather than taking global average fields and sampling them, as was done in previous studies such as Voulgarakis et al. (2011) or Zhang et al. (2006). This does imply a kind of sampling frequency corresponding to the time-step of the model (20 minutes) which we clarify in the text. However we omit them from the captions as, demonstrated by Voulgarakis et al. (2011) the results are insensitive to the exact period of sampling (i.e. quasi weekly averages produced similar results to three hourly averages) and thus this information is extraneous.

Page 3236, Line 2: Please add comma after "troposphere".

Done

Page 3236, Section starting at Line 24: In this section, or anywhere in the text, there is not much discussion/description of the overhead ozone column that was used for interactive photolysis calculations in the tropospheric chemistry model. What has been used, and how much do the authors trust it? Could stratospheric ozone features be driving tropospheric OH or ozone discrepancies?

We correct the omission of a description of how the ozone, oxygen and rayleigh scattering optical depths are calculated in the optical depths section. We also note in the model description that, above 30 hPa, where an upper boundary condition is imposed, the ozone field is taken from the Rosenlof climatology. We clarify that this is achieved by overwriting the model fields, therefore above 30 hPa the ozone field, and thus ozone column, is taken from the Rosenlof climatology. Below this level the field, and thus the ozone column, evolves according to the tropospheric chemistry scheme. The ozone column is not perfect, but not completely unrealistic. We believe that the good agreement of j(O1D) rates in the model with the observations indicate that the ozone optical depth verify this, though we do note that there are uncertainties in these measurements. However we did experiment with changing the ozone column, albeit in only a preliminary version of the model, and found that, to remove the biases in OH, the ozone column had to be increased to a level where the modeled photolysis rates were no longer compatible with the observations, even accounting for their uncertainty. We added something about this in the discussion.

Fig. 8: Please remove "of O3 column".

Done

Page 3237, Lines 1-4: Please clarify that the ozone biases are not caused by the use of interactive photolysis.

This can be seen by examining Table 3 (where Fast-JX reduces the tropospheric O3 burden) and Fig. 4 where a high bias can be seen for both photolysis schemes. We add this to the discussion.

Page 3237, Lines 15: Please remove “a”.

We have removed "a potential" after statements from the first referee.

Page 3237, Lines 19: You could perhaps suggest that for e.g. comparisons to aircraft data, aerosol layers and their effect on photolysis can be important (since temporal and spatial scales are relatively small).

That is true for individual points and for localised regions. We add this to the text.

Page 3237, Lines 23-25: Please state what you mean by “new techniques”.

OK this refers to the sampling along flight tracks and satellite orbits. We have clarified this to emphasise that this refers to new ways of sampling the model.

Page 3238, Lines 9: Please change the second “photolysis” to “scheme” or something equivalent, in order to avoid repetition. A comma before “the interactive” would also help.

Done

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