Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-525-AC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ultraviolet Radiation modelling using output from the Chemistry Climate Model Initiative" *by* Kévin Lamy et al.

Kévin Lamy et al.

kevin.lamy@univ-reunion.fr

Received and published: 17 September 2018

Response to the Referee's Comments

Kévin LAMY

September 17, 2018

We would like to thank the referee for his thorough review. The comments have been very beneficial. We hope that the newer version has improved the paper.

1 General comment

The influence of ozone, aerosols, surface reflectivity and clouds on UV in the past, present, and future were discussed in the 2015 Assessment of the effects of ozone depletion and climate change panel of UNEP (Bais et al., 2015) but this publication is not discussed. Bais, A. F., R. L. McKenzie, G. Bernhard, P. J. Aucamp, M. Ilyas, S. Madronich, and K. Tourpali (2015), Ozone depletion and climate change: impacts on UV radiation, Photochem. Photobiol. Sci., 14(1), 19-52. Results of that study are more recent and more relevant compared to those of Hegglin and Shepherd, 2009 and Bais et al., 2011, and should be discussed against the results of this study.

I have some doubts on using mean and median total ozone from all models to derive the UVI projections through the 21st century. By doing

this the inter model variability of the projected ozone is lost, and taking into account that UVI is not linearly related to ozone, the projected mean UVI should not equal the mean of the individual projections of UVI. The authors justify their decision to use the mean ozone on the results of Figure 3. However this figure shows annually averaged differences and does not provide any information on the spread of the seasonal differences as well as on the spread of the UVI projections by individual models. These two will determine the uncertainty of the projections and it would be essential to know how large thee uncertainties are. Unfortunately, if these assumptions are proven incorrect (or if they result into large uncertainties), then the whole discussion on the future evolution of the UVI projections will be questionable.

We fully agree that it would be very interesting to add informations about the seasonal variations and the spread of the model. However, given the results of the Figure 3, even if there is a seasonal variation and spread between models, we believe that these variations are relatively low and that the median information is of interest. Moreover, making the whole treatment for every model and every RCP and very sensitivity test requires a high computational cost that we unfortuntaley could not afford.

2 Specific comments

1. *P3, L21: Also in Bais et al. 2015*

Corrected.

2. *P4, L1: The 11-year solar cycle affects UV (especially UV-B) through changes in strato-spheric ozone, while the direct influence is almost negligible.*

As suggested by the referee, precision has been added on this point.

3. *P5, L6: It would be very informative to show in Table 1, or in a separate table, which of the input parameters are provided by each model and which models have participated in the different experiments used in this study*

Except for aerosols, most of the input parameters are provided by the models cited in Table 2, this is why we choose not to detail this information. Models which have participated in the different experiments are specified in Table 2.

4. *P6, L4: For which spectral region was the surface albedo provided by the models? Usually the models provide the broadband albedo which is much different than the UV albedo. Please explain how this was handled.*

We used the broadband albedo provided by the models, it is indeed different than the UV albedo. CCMI output were already made and additional diagnostic output were not possible.

5. *P6, L7: I don't think that taking the median of three numbers is representative for the most likely value of the parameter. Probably in this case the mean optical depth would be more representative.*

We agree that the mean value may be more representative in general but we took the median to avoid eventually local erroneous values. This comment has been added in the manuscript.

6. *P6, L16-17: The two references refer to absorption cross sections of ozone and not to solar spectra. Assuming that this is a typo, using in the RTM calculations absorption cross sections different than those used for the retrieval of total ozone (Paur and Bass 1985 for GB instruments) could introduce inconsistency in the results.*

Corrected.

7. *P6, L21: Please state which is this simplification, no matter if it is discussed in another section.*

Corrected.

8. *P6, L22: Could you provide an estimate of how large would be the effect on the UV calculations due to using zonally averaged profiles?*

The distribution of ozone is mainly zonal, and in particular the altitude of the maximum concentration or the maximum concentration has a zonal distribution. On the other hand, the vertical distribution of ozone (such as that of aerosols) has a very small effect on UVR (compared to other important parameters influencing UVR variability such as total ozone or AOD). The use of a zonal mean therefore introduces only a minor effect on UV calculations. It is reasonable to neglect it compared to the other uncertainties associated with the method.

9. *P6, L27-31: Which models are used in the experiments? This is related to my previous comment (P6, L6).*

The models used for each experiments are listed in Table 2.

10. *P7, L7: In the comparison with GB data, has the altitude of each station been taken into consideration in the TUV calculations? UVI at Mauna Loa is by far greater compared to UVI at the seas surface for the same latitude.*

TUV calculations are made at sea level. We thought that the altitude of theses stations was relatively small (between 8m for Barrows and 370m for Lauder). We made an error due to the NDACC file (.mku file) from the Mauna Loa station which report a 3m altitude station. It is apparently about 3km above sea level. This should explained part of the discrepancy between our modelling results and the stations measurements. A remark has been added in the manuscript on this subject.

11. *P7, L15: In the monthly climatology, were data of the 15th used only (as with satellite data) or the mean of the entire month? How missing data were handled?*

For the monthly climatology we used a 10 day average around the 15th. It is now specified in the manuscript.

12. *P7, L29-31: It is not clear which the satellite data are used. Is always the 10 day average around the 15th is used, or only when data on the 15th are missing? How the clear-sky satellite data are selected?*

It is always the 10 day average satellite data which are used. It is now specified in the manuscript.

13. *P7, L31: As it appears from the text, for the monthly GB climatology all available data were used, but for the satellite day only the measurement*

on the 15th (or the 10- day average). If this is true, the data used in Figure 1 are inconsistent and I do not understand how one can compare these datasets with the model results.

A 10 day average is used in both ground based and satellite data selection. It is now specified in the manuscript.

14. *P7, L32- P8, L10: Please check this section and make the discussion clearer. I suggest focusing the discussion on differences between model and GB data and not on differ- ences between GB and OMI because this is not the main subject of this paper. The changes reported in the abstract and the conclusions (-4 to 11all in this section. Furthermore it would be good to report in Table 3 the spread of the model differences to GB for each station (e.g. the standard deviation).*

15. *P8, L22: Averaging the UV index over the entire globe is not a good approach due to large latitudinal (and seasonal) differences.*

For this computation we computed the difference between months and then took the average. Nonetheless we fully agree that it is not a good approach due to latitudinal and seasonal differences. But in this case, we aim at having a first idea of the global difference of behaviour between the models. This part of the article is not intended to study the difference between the model+TUV and OMI outputs in detail, but rather to estimate the overall behaviour of the models towards OMI measurements in order to infer their homogeneity. The result, presented here, even global, is consistent with the publications comparing OMI and ground stations.

Specifications on the averaging process and on the limitation of this sensitivity analyses have been added.

16. *P8, L31-34: The results presented in Figure 3 are yearly averaged. However, the vari- ability on predicted ozone varies seasonally (see Dhomse et al., 2018) and differences in UVI may also have a seasonal effect, which now is suppressed. Moreover, the test is performed for a period (2000-2010) when inter-model ozone variations are smaller than later in the 21st century. As changes in the UVI are nonlinearly related to ozone, the assumption that total ozone evolution in the 21st century can be represented by the mean or median is not a safe choice. I suggest checking whether the seasonal behavior of the differences is still within the reported limits, particularly in the southern high latitudes.*

We fully agree with the referee on this point. We added a remark about theses concerns in the manuscript. It is not proposed here that the evolution of TOZ be represented by the mean or a median, but it is just suggested that averaging TOZ before or after the use of TUV does not make a significant difference, which allows a significant saving in computing time. It is true that a significant seasonal variability of total ozone coupled with the non-linearity of the relationship between UV and ozone (low non-linearity) could induce a bias, but this would only be problematic at high latitudes in the southern hemisphere, over a relatively short period. We can see that the most important differences on fig 3 are seen in this region of the globe. But this should not call into question the general method.

Unfortunately, for now we do not have the resources to investigate longitudinal and seasonal changes but it should be thoroughly analysed on the future study from AerChemMIP results along with detailed AOD effects.

17. *P9, L4: Moreover, Figure 3 shows the sensitivity due to averaging of only the total ozone. What is the uncertainty introduced by the averaging of the other input param- eters; the ozone and temperature profile, the surface reflectivity, the optical depth of aerosols?*

The referee is right to bring up this matter. Nonetheless, as Bais et al. (2015) noted, absorption by ozone is the dominant factor controlling levels of surface UV for clear skies and low aerosols conditions. Our study is made exclusively in clear sky conditions and for aerosols AerChemMIP results would be a better context to investigate the uncertainty associated with aerosols along with profile and surface reflectivity differences.

18. *P10, L17-28: The changes derived in (Bais et al., 2011) have been based on a different reference period (as noted in the text); therefore the results of this study are not directly comparable, particularly as in these early years ozone variations were quite large.*

We kept the discussion on Bais et al. 2011 results, while it is not fully comparable, Bais et al. (2011) study and ours are similar and should be discussed.

19. *P12, L31-25: Effects of AOD on UVI have large longitudinal variability (see Bais et al., 2105) which is suppressed when taking zonal averages. It would be interesting to compare the effects of TOZ and AOD on UVI of this study with those reported in Bais et al., 2015.*

A discussion has been added on this subject.

20. *P13, L4: As mentioned above AOD exhibits large spatial variability, therefore the results found for zonally averaged UVI changes should not be generalized for all latitudes. For example, AOD over China will decrease substantially by 2100 but at similar latitudes over the Pacific the effects are almost negligible.*

We added a sentence at the beginning of the analysis emphasizing that the results are in zonal average and should not be generalized. Along with the discussion on Bais et al (2015) results and ours, concern over this point was also expressed.

21. *P13, L24: Please discuss to which level of accuracy the UVI can be reproduced.*

This was discussed P7, L24,28. Specific values from Brogniez et al. (2016) has been added in P7 and P13.

22. *P14, L1: State here that UVI projections are for cloud-free skies. Cloud effects can alter significantly the predicted changes at high latitudes.*

Corrected.

23. *P14, L6: State the period for the increase.*

Corrected.

3 Technical comments

24. *P4, L14: Replace "increment" with "increase"*

Corrected.

25. *P6, L26-27: Replace to: ". . . with each other, we defined two experiments from two sets of models. These are summarized . . ."*

Corrected.

26. *P8, L11: I suggest replacing "wind variability" with "stratospheric circulation"*

Corrected.