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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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This paper addresses projections of UV radiation in the 21st century based on projections from Chemistry Climate Models participating in CCMI. The innovation of this study, comparted to previous studies, is the use a new set of projections for ozone and other UV affecting variables.

One important aspect of the study is the investigation of the effects on UV projections under different RCPs.

Overall is an important contribution to quantifying the effects of future atmospheric changes on UV radiation at the surface. However, there are several aspects that need attention and should be clarified before the paper is accepted for publication. Furthermore, the manuscript needs a through language checking.

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General comments:

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.

Specific comments:

P3, L21: Also in Bais et al. 2015

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.

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

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.

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.

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.

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

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

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

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.

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?

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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?

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.

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 differences 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 11%) are not discussed at all 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).

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

P8, L31-34: The results presented in Figure 3 are yearly averaged. However, the variability 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.

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 parameters; the ozone and temperature profile, the surface reflectivity, the optical depth of

aerosols?

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.

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.

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.

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

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

P14, L6: State the period for the increase.

Technical comments:

P4, L14: Replace “increment” with “increase”

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

P8, L11: I suggest replacing “wind variability” with “stratospheric circulation”

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