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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Clear sky UV simulations in the 21st century based on ozone and temperature projections from Chemistry-Climate Models" *by* K. Tourpali et al.

Anonymous Referee #1

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The manuscript presents a prediction of cloudless sky erythemal irradiance, based on a set of recent total ozone predictions by the leading CCMs. As such it represents the best knowledge about UV levels in the 21st century available to date. UV radiation is relevant for a number of issues including its harmful effects on the biosphere which was one of the main reasons for initiating the whole ozone research. For these reasons, the publication is relevant for the scientific community. The manuscript is well-written and nicely presented and I suggest publication after consideration of the points below.

Specific points:

Title: I suggest "UV simulations for the 21st century" instead of "in the 21st century"

(although the simulations were also done in the 21st century, but this is probably not what the authors meant)

page 13046, line 20: "no usable information on the future levels of the other UV influencing factors is available" is a very strong statement. Clouds and surface albedo are among the most relevant climate feedbacks and any climate prediction neglecting those would be invalid. The IPCC models therefore certainly include predictions of these variables but the authors are right that the magnitude and, in the case of clouds, even the sign of the change is unclear. As the current formulation might suggest that clouds are not treated at all by GCMs I suggest to change the formulation. Clouds are properly treated, but their changes are still uncertain.

page 13047, line 18: What was the motivation for using zonal and monthly means of ozone instead of calculating erythemal irradiance for each model time step and grid box and averaging afterwards? This would of course be computationally more expensive but the relationship between total ozone and erythemal irradiance is non-linear and the procedure followed in this manuscript causes extra uncertainty. Can the authors give an estimate of this uncertainty? Also, as the relationship roughly follows an exponential law ($erythemal\,irradiance \propto exp(-A \cdot ozone\,column)$) one could think of averaging $exp(-A \cdot ozone\,column)$ instead of the ozone column. The latter is probably not feasibly at this stage, but an uncertainty estimate would be appropriate.

page 13048, line 4: Here it is argued that simulations including a standard aerosol are more realistic than simulations without aerosol. Wouldn't the same also be true for clouds? One could use "standard clouds" (e.g. annually averaged zonal mean cloud cover from one of the models including clouds). My feeling is that the results of this manuscript wouldn't change much, but the same is true for the aerosol.

page 13048, line 22: Why not use the same reference period (2000-2005) for all models?

page 13051, line 8: Does this paragraph imply that the changes in the temperature

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Interactive Comment

profile in the 21st century have negligible effect on the erythemal irradiance, compared to the effect of total ozone changes? Please formulate more clearly.

page 13051, line 22: According to (IPCC, 2007), magnitude and sign of the cloud feedback is still unclear. Doesn't that imply that changes in the UV irradiance could be positive or negative? Here the authors state that they expect an increase in erythemal irradiance due to a decrease in cloudiness, in contrast to their statement on page 13046, line 20 which basically says that nothing is known about changes in clouds.

page 13052, line 1: Yes, it is true that an increase in surface albedo would increase surface irradiance. But the increased UV irradiance due to increased surface albedo is not necessarily relevant e.g. for vegetation as it never reaches the ground but is reflected away by the snow: Although snow always leads to an increase of irradiance above the snow, the irradiance below the snow is always decreased. Therefore an increase due to albedo is not necessarily comparable to an increase due to ozone depletion. Maybe the authors could add a statement.

Figure 1: Changes in erythemal irradiance in this study are due to changes in total ozone only. I would therefore strongly suggest to show additionally at least one plot of total ozone. Probably the same curves as in Figure 1, but with ozone instead of erythemal irradiance to give the user a feeling for the percentage difference in total ozone causing the differences in the current Figure 1.

Technical points: page 13047, line 9: Please move the reference to table 1 one sentence upward, before you mention AMTRAC and E39C

page 13047, line 16: typo, "reslution"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13043, 2008.