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Comment

Interactive comment on “Projections of UV

radiation changes in the 21st century: impact of ozone recovery and cloud effects” by A. F. Bais et al.

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Reply to the comments of the reviewers of the paper “Projections of UV radiation changes in the 21st century: Impact of ozone recovery and cloud effects”

Anonymous Referee #1

We would like to thank the reviewer for his/her constructive comments that helped in the improvement of the manuscript. In the following we provide our response to the comments. Each of the reviewer’s comments is repeated and our response follows.

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1) Page 10771, lines 11-15. It is stated that at high latitudes the UV-Ery will decrease by 12% (line12). In line 15 it is stated that another 2-3% reduction is due to cloud effects. Does this imply that cloud effects are not included in the 12%? Should 2-3% percent be added to the 12% to get both ozone and cloud effects? Please clarify.

The 2-3% reduction is already included in the 12%. This point is clarified in the revised version of the abstract.

2) Page 10772, lines 23-25: "They examined the changes between 1965 and 2095 and therefore they isolated the effects of climate change from the effects of ozone depletion/recovery." How was ozone and cloud effects isolated by studying the changes between 1965 and 2095?

The assumptions made in the paper of Hegglin and Shepherd (2008) is that in 1965 ozone depletion had not started yet and by 2095 ozone recovery from ODSs is projected to have been already completed. Therefore any changes in the atmosphere would be due to climate change only. These assumptions are supported by model simulations and for the start of ozone depletion also by observations (see WMO Ozone Assessment 2011). Ozone recovery from ODSs has been mentioned in the revised paper to clarify further this statement.

3) Please include a list describing all acronyms used in the paper.

In the paper we use only 7 abbreviations (excluding the model names) which are too few to justify a separate table, particularly as some of them are very common in the literature (e.g. CCM, GHG, UV)

4) Page 10773: Please include a list of all CCMs used in the paper. Include references to the models, resolution, whether they could calculate CMFs, their contributions/use in the paper, etc.

This was indeed an omission. A relevant table is provided now.

5) Page 10774: Mean optical properties are used to calculate the changes in surface

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UV-radiation. Usually the mean of the radiation for a time period is different from the radiation calculated using mean values for the same time period as inputs. For example, the mean radiation for a day is different from the radiation obtained using the mean solar zenith angle (or mean of the cosine of the solar zenith angle). The authors have to show that their approach using means as input is valid. Monthly averaged ozone and cloud fields are input to the radiative transfer model to calculate the UV-Ery changes for local noon. How are these changes affected if daily ozone and cloud fields are used instead? What happens with an even better temporal resolution?

We agree that the mean monthly irradiance from daily values should be different from the irradiance calculated on the 15th of each month, because the day-to-day changes in ozone and noon solar zenith angle. However, we believe that such an approach would change only the variance and would not affect the calculated changes in time. Daily simulations for 14 models and 140 years would have been very difficult because this approach would increase the computational time by a factor of 30. Already the monthly simulations took more than 6 months to complete. Nevertheless, to satisfy the reviewer's concern, we have performed daily simulations for selected months and we compared the average UV-Ery calculated from daily values with the UV-Ery calculated for the 15th of each month based on mean monthly ozone. Although there are differences in the absolute values and in the month-to-month variability, in the long term there is no significant trend. In this study we address only relative changes on decadal time scales, therefore only differences in the trend would matter. We have checked for example, December at 54° S and October at 74°S and the trend of the relative differences between the two approaches is practically zero (2e-5 %/year and 0.008 %/year respectively). We expect that at lower latitudes and other months the effect would be even smaller as the day-to-day variability of ozone is reduced. Therefore it is very unlikely that the results presented in the paper would be different if we had used daily values instead of monthly means. The following text was added in section 2.2: "The calculation of local noon UV-Ery on the 15th of each month instead of deriving monthly mean UV-Ery from daily values, which was avoided due to limitations in computational

time, affects only the level of variability of UV-Ery but has practically no effect on its long term changes.”

6) Page 10775, lines12-14: It is stated that "However, the CMF calculations depend less on the accuracy of the radiation schemes employed in the CCMs, and more on how clouds and their effects on radiation are represented in each model." May you please elaborate on this?

The following text was added: “This is because the CMF reflects the impact of the clouds on surface irradiance under clear skies, irrespective of the absolute values of the calculated irradiance. Any deviations induced from the radiation schemes employed would be the same in clear-sky and all-sky simulations, hence they would cancel out.”

7) Page 10775: For the 5 CCMs that calculates CMFs: Please describe the cloud models used in the various models (maybe a Table?). What are the similarities and differences between the cloud models? How are the clouds represented in the CCMs?

We believe that in the frame of this study we cannot expand to discussing the radiation and cloud models used in the CCMs, as this topic is relevant to the development of the models and it has been discussed already in other publications (e.g., Morgenstern et al, 2010 and Chapter 3 of the SPARC CCMVal report, 2010). A relevant sentence has been added in the text with the corresponding references. Certainly clouds are a source of significant uncertainty, but currently these simulations are probably the best available information. This was already mentioned in the paper in section 2.1 paragraph 4; however, we expanded the discussion in this paragraph to make it clearer. In any case, it is not possible to perform any sensitivity tests in the frame of this study for the estimation of uncertainties due to the used cloud or radiation schemes.

8) Fig. 2. To strengthen the validity of the model simulations in Fig. 2 it would be beneficial to include similar "measured" numbers estimated from satellite data from the late seventies until present (TOMS etc.). If the present model runs are correct, the peak in the change should be present in the satellite data as well.

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In fact the satellite derived erythemal irradiance is also an RTM calculation based on measured ozone and reflectivity. These estimates were repeatedly validated against ground based measurements and these validations revealed large deviations, induced mainly by aerosol and cloud effects that cannot be accurately taken into account. A newer calculation of erythemal irradiance from all available ozone data including aerosol and reflectivity changes has been presented by Herman (2009) but this data set is not available to use in this paper. Finally, the anomalies shown in Fig 2 are relative to the average in the years 1975-1985, so for the first half of this period no satellite data are available. An indication of the agreement between our simulations and the old TOMS-derived UV data set is shown in the executive summary of the recent WMO Ozone Assessment (2011).

9) Is altitude accounted for in the RT simulations? If so, which topography map was used? If not, what is the effect of neglecting altitude?

We would like to thank the reviewer for this comment. Yes, the mean altitude for each grid derived from GTOPO30 was taken into account in the calculations. We are sorry that we missed to include this information in the paper. We have added the following paragraph: “For each 5°x5° grid, the mean surface elevation was used in the RT calculations derived from the global digital elevation model GTOPO30 (<http://www1.gsi.go.jp/geowww/globalmap-gsi/gtopo30/README.html#h12>).”

Technical comments:

Page 10793, table 2: What is the meaning of (2035), second row, third column?

*At this belt there an ambiguity in the exact determination of the return year. The year in parentheses may be valid as well. A second footnote was added in Table 2.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C5706/2011/acpd-11-C5706-2011-supplement.pdf>

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