

Interactive
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 #2

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 followed by our response.

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1. In most sections this is a well written paper, in which the methods and results are well described. However, some improvements are necessary prior to publication:

2. The authors have an implicit assumption, which may not be justified. The difficulty starts with the first sentence of the abstract. They do not derive the irradiance, but an average (the monthly averaged irradiance at noon? or the monthly irradiation?). Unfortunately it does not become clear which quantity is provided.

The abstract has been revised to clarify which quantity is used. What we calculate is the erythemal irradiance at local noon for the middle (15th) of each month. We believe that this is clearly documented in section 2.1, but it is true that it was unclear in the abstract.

Comment: While the term may be easily corrected the biological implication cannot be fixed easily. There are biological effects on much smaller time scales which cannot be addressed by the method described here. Even erythema does not develop on a monthly basis; the same is valid for other biological effects.

In the paper we do not address any biological effects on any time scale, and we do not claim that these projections can be used to assess short scale biological effects. We simply provide estimates of the likely changes in erythemal irradiance in the long term, which biologists may interpret according to their needs.

Comment: The conditions for the monthly doses need to be clearly addressed, both with respect to ozone changes and changes in cloudiness (a request also made by referee #1).

We have not calculated doses, but dose rates at local noon, based on monthly ozone projections. The use of monthly averaged ozone does not introduce significant effects on the trend of the derived erythemal irradiance. It does change the absolute level and monthly variability of erythemal irradiance which has no effect on the derived changes. Please see our response to the relevant comment of the first Reviewer and the relevant

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text added in section 2.2.

Comment: In addition the CMF factors are no longer valid on shorter time scales. Even the concept of reduction of irradiation by clouds is questionable if shorter time scales (minutes to full days) are considered.

The CMF concept is a statistical approach that is used to simulate the relative changes in irradiance from clouds. Estimates of higher time resolution were not generally available, although some CCMs provide daily or even hourly estimates of surface irradiance.

Comment: The limitations must be better described and the uncertainties need to be discussed.

Section 2.2 has been revised to address the above issues.

3. Page 10775, line 1: please provide references and some quantification

The following text was added: “The attenuation of total (or shortwave) irradiance by clouds is weaker (about four times less) than of UV-B irradiance”, along with 2 references to Seckmeyer et al., 1996 and Blumthaler et al., 1994.

4. Page 10777, line 23: the uncertainties given here are by far too optimistic and are in conflict with published literature. See for example [Weihs and Webb, 1997] or [Cordero et.al. 2007].

The uncertainties given here (2-5%) refer to the radiative transfer model calculations (solving of the radiative transfer equation) for given input parameters and there is enough evidence to support this. We agree, and we state this, that large part of the uncertainty is due to the input parameters that are usually poorly known. We have added the two suggested references to support this statement.

5. Page 10778, line 3: the estimation does not seem to be justified, even not for changes. The uncertainties may be much larger, both due to the above mentioned monthly averages and the uncertainties of the input parameters

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The quoted “few percent” uncertainty is a qualitative statement because it is very difficult to estimate exactly the effects that changes in surface albedo and aerosols will have on surface UV in the future. So far, long term changes in aerosols (2-3 decades long) have not produced changes in UV of more than 10% over most areas on earth. A similar situation we believe has occurred with the changes in albedo, excluding of course areas where the albedo has changed dramatically. We have added two supporting references (Arola et al., 2003 and Bernhard et al., 2007). Finally, this statement is rather irrelevant of whether we use monthly averages of irradiance or with the uncertainty in the input parameters.

6. Page 10784, lines 6-16: The findings are not new, but have been described in the literature before, starting in the WMO ozone assessment in 2002. It may be added that the albedo changes occur already before ice melting, e.g. [Meinander et al., 2008] [Wuttke et al., 2006]

These are not new findings but just a discussion of indirect climate effects on UV. The 2 suggested references were added.

7. Page 10784, line 29: this conclusion is not new, but is already contained in the WMO assessments from 2002 and 2006. It should be stated that the calculation reconfirm earlier findings.

As there is not such line number in the document, we assume that the reviewer refers to UV calculations presented here. If fact none of the previous ozone assessments 2002 and 2006, presented UV projections including cloud effects. In 2006 assessment there was one simulation from one CCM based only on projected ozone. Therefore we do not agree that this work reconfirms earlier studies.

8. More information should be given to the spread in model results provided in figure 2. Is the grey area one sigma? Does it include all minima and maxima? How much is the grey area affected by the smoothing? At least one more figure should be provided discussing the spread of the model results.

As stated in the caption of Figure2, the grey area represents the range of the individual model variations. Hence for each year it is defined from the maximum and minimum deviations from all models. It is not a one sigma envelope. The 1:2:1 filter affects only marginally the grey area and it is mostly applied to make the figure look better. The spread in the model results arises from the ozone projections from the different CCMs and is discussed in detail in other publications (e.g., WMO ozone assessment 2011, Eyring et al 2010, and others that are included in the reference list of the paper) Therefore we believe that such discussion is out of the scope of the current paper.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C5716/2011/acpd-11-C5716-2011-supplement.pdf>

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