

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.

Anonymous Referee #2

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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. 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 de-

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scribed here. Even erythema does not develop on a monthly basis; the same is valid for other biological effects. 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). In addition the CMF factors are no longer valid on shorter time scales.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 limitations must be better described and the uncertainties need to be discussed. 3. Page 10775, line 1: please provide references and some quantification 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]. 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 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] 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. 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.

Weihs, P., and A. R. Webb (1997), Accuracy of spectral UV model calculations 2. comparison of UV calculations with measurements, *Journal of Geophysical Research*, 102(D1), 1551-1560. Meinander, O., A. Kontu, K. Lakkala, A. Heikkila, L. Ylianttila, and M. Toikka (2008), Diurnal variations in the UV albedo of arctic snow, *Atmos. Chem. Phys.*, 8, 6309–6323, 2008, 8, 6309–6323. Cordero R.R. , Seckmeyer G., Pissulla D., DaSilva L.: Uncertainty evaluation of the spectral UV irradiance evaluated by using the UVSPEC Radiative Transfer Model, *Optic Communications*,

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