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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ultraviolet actinic flux in clear and cloudy atmospheres: model calculations and aircraft-based measurements" *by* G. G. Palancar et al.

Anonymous Referee #3

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This paper discusses the comparisons of modeled actinic fluxes to measurements performed with an aircraft in a large range of conditions. Thematically the paper is appropriate for ACP but there are issues to be delineated before it is accepted for publication. The paper contains interesting results, but some questions arise concerning the use of the model. In the model, aerosol effects have not been taken into consideration and I believe this is a handicap of the paper. There is also some concern about how the authors interpret the statistical results of the comparisons. The abstract is rather general and I would suggest to specify more quantitatively the findings instead of giving statements like the one in lines 9-11. The simple conceptual model is not included in the objectives of the paper as they are given in the introduction. Consequently sections

4.2 and 4.3 come unexpectedly in the scene. I found useful the approach presented in Figures 6 and 7.

Comments:

3322, 9-10: The numbers give here should be adjusted to reflect my comments at (3329, 8-18). The same stands for the statement "good agreement".

3327, 4: What parameters contain the aircraft data files that were used as inputs to the model?

3327, 7-8: Is this statement correct given that no aerosols were taken into account in the model?

3327, 14: Excluding of aerosols is not a good choice. At least typical conditions should be included.

3327, 17: I suggest to give a brief description of the land over the area covered by the measurements, to justify the selection of the albedo values.

3328, 5-6: Why the authors have chosen to degrade the spectral measurements to such broad integrals? The contribution of the UVB is now almost negligible; hence the analysis reflects effects mainly on UVA flux.

3328, 18-19: Good agreement is a very vague statement particularly as differences of larger than 20% are seen in the comparisons. Generally the 2 examples shown do not represent the entire campaign, as Table 1 suggests.

3328, 21: Table 1 suggests that the upwelling flux is underestimated on average by 10% and the spread is +/- 50%, which is not good agreement

3329, 5: Isn't the presence of clouds confirmed by the cameras?

3329, 8-18: I do not agree with the interpretation of the statistics of Table 1. For each flight there is a large spread in the data ranging between +/- 7% to +/- 40%. This implies

that the agreement between model and measurements is generally not very good. The statistics for the entire data set is based on the average rations from all days hence the calculated range is small. This shows some consistency in the results between different days, but tells nothing about the agreement between model and measurements.

3329, 19: It would be better to show in Figure 3 the fraction of measurements (in %) instead the number of data. This would clearly show that the comparisons under cloud free conditions are much better.

3329 8-29: Why downwelling fluxes agree better than upwelling even under cloud free conditions? Could it be the selected constant albedo or boundary layer aerosols which are not taken into account?

3330, 5: "...values that approach the experimental and modeling uncertainties." This is probably true for down welling flux but for upwelling the expected experimental or modeling uncertainties are certainly lower than 20 or 40%.

3330, 7-10: In the absence of clouds there should be no doubt that aerosols, which were not considered in the model, reduce the observed flux. I think this clearly suggests that choosing to use zero aerosols in the model was a wrong decision.

3330, 11-16: The small SZA dependence might be also a consequence of the imperfect angular response of the detectors. For upwelling flux the picture is indeed too complex to make accurate attributions. I would add again here the effect from excluding aerosols in the model. It would be interesting to perform some sample runs with typical aerosols in the model and see if the comparison improves (not only with respect to SZA, but generally for upwelling flux).

3332, 19: I would remove the word "collimated". It does not help and, in any case, the direct solar radiation is not parallel.

3333, 1: I do not think that Figure 8 helps, particularly as the right panel is a bit confusing with the upward reflection of clouds to the aircraft. I suggest to remove it.

3332, 17: This section presents an interesting simplification for actinic flux aloft. In the first place it comes unexpectedly in the paper which to this point is focused on the interpretation of the model to measurement comparisons. I am puzzled as to whether this discussion is appropriate for this paper. At least I would suggest to the authors to try to tight it better with the rest of the paper.

3339, 2: See comment at (3329, 8-18).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3321, 2011.